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THE

SCIENTIFIC PAPERS
OF BERTRAM HOPKINSON

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B. Hopkins

THE SCIENTIFIC PAPERS OF BERTRAM HOPKINSON

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1921

PREFATORY NOTE

WHILE the sense of loss to the University, arising from the death of Prof. Hopkinson in national war service, was still recent, it occurred to his scientific friends, including the staff of the department over which he presided at Cambridge, that no memorial could be more suitable or permanent than a collected edition of his writings on mathematical and engineering science. When the project was brought to the notice of the Syndics of the University Press, they promptly concurred and undertook to be responsible for the cost of publication.

The volume that has been the result includes probably all his writings that are of permanent importance, the omissions being either publications of transient interest or others adequately represented by papers included in the collection. The separately published tractate on the *Theory of Vibrations of Systems having One Degree of Freedom* (Cambridge University Press, 1910), which forms the introductory volume of a series of Cambridge Tracts in Engineering, has not been reprinted.

Acknowledgment for assistance and advice is due to the authors of joint papers, especially to Sir Dugald Clerk, F.R.S. and Sir Robert Hadfield, Bart., F.R.S.; and very especially to Mr J. W. Landon, Fellow of Clare College and University Lecturer in Mechanical Engineering, who undertook the whole of the correction of the proofs and contributed substantially to the completeness of the record.

Two biographical notices have been prefixed: one contributed by Sir Alfred Ewing to the *Proceedings of the Royal Society*, and the other an appreciation published in the *Alpine Journal* by Mr A. V. Hill, F.R.S., Fellow of King's College, one of his colleagues in the Air Service, now Professor of Physiology in the University of Manchester.

For permission to republish from their collections acknowledgment is made to the Royal Society, the Institution of Civil Engineers, the Institution of Mechanical Engineers, the Institution of Electrical Engineers, the Iron and Steel Institute, the Institution of Naval Architects, the Royal Institution, the North-East Coast Institution of Engineers and Shipbuilders, the Sheffield Society of Engineers and Metallurgists, the London Mathematical Society, and to the Editors of *The Philosophical Magazine*, of *Engineering*, and of *The Electrician*.

The Editors desire to record their obligation to the officials of the Cambridge University Press for unfailing courtesy and assistance.

CAMBRIDGE, *November* 1920.

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BERTRAM HOPKINSON

[From the "PROCEEDINGS OF THE ROYAL SOCIETY," A, VOL. XCV.]

BERTRAM HOPKINSON, who was killed in a flying accident on August 26, 1918, while engaged on national service, was born at Woodlea, Birmingham, on January 11, 1874. He was the eldest child of Dr John Hopkinson, F.R.S., born when his father, then only 25 years old, was at the threshold of a career which was to achieve great things in science. At that time John Hopkinson, but lately Senior Wrangler and Smith's Prizeman, had accepted a responsible position with Messrs Chance Brothers, and was applying his scientific skill to the task of improving the quality of their optical glass and developing its uses, especially for lighthouse illumination. The position enabled him to marry: his wife was Evelyn Oldenbourg, a lady of remarkable gifts, to whom he had become engaged almost immediately after he left Cambridge. Bertram was the eldest of a family of six, three boys and three girls. When he was three years old the family moved to London, where John Hopkinson took up professional life as a consulting engineer and inventor. This he pursued with conspicuous success until his premature death by an Alpine accident in 1898.

Bertram owed to his parents far more than a heredity which was at once sound and of brilliant promise. The home atmosphere could not have been more congenial or more stimulating for a boy born with scientific tastes and richly endowed with mental and physical activity.

John Hopkinson's absorption in invention and discovery never stood in the way of companionship with his children. He was in a rare degree his children's friend, and to Bertram especially this friendship was in itself a liberal education. The boy was no doubt extraordinarily receptive. From early childhood he was the almost constant associate of his father, sometimes assisting him in work, often his comrade in play. He lived at home, going as a day-boy to St Paul's School, and enjoying intensely his father's company in the hours spent out of school, joining in long walks, in discussions, in experiments. Always strong in body and mind, he could profit by such a life without being over-strained. He prospered at school, was the youngest boy in his mathematical form, and secured a major scholarship at Trinity before he was seventeen. By that time he had imbibed not only much science and engineering, but the habit of scientific thinking and of applying scientific thought to real problems. In the researches he was to publish later, one sees at every turn the reflection of his father's mind,

From his father, too, and scarcely less from his mother, he acquired a wholesome love (which he never lost) for most kinds of open-air activity. Walking, climbing, rowing, sailing, ski-ing, chamois-hunting in the Tyrol, in such things he spent his holidays with the keen pleasure of one who was immensely alive in everything he undertook.

At Cambridge he read for the Mathematical Tripos, which by that time was divided into a first and second part. In the first part he was compelled to content himself with an aegrotat degree. An unlucky attack of illness—a rare event with one so robust—prevented his taking the examination; but next year, in the second part, his quality was shown by his being placed in the First Division of the First Class. For the rest his University life was normal and uneventful. He made some lasting friendships, and was one of a Trinity crew at Henley. Of his work for the Tripos Sir Joseph Larmor remarks: "Those who had to do with him in that connection may remember a certain modest and deferential robustness and independence: he had obviously the gift of friendship for his peers: and it could not be said that he sank the manifold interests of University life in one intellectual ambition."

On leaving the University at the age of 22 he proceeded to read for the Bar, a profession with which his father was connected by often serving as an expert in patent actions, and in which Bertram could count upon useful introductions. After working in counsel's chambers, where he was soon recognised as a pupil of distinction, he was called to the Bar in 1897. Next year he was on his way to Australia to carry out a legal enquiry, when the tragic death of his father, his brother, and two sisters changed the course of his life. The family were spending their holiday that autumn in Switzerland, at Arolla, where Bertram joined them for a few days before taking ship to Australia. The days were spent in climbing expeditions, which included a traverse of the Matterhorn, after which Bertram went off to pursue his journey, and his father returned to Arolla. A few days later, John Hopkinson, with three of his children, was killed in climbing the Petite Dent de Veisivi. A telegram recalling Bertram reached him at Aden.

Faced with this calamity, Hopkinson decided to take up, so far as he could, his father's unfinished work. He joined in partnership with his uncle, Mr Charles Hopkinson, and his father's valued assistant, Mr Talbot, and with them was responsible for the design of electric tramways at Crewe, Newcastle, and Leeds, as well as other works. To do this required courage, not to say daring: in that he was never wanting. It required, too, a rare adaptability, and perhaps was possible only because of the irregular and almost unconscious training in engineering which he had received from his father. An important element in his life at the time was his intercourse with Sir Benjamin Baker, who had been for many years a close friend of the

family. Sir Benjamin appreciated Bertram, enjoyed scientific discussion with him, and remained, until his death in 1907, an intimate and most helpful friend.

Hopkinson not only succeeded in the practical aspect of his new undertaking, but soon found in it material for scientific research. A paper by the three partners on "Electric Tramways," read before the Institution of Civil Engineers in 1902, gives particulars of his experiments on the electrolysis of pipes and on other matters arising out of their engineering work. For this he was awarded a Watt Gold Medal by the Institution, of which later he became a full member.

In 1903 the chair of Mechanism and Applied Mechanics in the University of Cambridge became vacant. Hopkinson was then only 29 years of age, and so far as teaching was concerned he was wholly untried. But already he had a considerable professional reputation, and he gave a personal impression of brilliancy and promise which satisfied the electors. In electing him they certainly made a wise choice, and were entirely justified in the result. The professorship meant his assuming charge of the Cambridge Engineering School, which by that time had become an active and considerable section of the University. Its expansion had been rapid from about 1892, when the Mechanical Sciences Tripos was established. Successive extensions of the buildings had been made which barely kept pace with its growth in numbers. One of these extensions was the Hopkinson Wing, erected in memory of John Hopkinson and his son John by the surviving members of the family. There was a peculiar appropriateness in Bertram's appointment to be head of an establishment in whose creation his father had from the first taken a helpful interest, and whose development was in this way associated with his father's name. Under Bertram's leadership the prosperity of the Engineering School was more than maintained. It continued to grow in numbers and in academic and professional repute. Its attractiveness to students was in some sense a danger, but he was careful to fence the approach to the Tripos so effectively as to maintain a high standard. His own passion for research found in Cambridge a wider scope than had been open to it in the earlier part of his career. He entered there upon a period of remarkable productivity, stimulating selected students to attempt original work, and securing their co-operation in many important researches. He became a Fellow of the Royal Society in 1910, and was serving on the Council at the time of his death.

His marriage, in 1903, almost coincided with the beginning of his professoriate. His wife, who was the eldest daughter of Mr Alexander Siemens, survives him with seven daughters. In domestic life he found continuous quiet happiness, as well as freedom to pursue his scientific interests. There was no son who might have had from Bertram the same

kind of nursing in scientific method that Bertram had from John. But if he had no son he had among his students not a few disciples who were fired by his teaching and example and were full of affection for their chief. Some of them will carry on the work he left undone. Others gave their lives, as he did, in the war. Of that band was his own brother Cecil, a man of rare promise, who passed out with First Class Honours in the Mechanical Sciences Tripos only a year before the war began. He had been his brother's assistant in more than one research. Cecil Hopkinson hastened to join the army in August, 1914, and received a fatal wound in Flanders near the end of the following year. Those who knew Cecil and his great potentialities felt the eclipse of that young life to be far more than a personal loss. To Bertram it was a grievous blow; but he did not let it disturb his absorption in war work, which by that time was complete.

During his tenure of the Cambridge professorship, Hopkinson took no large part in the administrative work of the University outside of his own department, the claims of which were sufficiently exacting. He was, however, an energetic promoter of the Officers' Training Corps, and organised in it an engineering section, thereby maintaining a family tradition; for his father had been a pioneer in the foundation of the Corps of Electrical Engineers. In 1914 he accepted a professorial fellowship offered him by King's College. Outside the University he took a leading part in the work of more than one Research Committee. He served jointly with Sir Dugald Clerk as Secretary of the British Association Committee on Gaseous Explosions, whose Reports, spread over several years, contain records of many of his own experiments. He was an original member of the Advisory Committee set up in connection with the establishment of the Department of Scientific and Industrial Research. When the Royal Society appointed a committee of engineering experts to advise on problems of the war, Hopkinson became its secretary. The indirect effect of this was specially important; it brought him into close relations with the technical experts of the Army and Navy, and so did much to determine the direction which his energies took in the final years when his undivided efforts were given to national service.

On the outbreak of the war he dropped all other interests. Obtaining a commission in the Royal Engineers he first undertook teaching duty at Chatham to relieve others for active service. Later he was engaged at the Admiralty in a department organised by the present writer, on work of a kind entirely new to him, which he took up with conspicuously good effect. It was mainly concerned with the collection of intelligence; but independently of that, he was at the same time occupied in conducting experiments in connection with an arrangement for the protection of warships from the effects of mines and torpedoes, by the addition to the hull of a "blister" or outer shell. His work showed, by experiments of gradually

increasing scale, which were most ably carried out, that the law of comparison held good, subject to a certain modification in going to full size, and he suggested the insertion in the "blister" of a structure capable of absorbing the energy of an explosion in the act of becoming deformed, in lieu of a water-jacket. The value of his contribution was quickly recognised by the Admiralty; it at once received official acceptance and substantial acknowledgment. It has been applied in the design of some of the newest units of the Fleet, and is a feature of one of the most powerful of them, the battle-cruiser "Hood." The origin of the suggestion is of interest, for it arose out of an earlier scientific research which Hopkinson had carried out with no idea at the time of putting it to this important use. For years before the war he had made a special study of explosions, their nature and the measurement of their effects. The events of 1914 gave this kind of knowledge a new significance. He applied himself to problems of attack as well as of defence, taking up not only the protection of ships, but the design of bombs for use by air-craft. From that he passed to the equipment of aircraft generally. He accepted a position under the Air Board, and in each successive transformation of that branch of the Service the authority and responsibility of his office seemed to be enlarged. Some months before his death he had become a colonel and was awarded the C.M.G.

Much of his work was done at an experimental station on the East Coast, but he had to visit many aerodromes, and he found that flying was his best means of locomotion. It was, moreover, an art he had to acquire in the interests of the work itself. He soon learnt to be his own pilot, and generally flew alone. It was in one of these flights that he fell, near London, in bad weather on August 26, 1918, and was instantly killed. The Air Council took the unusual step of recording "their deep sense of the high and permanent value of the work done for the Flying Forces by the late Colonel Hopkinson, and their recognition of the private self-abnegation with which he devoted his great abilities and scientific attainments to the public service"; and they communicated to the Vice-Chancellor of the University "an expression of profound regret at his untimely death and at the loss which has thereby fallen on the University of Cambridge."

Of Hopkinson's work for the Air Force, the following notes have been furnished by one who served under him in it, Lieut.-Col. H. T. Tizard:

"His work in connection with flying began about March, 1915, when he was asked by the Department of Military Aeronautics to conduct experiments on bombs. These were carried out at Cambridge and elsewhere, and included the building of a model factory on a one-sixth linear scale on which the effect of model bombs was tested with the object of determining (*a*) the best proportion of bomb-case to weight of explosive, and (*b*) the best material of which to make the case. The experiments were continued until the end of May, 1915, when the model building was completed and the trials

were witnessed by the Ordnance Board and representatives of other Government departments. While preparations for these trials were proceeding, he was consulted by the Superintendent of the Royal Aircraft Establishment in connection with the design of bomb-sights. In July, 1915, he was appointed on the panel of Lord Fisher's Board of Invention and Research, and during the following months until November, his time was chiefly occupied in work for that Board, and in other experiments in connection with bombs and gyro bomb-sights both at Cambridge and at Farnborough.

In November an official connection with the Royal Flying Corps was established by his appointment to the Department of Military Aeronautics, where he took charge of both the design and supply of bombs, bomb gears, guns, and ammunition. Most of the experimental work for the corps was at this time carried out either at manufacturers' works or at the Central Flying School, Upavon, and at Farnborough and Hythe. It was not long before Prof. Hopkinson saw the drawbacks of combining experimental and training work at one station, and he urged the formation of a separate station for the former. This recommendation was adopted, and an armament experimental station was started at Orfordness in the spring of 1916. The work of this station was entirely under the control of Prof. Hopkinson, and he threw his whole energies into its development. Most of the personnel was selected by him from existing R.F.C. stations. By the middle of 1916, in spite of the difficulties of working in temporary buildings, the station was in full swing. Shortly afterwards a large amount of the supply work, which had hitherto been done by Prof. Hopkinson's section at headquarters, was transferred to a special supply section, which left him more time to devote to purely experimental work. The work at Orfordness had great influence on the development of armament in the R.F.C. and subsequently in the Air Force, and was very varied in character. It included the development of bombs, bomb-sights and methods of bombing; guns, gun-sights, and ammunition; self-sealing tanks and other accessories; and not least the systematic development of night flying, and of navigation in clouds and in bad weather, the influence of which work was beginning to be felt strongly at the time of his death, and will increase as time goes on. In all this work Major Hopkinson was at his best. He possessed the great capacity of understanding human limitations and knowing where, for war purposes, it was uneconomical to proceed further with the development of any particular scientific work. His judgment was well shown in the great pains he took to keep training and experiment in the closest possible contact.

The success of the work at Orfordness was such that it was soon decided to put the testing of aeroplanes, which up to this time had been done at the Central Flying School, also under his direction. Towards the

end of 1916 this work was removed to Martlesham Heath. Major Hopkinson selected the site and made all preliminary preparations for clearing it and putting buildings in hand. The station soon developed in importance, and the work, which consisted of the testing of complete aeroplanes, of engines and other accessories as well as a certain amount of experimental photography, expanded considerably under his direction.

At the end of 1917 Lord Rothermere's reorganisation of the Air Force still further increased the scope of Major Hopkinson's duties. The experimental work at the R.A.E. and the Naval Aircraft Experimental Stations at Grain, came under his control, and from this time until his death his influence on the general development of aeronautics steadily increased. He was appointed Deputy Controller of the Technical Department in June, 1918.

He found time, in spite of the pressure of his work, to learn to fly at Orfordness. There can be no doubt of the wisdom of this step, although many of his friends thought that it was an unnecessary risk. It increased both his judgment and his influence, especially his influence over the officers at the experimental stations. He took an intense pleasure in flying. He learned remarkably quickly for a man of his age, and was soon at home on many types of machines. At the time of his death he was flying a Bristol Fighter, and had started from Martlesham Heath on his way to London. The weather was threatening at the start, although the sky was only partly clouded, but in the neighbourhood of London the conditions were much worse and the sky was completely covered with low clouds. It seems clear that he flew above the clouds for some way, and then finding no gap descended through them. He probably lost control of the machine in the clouds (which would be quite easy for even a very experienced pilot to do on the type of machine he was flying), and on going through the clouds did not have sufficient time to regain control before the machine crashed, and he was instantaneously killed."

In this connection another friend (Sir R. Threlfall) writes:

"Hopkinson's services to the country are not to be associated with any particular invention. They lay in his contribution to the building up of the greatest and most formidable air power in the world. In the struggle for aerial supremacy no one factor was more important than keeping the lead in technical equipment and air fighting tactics. This of course rested ultimately on experimental work, and it was the general organisation and direction of this work which constituted Hopkinson's contributions to his country's victory. When he was first connected with the Air Force experimental and training work were carried on together at one or two air stations, but by the establishment of the Experimental Station at Orfordness, Hopkinson was able to bring a concentrated effort to bear on

experiment unhampered by the exigencies of training. For this no one could have been more fitted either by nature or experience. Tall, of a commanding presence, with immense physical strength and energy, with ripe engineering experience and great originality of mind, of a perfect and unruffled kindliness and serenity, he commanded respect and confidence in all those who worked with him. His character was in keeping with the external attributes of manliness, without a trace of self-seeking, without a thought that was not directed to the country's good, obviously ready to make any personal sacrifice, and as obviously of a strong, independent, and fearless personality, he was born to command; and command he did.

The war has brought to light a great reserve of nobility and talent in the nation, but no more conspicuous example is to be found than in the life and work of Bertram Hopkinson."

It may be added that immediately before his death plans were being matured for the establishment of a great national school of Aeronautical Engineering, of which Hopkinson was to have been the head.

Hopkinson's published work included a memoir of his father, written as a preface to the reprint of John Hopkinson's collected papers, as well as many accounts of his own researches. No attempt need be made here to analyse these, but a rough notion may be conveyed of the scope of the more important of them. For this purpose they may be grouped under three or four general heads.

Two papers, written in conjunction with Sir Robert Hadfield, describe researches on the magnetic properties of alloys. The molecular theory of magnetism which ascribes the process of magnetisation to the turning into alignment of molecular magnets whose moment is constant, implies that there is a finite "saturation" limit to the magnetisation that can be reached under the influence of any magnetising force, however great. According to the present writer's molecular theory, this limit is easily reached, for the molecular magnets are free to turn save for the control they exert on one another through their mutual magnetic forces. Experiments made as early as 1887 by the "isthmus" method had shown that this is the case in iron, and had determined the saturation limit. Hopkinson, adopting the "isthmus" method, confirmed this conclusion, and went on to examine the limit in a series of iron alloys prepared by Hadfield. These were steels containing various percentages of carbon, and the results confirmed Hopkinson's anticipation that the magnetism of saturation might be predicted from the composition, treating each steel as a mixture of iron and of less magnetisable carbide of iron. This deduction should follow from the principle (deduced from the molecular theory) that the saturation magnetism of an alloy is the sum of that of the constituents, when due account is taken of the proportions in which they are present, provided

they behave simply as a mixture and do not interfere with one another's magnetic properties. It was found that this principle held good in carbon steels with sufficient exactness to suggest that measurements of magnetism might be used as a means of determining the proportion of carbon present in the form of carbide of iron. It was similarly found that silicon and aluminium act mainly as (in the magnetic sense) inactive diluents. With manganese no such simple relation was discovered, and the anomalies which that metal presents in association with iron are suggestive in connection with the Heussler alloys, where it acts as one of three constituents, each non-magnetic when tested alone, to produce an alloy that has a strongly marked magnetic quality. Another joint paper, published by the Iron and Steel Institute in 1914, describes an interesting investigation of the very complicated magnetic properties of Hadfield's manganese steel, which can be toughened by sudden cooling from a high temperature, in which state it is non-magnetic, but can be made to assume various degrees of magnetic susceptibility by prolonged exposure to moderate heat.

Another group of papers deals with the elastic properties of steel and other metals and the departure from perfect elasticity which is known as elastic hysteresis (*Roy. Soc. Proc.* A, vols. 76, 86, and 87). In the course of the investigation Hopkinson devised a high-speed fatigue-tester for examining the endurance of metals under alternating stresses of great frequency. With this machine he could reverse the stress in the specimen with perfect regularity as often as 7000 times per minute, measure the amount of energy dissipated internally through elastic hysteresis, and determine the number of repetitions that were required to produce fracture for various ranges of stress, as well as the range that could (apparently) be endured without fracture, no matter how often the reversal of stress might be repeated. Apart from the interest of the results, the methods of observation described in these papers have much individuality, and may be expected to open the way to further discovery in the same field.

The development of the gas-engine and other internal-combustion motors appealed strongly to Hopkinson as a practical matter on which scientific consideration could usefully be brought to bear. He invented a method of internal cooling, of which great things were hoped, but the results were disappointing. He also invented an optical indicator, which proved itself to be a most effective instrument for revealing what happens in the cylinder. He investigated problems of heat flow and temperature distribution, and he gave much time to the study of gaseous explosions by means of experiments in which a mixture of gas and air was ignited in a closed vessel furnished with appliances for exhibiting and recording the action at various points in the interior, from which the true nature of the action and the manner in which the explosion was communicated from

point to point could be inferred. The devices used in this research were of great ingenuity, and the experiments were prosecuted with conspicuous thoroughness. They cleared up matters that had been obscure, and removed a number of current misconceptions. Sir Dugald Clerk has been good enough to furnish the following notes regarding Hopkinson's labours in this field:

"The science of flame and explosion has suffered a great loss by the death of Bertram Hopkinson. He was one of the most brilliant and enthusiastic experimenters in that field, and carried out very important investigations into the phenomena of gaseous explosion by which he arrived at valuable and interesting conclusions. He demonstrated that in gaseous explosions the maximum temperature is attained in the space surrounding the ignition point. This is due to the adiabatic compression of the flame first formed at the sparking position, by the compression due to the rise in pressure produced by the inflammation of the outer layers, and the temperature there may be raised as much as 300° C. above the average of the explosion from that cause alone.

He also proved by the use of platinum wire resistance thermometers that in such explosions within closed vessels, the expanding flame fills the whole vessel considerably before the termination of the rise in pressure. This had been inferred in early explosion experiments by other experimenters, but the inference depended on a study of the changes occurring in the rate of rise of the explosion curve; it remained for Hopkinson to prove definitely by resistance thermometer observations that the flame reached the sides of the vessel in advance of the point of maximum explosion pressure.

Hopkinson also determined the effect of radiation from the flame in checking the rise of pressure and in altering the rate of cooling. His first experiments were made with cylindrical vessels silver-plated internally and highly polished. In these vessels the maximum pressure increased about 4 lbs. per square inch, equivalent to a temperature rise of 70° C., in the polished vessels as compared with experiments with the same mixtures in the same vessels with the polished surface blackened over.

To study further the radiation from high temperature explosion, Hopkinson designed an ingenious apparatus by which the flame radiation passed through a fluorite window in the explosion vessel, and was measured by means of a platinum strip bolometer. By this he determined that the loss of heat during the explosion period of 0·05 second amounted to nearly 5 per cent. of the whole heat of combustion. Hopkinson continued this investigation with his pupil David, and added greatly to our knowledge of the behaviour of flame as to loss by radiation at about 2000° C. His experiments also on the residual turbulence within the cylinder of internal

combustion engines assisted materially in clearing up our ideas on the effect of the inlet velocity of the gaseous charge upon the economy and action of working engines.

Colonel Hopkinson was most able and resourceful in all his experimental work, which threw much light on the phenomena of gas and petrol engines. He applied his great knowledge to the service of the Admiralty and the War Office while he was Secretary of the Engineering War Committee of the Royal Society, and later in his official position as Deputy Controller of the Technical Department in charge of aeroplane and aero-engine design and construction. His loss is keenly felt by his scientific colleagues and his associates of the Army and Navy."

In still another group of researches Hopkinson dealt with the dynamics of explosions from a different point of view. His paper on "A Method of Measuring the Pressure produced in the Detonation of High Explosives, or by the Impact of Bullets," read before the Royal Society in November, 1913, and published in the *Philosophical Transactions*, describes an investigation remarkable for its completeness no less than its originality. Taking the familiar method of the ballistic pendulum, which serves to measure the momentum of a blow, Hopkinson shows how to analyse this into its two factors, force and time, by means of a novel and ingenious variant. He used for the pendulum a steel rod divided by a transverse joint into a long and a short portion. The rod takes the blow longitudinally and transmits it as a wave of elastic compression which proceeds from the long piece to the short one. At the extreme end of the short piece the wave of compression is reflected back along the rod as a wave of tension, and when the reflected wave reaches the joint the short piece flies off, carrying with it a fraction of the whole momentum which depends upon its length. By adjusting the length of the short piece it may be made to absorb the whole of the momentum of the blow, leaving the main portion of the rod at rest. This enables the length of the pressure wave to be determined, and from that the duration of the blow is readily inferred. Moreover, by using a very short length for the detachable piece the maximum pressure is also measured. The detachable piece is at first maintained in contact with the main portion of the rod by magnetic attraction, which keeps them together while the compression wave passes, but allows them to separate easily as soon as the stress at the joint changes into tension.

Applying this method to examine the blow given by a leaden bullet when fired so as to strike normally the end of the rod in the direction of the length, Hopkinson found that the results confirmed the view that the bullet behaved on impact like a fluid, producing at high speeds nearly the same pressure as could be produced by a fluid jet of corresponding velocity, density, and form. The duration of the blow was only slightly greater than that of such a jet. He discusses fully the causes of the small discrepancy,

which is due in part to rigidity in the bullet and in part to the fact that the strain transmitted along the rod is not a simple wave of compression, uniform over the whole cross-section. But the discrepancy is so small as not to interfere with the value of the method as a means of measuring the force and duration of any blow. From the blows of bullets he passed to those caused by the detonation of gun-cotton near or close to one end of the rod, determined approximate limits of maximum pressure, and showed that at least 80 per cent. of the impulse of the blow had been delivered in one fifty-thousandth part of a second. The results detailed in the paper throw much light on the process of detonation and on the manner in which it produces its destructive effects. During the war the method described in this paper was developed into a standard means of testing explosives, detonators, fuses, etc., for service purposes.

Notably here, and scarcely less in some of his other papers, Hopkinson's scientific writing recalls that of his father. In saying this, one pays it a high tribute. There is the same absence of excrescence and verbiage and vagueness, the same avoidance of side issues, the same direct approach to the very core of the subject; there is the same impression of mastery and ease. Perhaps it is too terse; certainly it is so terse as to need very careful reading. But the careful reader is satisfied as well as convinced. What was said in this place* twenty years ago of the father's writings is no less true of the son's.

His writings, indeed, reflect the sincerity of the man. It was apparent to all with whom he came in contact; he quickly won their liking and respect. His nature was strong and self-reliant, singularly free from egotism or self-seeking. An obviously forceful personality, impressive in figure, in manner, in voice, he was conscious of his power, though not in the least vain of it. No doubt this contributed to another notable characteristic, that his good-humour was imperturbable. His temper kept sunny under the most trying conditions. Always a cheerful comrade, willing to take a full share of any duty, to respond to any demand, buoyant, frank, open, careless of self, he was a delightful associate in office or committee-room. One felt he lacked something, especially in his earlier years, of that comprehension of other men's idiosyncracies and weaknesses which is essential to perfect sympathy; but as time went on his experience, first as a teacher and later as an administrator, mellowed him. Among the men with whom he worked he was, it seemed, a universal favourite.

On the scientific side Hopkinson's strength lay, just as his father's did, in his combining a comprehensive grasp of principles with a just appreciation of practical requirements and possibilities. It was this combination that made him a successful head of the Engineering School at Cambridge,

* *Roy. Soc. Proc.*, vol. 64, Obituary Notices, p. xxiv.

and determined the character of his researches; it was this again that made the value of his war work almost unique. The experiments of the Cambridge laboratory were of high interest in themselves and in their bearing on engineering practice. But to Hopkinson they were more; one may say they constituted an apprenticeship for the culminating work of his life, the work of the last four years. The war gave him an opportunity such as he did not have before. Into it he threw all his inventiveness, all his initiative, his untiring energy, his power of organisation, his unrivalled capacity for getting the best out of himself and out of others. No worker rejoiced more in his work or accepted its call with more absolute self-renunciation. He was amazingly aloof from any consideration of private advantage or personal convenience. The strain was immense: the pressure of claims on his attention was continuous, but it never seemed to ruffle his serenity nor impair the soundness of his judgment. Many will mourn him as a trusted friend, but only those who knew something of what he did in the war can have a right idea of the magnitude of the nation's loss.

The President of the Royal Society, speaking as Master of Trinity in a Commemorative Sermon at the College, said of him: "Our Roll of Honour contains the name of no one who has rendered greater services to his country."

J. A. E.

AN APPRECIATION

[From the "ALPINE JOURNAL," VOL. XXXII, No. 219.]

SOME ten years ago, I forget when or how, a few young men at Trinity were discussing whether anyone they knew at Cambridge could be expected to reach the South Pole if he tried: and they decided that the only man was Hopkinson. It may seem a small thing to record, but it typifies the way in which his personality appealed to younger men; he seemed to combine two great natural gifts—the vigour and enterprise of youth and the knowledge and experience of middle age.

I met him first when, as a young student fresh from examinations, I was beginning research on the mechanical nature of muscular contraction; it occurred to me that this might be regarded, by the not too earnest, as a problem for the Professor of Mechanism, so to the Professor of Mechanism I went and asked his help. He took my visit entirely in the humour in which it was made, and helped to clear up my rather vague ideas as to the meaning of various mechanical conceptions. It was a fortunate introduction, and was followed by many pleasant visits to his house and laboratory, where I learnt to appreciate and admire the vigour, kindliness, and enterprise of his character. My first visit showed me how fundamentally his mind was attuned to the scientific outlook: interested in and concerned with practical problems as he was, and as every inclination made him, his mind remained alert to the methods and ideas of science, not only for their power—which he fully realised—but for their intrinsic merit. It is for this reason that his loss is such a grievous one to Cambridge, where a Professor of Mechanism can hope to make a school essentially in touch with the traditions of the place, only on condition that his interests are largely if not mainly scientific. In Hopkinson Cambridge had an ideal Professor, and the pupils trained in his school have already, especially during the war, raised a memorial to him by their work.

Apart from his work as Professor of Engineering he had a variety of interests, among which may be counted mountain climbing, rowing, sailing, ski-ing and the Officers' Training Corps. He was in command of the R.E. Company in the Corps, and it was in camp at Farnborough that he made his first flights in an aeroplane—surreptitiously before breakfast.

The war, when it came, claimed him at once, though it was not for some months that he turned to the Flying Corps. For all his previous success, and for all his earlier enterprises, it was the war which generally

proved him. He lived just long enough to see the recognition of his work and the success of the men he collected and inspired. The Station at Orfordness was the thing on which he really set his heart, and whenever one saw him there one could see that there was a kind of domestic feeling about it, a feeling that it was his "show," his ideas and his men, working together with a mutual bond of personal respect and affection for him. In spite of the greatly enlarged scope of his authority during his last year it was Orfordness which retained his chief love: he would turn up suddenly, by air or road, with an oily old raincoat, a long lurching stride, a deep voice, a noisy laugh and a tentative unsymmetrical smile half-hidden by a large grey-brown moustache: and would proceed at once to "touch off" a rocket, to fire incendiary bullets into a gas-bag or a petrol tin, to inspect some new "gadget" for a machine-gun, or to practise some other of the many strange arts of which Orfordness was the home. One felt almost envious of the good feeling that surrounded him, and of the pleasure which the work there obviously gave him.

Although twice the age of the average pilot, he learnt to fly and took his "wings." Few can hope to be really good pilots who learn at that age, and of course he was not: he knew it and did not practise "stunts." He was always flying, however, to France, to Orfordness, to Farnborough, and some of his friends felt nervous, knowing his great value and realising the existence of the ten-thousandth chance. He had, however, faced the matter out with himself, and firmly decided that in order to do his work efficiently and to win the necessary approval of his methods, he had himself to be a pilot. The ten-thousandth chance came, and he was killed flying in a bad storm: yet I doubt if anyone will presume to say that he was wrong. He could never have got the power and influence he had in the technical development of the Air Force if he had taken the less courageous and generous course: his conviction of his rôle and his adventurous spirit could never have allowed him to do other than he did: and over all was the fact that he really loved flying and flying by himself.

He was a person of vigorous and commanding mind, softened by a reserved and semi-humorous kindliness and simplicity. He believed strongly in a certain type of men, collected them around him, studied and appreciated their ideas, and backed them up with all his power. The Air Force and the Technical Department owe a great deal to his work and to his wise and critical leadership, and it is difficult to understand why he was allowed to remain a major while doing work of such importance. I doubt whether he cared much—he cared a little, though he laughed at himself even for that little, and was too busy and too wise to let it worry him—and it was obvious that he cared for the work far more than for any possible recognition of it.

A few months before his death I went to see him at his office in Kingsway to tell him of the success of a scheme the details of which he and his people had suggested and of which he had asked me and my people to undertake the development. He had given us all the early opportunities of experimenting on it at Orfordness, and at one critical conference he had interposed when an element of the "old gang" was maintaining that no further developments were needed, and that things were perfect as they were. A few wise decisive words he had spoken at the critical moment secured the possibility of developments required, and the scheme was beginning, at the time I saw him last, to show signs of being a real success: if the war had lasted longer it would have proved a vital factor in air defence. This was merely an offshoot of his work and is given here only as an instance: his part in it, however, his instant appreciation of a fertile method, the confidence he maintained in it against opposition or indifference, his wise and firm support of the people who were undertaking its development, and his pleasure in its success, were typical of the great part he took in the war, and of the still greater part he was destined to take at Cambridge and for the nation had he lived. When I saw him the last time at his office in Kingsway he seemed less reserved, happier, perhaps fitter: it was about that time that he felt his uphill task was over and that at last appreciation of his work, of his men, and of his methods had been reached. His promotion came shortly after, and further promotion would soon have followed.

To the high value of his services to the Air Force perhaps no better tribute can be paid than the one contained in the following letter from the Air Council to his mother; the letter, which was dated August 31, 1918, is quoted in full:

I am commanded by the Air Council to inform you that at their meeting yesterday they passed a resolution placing on record their recognition of the high and permanent value of the work done for the Flying Forces by your late son, Colonel Hopkinson, and their deep sense of the patriotic self-abnegation with which he devoted his great abilities and scientific attainments to the public service. They regret profoundly his untimely death and they desire that there shall be conveyed to you a heartfelt expression of sympathy in your bereavement. The University of Cambridge has lost in him one of its most distinguished members, but the Council feel that he has died in the service of the State and for the furthering of the just cause for which the allied nations are fighting.

It will be difficult to do without him and his vigorous inspiring personality at Cambridge. Great developments will come, as he foresaw, to his school, and he has left a legacy of enterprise and wisdom which cannot fail to bring renewed prosperity. But, for all that, those who knew him at his best, and his best was in the last three years, will appreciate the incalculable loss that has been suffered by his death.

A. V. H.

COLLECTED PAPERS

1

DISCONTINUOUS FLUID MOTIONS INVOLVING SOURCES AND VORTICES.

["PROCEEDINGS OF THE LONDON MATHEMATICAL SOCIETY,"
VOL. XXIX, 1898.]

THE theory of fluid motion in two dimensions where the boundary consists partly of straight walls and partly of free stream-lines over which the pressure is constant has been investigated in a general manner by Mr Love *. His method, however, only applies to cases in which there are no sources or other singularities in the moving fluid, the motion being produced entirely by streams proceeding to or from infinity. This paper contains an extension of the theory to cases in which there are singularities present anywhere in the fluid.

In order the more clearly to show the modifications of Mr Love's method necessitated by this extension, I will quote the preliminary account of the problem and of the method of solving it which appears at the head of his paper:

"The motion of the fluid is supposed to take place in the plane of a complex variable z, and to be given by means of a velocity-potential ϕ and a stream-function ψ, so that $\phi + i\psi$, or w, is a function of z. The object of the theory is to show how in any given problem the functional relation between w and z can be discovered. For this purpose we consider two functions ζ and Ω such that

$$\zeta = \frac{dz}{dw} \quad \text{and} \quad \Omega = \log \zeta,$$

and it is well known that Ω is a complex quantity whose real part is the logarithm of the reciprocal of the velocity at the point z, and whose imaginary part is the angle which the direction of this velocity makes with the real axis in the z-plane. In the kind of motion to which this method applies, the region of the z-plane within which the motion takes place is bounded partly by fixed straight lines and partly by free stream-lines. Along the fixed boundaries the direction of the velocity is given, so that the corresponding parts of the boundary in the Ω-plane are lines parallel to the real axis. Along the free stream-lines the velocity is a given constant, so that the corresponding parts of the boundary in the Ω-plane are parts of a straight line parallel to the imaginary axis. Hence the boundary in the Ω-plane is a polygon which we know how to draw. In like manner, the boundary in the w-plane, consisting of parts of straight lines parallel

* *Proc. Camb. Phil. Soc.* vol. 7, 1891.

to the real axis (ψ = constant), is a polygon which we know how to draw. If, now, we take an auxiliary complex variable u, the polygons in the Ω- and w-planes can be conformally represented on the half-plane for which the imaginary part of u is positive. The required transformations are given by the theory of Schwarz and Christoffel, and we are thus in a position to write down two relations,

$$\frac{dz}{du} = f_1(u),$$

$$\frac{d\Omega}{du} = f_2(u),$$

from which

$$\frac{dz}{du} = Cf_1(u) \exp\{\int f_2(u)\, du\}.''$$

In this way a relation $z = f(u)$ is obtained which is such that the area of motion is conformally represented on that half of the plane of u for which the imaginary part of u is positive.

In cases where sources, sinks, or vortices are present in the moving fluid, it is still possible to obtain the relation $z = f(u)$ by finding Ω and w in terms of u. Mr Love's method of arriving at these functions here fails, however, because they become infinite at the singularities, and the half-plane of u can no longer be conformally represented on areas in the planes of Ω and of w. We cannot, therefore, have recourse to Schwarz's expression for $\frac{d\Omega}{du}$ and $\frac{dw}{du}$, but must obtain these functions direct from their boundary and infinity conditions. I proceed to show how to do this.

Suppose that the motion goes on in a simply-connected area in the z-plane, and that the boundary consists of straight walls and free stream-lines alternately. There may be streams proceeding to or from infinity, and these streams may be bounded either by parallel straight walls or by free surfaces ultimately straight and parallel. ψ is constant over the boundary, save for discontinuities at those points at infinity whence the streams come or whither they go. The velocity is supposed to be finite at all points of the boundary. At isolated points within the area the velocity becomes infinite. The type of these points is $z = \zeta$, where $\frac{dw}{dz}$ becomes finite in the form

$$\frac{A_1}{z-\zeta} + \frac{A_2}{(z-\zeta)^2} + \ldots + \frac{A_m}{(z-\zeta)^m}.$$

Here $A_1, A_2, \ldots, A_m$ may be any complex quantities. The index m is, of course, an integer, and we may call it the order of the singularity at ζ.

The area of motion can always be conformally represented on that half of the plane of a complex variable u for which the imaginary part of u is positive. The problem is to find the relation between z and u in any given case, so that this representation can be effected.

Now, if w be expressed as a function of u, it will represent a fluid motion in the half-plane of u. In this motion the real axis of u is a rigid

boundary over which ψ is constant save for discontinuities of amounts πm_1, πm_2, ... at points t_1, t_2, ... (which correspond to the discontinuities in ψ on the boundary in the z-plane). πm_1, πm_2, ... are the rates of discharge of fluid respectively by the several streams in the z-plane to which the points t_1, t_2, ... correspond. There will be singularities at the points corresponding to the singularities in the z-plane; and the order of each singularity in the u-plane will be equal to the order of the corresponding singularity in the z-plane. Thus, at the point $u = \tau$ corresponding to $z = \zeta$, $\frac{dw}{du}$ becomes infinite, like

$$\frac{B_1}{u-\tau} + \frac{B_2}{(u-\tau)^2} + \ldots + \frac{B_m}{(u-\tau)^m},$$

and we note that $B_1 = A_1$.

The relation between w and u must, therefore, be

$$\frac{dw}{du} = \frac{m_1}{u-t_1} + \frac{m_2}{u-t_2} + \ldots + \Sigma\left[\frac{B_1}{u-\tau} + \frac{B_2}{(u-\tau)^2} + \ldots + \frac{B_m}{(u-\tau)^m}\right] + \Sigma\left[\frac{B'_1}{u-\tau'} + \frac{B'_2}{(u-\tau')^2} + \ldots + \frac{B'_m}{(u-\tau')^m}\right],$$

where $B'_1, B'_2, \ldots, \tau'$ are the complex quantities respectively conjugate to B_1 B_2, ..., τ, and the summations extend to all singularities, such as $u = \tau$.

This equation reduces to the form

$$\frac{dw}{du} = \frac{F(u)}{\phi(u)},$$

where $F(u)$ and $\phi(u)$ are rational integral functions of u with real coefficients, and $F(u)$ is of less degree than $\phi(u)$. The real roots of $\phi(u)$ are $t_1, t_2, \ldots$, and no two of them are equal. The complex roots occur in conjugate pairs, and those of them whose imaginary parts are positive are at the points τ which correspond to the singularities. Each complex root is repeated a number of times equal to the order of the singularity to which it corresponds.

The method of procedure which I adopt is as follows:—Beginning with the motion in the half-plane of u given by the general formula

$$\frac{dw}{du} = \frac{F(u)}{\phi(u)},$$

I shall find the most general relation $z = f(u)$ which is such that,

(1) The half-plane of u is conformally represented on a simply-connected area in the z-plane.

(2) In the motion in that area given by w (expressed as a function of z) the velocity of q is constant over some parts of the boundary, while the direction of motion of the fluid is constant over the other parts.

It will then be possible, by proper choice of the functions $F(u)$ and $\phi(u)$ and of the constants which arise in the general form of $z = f(u)$, to

arrive at any possible case of motion of the kind described in the z-plane. The proper choice of these functions and constants to suit any particular case forms the second part of the problem.

The first step is to determine the infinity and boundary conditions to be satisfied by Ω. Suppose that there are n free pieces in the boundary in the z-plane. Corresponding to these there will be n segments of the real axis of u—say from u_1 to u_2, u_3 to u_4, ..., u_{2n-1} to u_{2n}. The remaining n segments, occurring alternately with these, correspond to the straight parts of the boundary. Ω is a function of u, the real part of which does not vary over any one of the first-named segments. On the other segments the imaginary part does not vary, except discontinuously at isolated points which correspond to the points on the straight walls at which the fluid is at rest.

Infinities of Ω.—Consider, first, points within the region of motion and not on the boundary. Since the representation of the region of motion on the half-plane of u is conformal, $\log \frac{dz}{du}$ does not become infinite at any such point. Now

$$\Omega = \log \frac{dz}{du} - \log \frac{dw}{du},$$

whence it follows that Ω becomes infinite at the same points, and in the same manner as $-\log \frac{dw}{du}$. That is, Ω becomes infinite at every complex root of $F(u)$ and of $\phi(u)$, the imaginary part of which is positive, and at no other points. Let σ be such a root of $F(u)$, σ' be its conjugate root, and let it be repeated k times, so that $F(u)$ contains the factor $(u-\sigma)^k (u-\sigma')^k$. Then Ω becomes infinite when $u = \sigma$, like $-k \log (u-\sigma)$. Similarly, at the point τ, where τ, as before, is a complex root of $\phi(u)$ within the half-plane, and repeated m times, Ω becomes infinite, like $m \log (u-\tau)$.

Turning now to points on the boundary, we remember that one of the postulates as to the nature of the motion was that the velocity should not become infinite at any point on the boundary. Hence Ω can only become infinite on the boundary, if at all, at those points at which the fluid is at rest. These points of rest can clearly only occur on the straight walls, and not on the free stream-lines. Now we have

$$\frac{dw}{dz} = \frac{dw}{du} \cdot \frac{du}{dz}.$$

Provided we confine ourselves to points at which both u and z are finite, $\frac{dz}{du}$ cannot be zero or infinite on a straight wall. Hence the fluid is at rest, and Ω is infinite at such roots of $F(u)$ as fall on segments of the real axis which correspond to straight parts of the boundary. If s be such a root of $F(u)$, and if it be repeated p times, then Ω becomes infinite when $u = s$,

like $-p \log (u - s)$. Ω does not become infinite at any other points on the real axis of u, except in certain cases when $u = \infty$.

The motion is supposed to go on in a finite area, except for the streams proceeding to or from infinity. Hence the fluid is not at rest, and Ω is not infinite for infinite values of z.

If the point in the z-plane corresponding to $u = \infty$ be either at an infinite distance from the origin, or on a free stream-line, Ω cannot be infinite there. If, however, it be on a straight wall, Ω may be infinite. But we have

$$\frac{d\Omega}{du} = -\frac{d}{du} \log \left(\frac{dw}{du}\right) + \frac{d}{du} \log \left(\frac{dz}{du}\right).$$

Now, at the point on the straight wall corresponding to $u = \infty$, $\frac{dz}{du}$ vanishes, like $\frac{\text{const.}}{u^2}$. Also $\frac{dw}{du}$ vanishes when $u = \infty$, like $\frac{\text{const.}}{u}$. Hence $\frac{d\Omega}{du}$ vanishes when u is infinite.

We may now sum up the conditions to be satisfied by the function Ω:

(1) Its real part is constant over each of the n segments of the real axis (u_1 to u_2, u_3 to u_4, ..., u_{2n-1} to u_{2n}) which correspond to the free parts of the boundary in the z-plane.

(2) Its imaginary part is constant over each of the remaining n segments, save for certain discontinuous changes.

(3) It becomes infinite at every complex root of $F(u)$ or of $\phi(u)$ which falls within the half-plane. At a root σ of $F(u)$, repeated k times, Ω is infinite, like $-k \log (u - \sigma)$; and at a root τ of $\phi(u)$, repeated m times, it is infinite, like $+m \log (u - \tau)$.

(4) It becomes infinite at those real roots of $F(u)$, of which the type is s, which are such that $u_{2r+1} > s > u_{2r}$. The infinity is of the type $-p \log (u - s)$, where the root is repeated p times.

(5) For infinite values of u, Ω may be infinite, but $\frac{d\Omega}{du}$ vanishes.

From these boundary and infinity conditions we have now to determine Ω.

Consider the equation

$$\frac{d\Omega}{du} = \frac{\chi}{\sqrt{(u - u_1)(u - u_2) \ldots (u - u_{2n})}}, \qquad \ldots\ldots(1)$$

where χ is a function of u which is real for real values of u. It is clear that, as u increases along the real axis from $-\infty$ to $+\infty$, the growth of Ω is alternately entirely in its real and in its imaginary part. On those segments of the real axis for which

$$u_{2r+1} > u > u_{2r},$$

and which correspond to the straight parts of the boundary in the fluid motion, the real part of Ω alone varies, and its imaginary part is constant.

On the other, and alternate, segments the real part of Ω is constant. Thus, Ω, if determined from this equation, will satisfy boundary conditions of the right character; and by means of the function χ we can introduce infinities and discontinuities into Ω as we please, the only restriction on that function being that it must be real for real values of u.

Suppose that we take

$$\chi = C_0 + C_1 u + C_2 u^2 + \ldots + C_{n-1} u^{n-1} - \Sigma \frac{p\alpha}{u-s} - \Sigma k \left(\frac{\beta}{u-\sigma} + \frac{\beta'}{u-\sigma'} \right) + \Sigma m \left(\frac{\gamma}{u-\tau} - \frac{\gamma'}{u-\tau'} \right), \quad \ldots\ldots(2)$$

where $C_0, C_1, \ldots, C_{n-1}$ are real. The symbols $p, s, \sigma, \sigma', k, \tau, \tau', m$ bear the same meanings as before, and the summations extend to all the infinities of Ω. The quantities α are real; and the quantities β', γ' are respectively the complex quantities conjugate to β, γ.

Then χ clearly satisfies the conditions of being real for real values of u. Also, at the point $u = s$, $\frac{d\Omega}{du}$ becomes infinite, like

$$-\frac{p\alpha}{\sqrt{(s-u_1)(s-u_2)\ldots(s-u_{2n})}} \frac{1}{u-s},$$

and Ω becomes infinite, like

$$-\frac{p\alpha}{\sqrt{(s-u_1)(s-u_2)\ldots(s-u_{2n})}} \log(u-s).$$

Hence Ω will become infinite at $u = s$ in the right way if

$$\alpha = \sqrt{(s-u_1)(s-u_2)\ldots(s-u_{2n})}.$$

Since the root s is so situated that

$$u_{2r+1} > s > u_{2r},$$

it follows that α is real for every such root, as was postulated above.

Similarly, in order that Ω may become infinite in the right way when $u = \sigma$, and when $u = \tau$, we must take

$$\beta = \sqrt{(\sigma-u_1)(\sigma-u_2)\ldots(\sigma-u_{2n})}$$

and

$$\gamma = \sqrt{(\tau-u_1)(\tau-u_2)\ldots(\tau-u_{2n})},$$

and we shall then have

$$\beta' = \sqrt{(\sigma'-u_1)(\sigma'-u_2)\ldots(\sigma'-u_{2n})},$$

$$\gamma' = \sqrt{(\tau'-u_1)(\tau'-u_2)\ldots(\tau'-u_{2n})},$$

the sign affixed to the square roots being such that β' is the complex quantity conjugate to β, and γ' that conjugate to γ.

The function Ω determined in this way only becomes infinite at the points whose types are s, σ, τ; and $\frac{d\Omega}{du}$ vanishes for infinite values of u. Thus Ω satisfies the right conditions.

The introduction of the n arbitrary constants $C_0, C_1, \ldots, C_{n-1}$ insures that Ω shall be the most general possible function that satisfies the conditions. This is most readily seen by considering a simple electrical analogy. Suppose that the half-plane of u is a uniformly conducting sheet, the real axis being its edge. Let the segments of the edge from u_1 to u_2, u_3 to u_4, ..., u_{2n-1} to u_{2n} be made perfectly conducting and kept at arbitrary potentials, the remaining segments being left free. Let there be electrodes in the sheet and on the edges thereof, at the points whose types are σ, τ, s. Let the electrode at s discharge an amount $-p\pi$ of electricity per second, that at σ an amount $-k\pi$, and that at τ an amount $+m\pi$. Then the electrical potential is clearly the real part of Ω, where Ω is the function that we have been seeking in the fluid motion problem. The arbitrary potentials at which the n segments of the edge are maintained are respectively the logarithms of the reciprocals of the velocities over the n free pieces of the boundary of the fluid motion. Now the real part of Ω, as determined from equation (1) above, contains $n + 1$ arbitrary constants—*i.e.*, the n constants $C_0, C_1, \ldots, C_{n-1}$, and one constant of integration. These can be determined in one way, and in one way only, for any given set of values of the potentials of the n segments of the edge, and of the potential at infinity.

This electrical problem has some little interest of its own. It has not, so far as I am aware, been solved hitherto, except for cases where all the electrodes are on the edge of the sheet. In such cases the ordinary equation of Schwarz and Christoffel effects an immediate solution. In the particular form of Ω found above, which corresponds to the fluid motion problem, the strengths of the electrodes are integers. The solution is, however, clearly not confined to cases where the electrodes are of integral strength.

It is worth noting that the above equation for $\frac{d\Omega}{du}$ is a particular case of a more general equation. The ordinary transformation of Schwarz and Christoffel,

$$\frac{dz}{du} = (u - u_1)^{-\alpha_1/\pi} (u - u_2)^{-\alpha_2/\pi} \ldots,$$

gives z as a function of u which is finite and continuous for all values of u, and which is such that the argument of $\frac{dz}{du}$ is constant over the real axis save for discontinuous changes at the points $u_1, u_2, \ldots$. If we take, instead of the equation last written, the modified equation

$$\frac{dz}{du} = \chi (u - u_1)^{-\alpha_1/\pi} (u - u_2)^{-\alpha_2/\pi} \ldots,$$

where χ is a function of u which is real for real values of u, we shall get a function z which is such that the argument of $\frac{dz}{du}$ is constant on the real axis save for discontinuous changes at the same points and of the same

amounts as before. The function z will, however, no longer be finite and continuous for all values of z; but we can introduce into it any infinities or discontinuities that we please by proper choice of χ.

The expression for χ in equation (2) may be put into a more compact form. The complex roots of $F(u)$ and of $\phi(u)$ occur in conjugate pairs, since the coefficients in these functions are all real. Hence the quantities of which the types are σ' and τ' are roots of $F(u)$ and of $\phi(u)$ respectively. We also have

$$\beta' = \sqrt{(\sigma' - u_1)(\sigma' - u_2) \dots (\sigma' - u_{2n})},$$

$$\gamma' = \sqrt{(\tau' - u_1)(\tau' - u_2) \dots (\tau' - u_{2n})}.$$

We may therefore write

$$\chi = -\Sigma \frac{\lambda}{u - \sigma} + \Sigma \frac{\mu}{u - \tau}, \qquad \dots(3)$$

where σ now stands for any root of $F(u)$, real or complex, and in either half-plane, which does not lie on a segment of the real axis of u corresponding to a free part of the boundary. Thus σ now includes all the roots of $F(u)$ previously denoted by the symbols s, σ, and σ'. τ stands for any complex root of $\phi(u)$ in either half-plane. We also have

$$\lambda = \sqrt{(\sigma - u_1)(\sigma - u_2) \dots (\sigma - u_{2n})},$$

$$\mu = \sqrt{(\tau - u_1)(\tau - u_2) \dots (\tau - u_{2n})}.$$

If a root be repeated any number of times, the term corresponding to it in the expression for χ is repeated the same number of times.

We have now, therefore, determined Ω in terms of u. The relation between z and u which we seek is given by

$$\frac{dz}{du} = e^{\Omega} \frac{dw}{du}.$$

This relation will involve a number of constants, such as the roots of $F(u)$ and $\phi(u)$, and the quantities $C_0, C_1, \dots, C_{n-1}, u_1, u_2, \dots, u_{2n}$. If we assign particular values to these constants, and if, furthermore, we make one particular point in the z-plane correspond to one in the half-plane of u, so as to fix z as a one-valued function of u in the half-plane, then the half-plane of u will be conformally represented on a determinate region in the plane of z. The motion in this region given by w will be such that over n pieces of the boundary the velocity is constant, while over the remaining n pieces, occurring alternately with these, the direction of motion of the fluid is constant. By properly choosing the constants we can arrive at any possible motion whatever which satisfies these conditions. The singularities in the motion will be the same in number and order as those in the corresponding motion in the half-plane of u.

In the examples which follow, I investigate in each case the general expressions for a particular class of motion in which there are singularities of a given number and order, and in which the boundary consists of a given

number of straight and free pieces. The number and order of the singularities in the motion in the half-plane of u are then known, but their positions and strengths are not known. Three of the constants which occur in the general relation connecting z and u can be arbitrarily assigned without affecting the generality. For, suppose that we take a new complex variable v, given by

$$v = \frac{au + b}{a'u + b'},$$

where a, b, a', b' are real quantities. Then the region in the z-plane can be conformally represented on the half-plane of v, and the functional relation between z and v will involve the three ratios of a, b, a', b'; to each of which we can give any value we please.

By proper choice of the remaining constants in the general relation between z and u, we can arrive at any particular case of motion which comes within the class which is being investigated.

Example I. The boundary consists of a free stream-line and a straight wall. ψ is constant all round the boundary; and the only singularity is a vortex, the circulation round which is $2m\pi$. The velocity on the free stream-line is unity.

If the area of motion be conformally represented on the half-plane of u, the motion in that half-plane represented by w will be that due to a single vortex, the circulation round which is $2m\pi$. The representation can always be effected so that this vortex is on the imaginary axis at the point $u = i\gamma$; and, at the same time, the straight wall may be made to correspond to that segment of the real axis of u which lies between -1 and $+1$.

The relation between w and u with which we start is, therefore,

$$\frac{dw}{du} = \frac{mi}{u - i\gamma} - \frac{mi}{u + i\gamma} = -\frac{2m\gamma}{u^2 + \gamma^2},$$

and from this it must be possible, by a proper transformation, to arrive at any possible motion which satisfies the conditions enumerated above. We note that Ω is not infinite when u is infinite, since the point corresponding to $u = \infty$ is on the free stream-line.

We shall take the axis of x parallel to the straight wall. Ω is then a function of u, the imaginary part of which vanishes on the real axis between -1 and $+1$, while the real part vanishes for real values of u outside these limits. Also, Ω becomes infinite when $u = i\gamma$, like $\log(u - i\gamma)$, and does not become infinite for any other value of u. We have, therefore,

$$\frac{d\Omega}{du} = \frac{1}{\sqrt{1 - u^2}}\left[\frac{\sqrt{1 + \gamma^2}}{u - i\gamma} + \frac{\sqrt{1 + \gamma^2}}{u + i\gamma}\right].$$

Take a new variable θ, given by $u = \sin\theta$, and let $\cos\theta = +\sqrt{1 - u^2}$. The effect of this is to conformally represent the area of motion on an

area on the plane of θ bounded by the lines parallel to the imaginary axis and by a segment of the real axis between $-\frac{\pi}{2}$ and $+\frac{\pi}{2}$; the latter segment corresponding to the straight wall (see Fig. 1). Let $\gamma = \sinh \alpha$, so that the point $u = i\gamma$ corresponds to the point $\theta = i\alpha$.

The equation for Ω now integrates in the form

$$\Omega = \log \frac{\cos\theta - \cosh\alpha}{\cos\theta + \cosh\alpha},$$

the constants being so chosen that the imaginary part vanishes if θ lie on the segment AA' in Fig. 1, while the real part vanishes if it be on AB or $A'B'$.

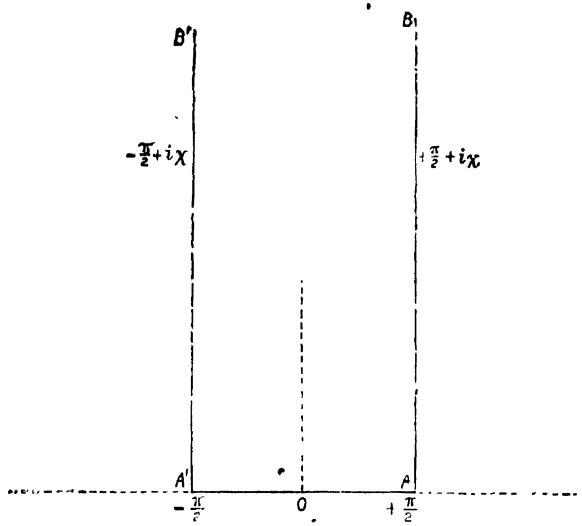


Fig. 1

Thus we find

$$\frac{dz}{du} = e^{\Omega}\frac{dw}{du}$$
$$= -2m\gamma \frac{1}{(\cos\theta + \cosh\alpha)^2}.$$

This is the relation between z and u, and it contains one arbitrary constant, *i.e.*, γ. By choice of γ it must be possible to cover every possible case of motion which satisfies the conditions with which we started.

We have

$$\frac{dz}{d\theta} = \frac{-2m\sinh\alpha\cos\theta}{(\cos\theta + \cosh\alpha)^2},$$

and, if the origin be taken in the middle of the straight wall, the coordinates of the ends of the wall will be

$$y = 0,$$
$$x = \pm\int_0^{\frac{1}{2}\pi} \frac{2m\sinh\alpha\cos\theta}{(\cos\theta + \cosh\alpha)^2}\,d\theta.$$
$$= \pm 2m\left(\frac{1}{\gamma} - \frac{1}{\gamma^2}\tan^{-1}\gamma\right).$$

The expression last written is equal to half the length of the wall, so that we have an equation for determining γ.

The vortex is at the point corresponding to $\theta = i\alpha$, and its coordinates are

$$x = 0,$$

$$y = -\int_0^\alpha \frac{2m\gamma \cosh\phi \, d\phi}{(\cosh\phi + \cosh\alpha)^2}$$

$$= -2m\gamma \left\{1 - \frac{1}{\gamma^2}\log(1+\gamma^2)\right\}.$$

Thus the vortex must be on the line of symmetry of the wall, and it must, furthermore, be at a determinate distance from the wall, in order that it may be possible to complete the boundary with a free stream-line.

One half of the free stream-line corresponds to AB in Fig. 1, and the other half to $A'B'$. At points on the first half θ is equal to $\frac{\pi}{2} + i\chi$, and on the other half to $-\frac{\pi}{2} + i\chi$, where χ is real and positive, and ranges from 0 to α.

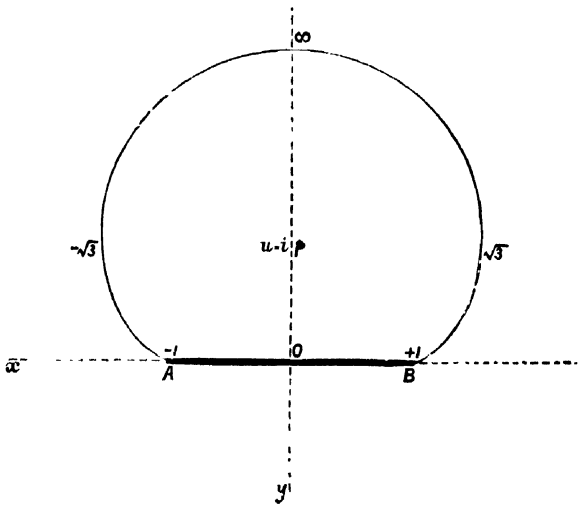


Fig. 2

In Fig. 2, I have drawn the case where $m = 1$, and $\gamma = 1$. In this case points on the free stream-line are given by

$$\frac{dz}{d\chi} = -\frac{2i\sinh\chi}{(\cosh\alpha - i\sinh\chi)^2}$$

or

$$\frac{dx}{d\chi} = -\frac{2\sinh\chi\,(3-\cosh^2\chi)}{(1+\cosh^2\chi)^2},$$

and

$$\frac{dy}{d\chi} = -\frac{4\sqrt{2}\sinh^2\chi}{(1+\cosh^2\chi)^2}.$$

The vortex is at P. The figures affixed to the various points are the values of u corresponding to these points. The length of the wall is $4 - \pi$, and the distance of the vortex from it is $4 - \log_e 2$.

Example II. The motion is produced by a vortex and doublet at the same point. The boundary consists wholly of a free stream-line, over which the velocity is unity. The circulation round the vortex is $-2\pi m$, where m is positive.

If the area of motion be conformally represented on the half-plane of u, the motion represented by w will be that excited by a vortex and doublet combined. The representation can, in all cases, be effected (by choice of the three constants at our disposal) in such wise that the vortex and doublet are at the point $u = i$, while the axis of the doublet is parallel to the real axis. The form of the boundary in the z-plane will manifestly depend solely on the ratio of the strengths of the doublet and vortex; its dimensions only being affected by the absolute magnitudes of these quantities. We therefore assume that the strength of the doublet in the u-plane is unity. The strength of the doublet in the z-plane will then depend upon the relation between z and u.

We have
$$\frac{dw}{du} = \frac{1}{(u-i)^2} + \frac{1}{(u+i)^2} - im\left(\frac{1}{u-i} - \frac{1}{u+i}\right)$$
$$= \frac{2(1+m)(u^2+\beta^2)}{(u^2+1)^2},$$
where
$$\beta^2 = \frac{m-1}{m+1}.$$

The motion in the half-plane of u given by this equation is to be transformed into a motion in the z-plane, over the boundary of which the velocity is unity. I propose to investigate the nature of this motion for various values of m.

The real part of Ω is zero for all real values of u; and Ω becomes infinite at those roots and points of $\frac{dw}{du}$ which are not on the real axis. Two classes of cases, therefore, arise, according as β is real or imaginary.

(*a*) $m > 1$ and β real. In this case Ω becomes infinite when $u = i\beta$, like $-\log(u - i\beta)$; and when $u = i$, like $2\log(u - i)$. We have, therefore,
$$\Omega = \log\frac{u+i\beta}{u-i\beta} + 2\log\frac{u-i}{u+i},$$
whence
$$\frac{dz}{du} = e^{\Omega}\frac{dw}{du}$$
$$= \frac{2(1+m)(u+i\beta)^2}{(u+i)^4}.$$

This gives, on integration,
$$\frac{z}{2(1+m)} = -\frac{1}{u+i} + \frac{i(1-\beta)}{(u+i)^2} + \frac{(1-\beta)^2}{(u+i)^3},$$
the origin being taken at $u = \infty$.

The vortex is at $u = i$, and the fluid is at rest at $u = i\beta$. The equation to the boundary is formed by giving u real values in the equation for z. Thus we find, for points on the boundary,

$$\frac{x}{2(1+m)} = \frac{u}{3(1+u^2)^3}\left[-3u^4 + (1 - 8\beta + \beta^2)\,u^2 - 3\beta^2\right],$$

$$\frac{y}{2(1+m)} = \frac{1}{3(1+u^2)^3}\left[3(2-\beta)\,u^4 + 3(1 + 2\beta - \beta^2)\,u^2 + 1 + \beta + \beta^2\right].$$

Fig. 3 shows the case where $m = \frac{5}{3}$ and $\beta = \frac{1}{2}$. The vortex and doublet are at P, and the point of rest ($u = i\beta$) is at Q.

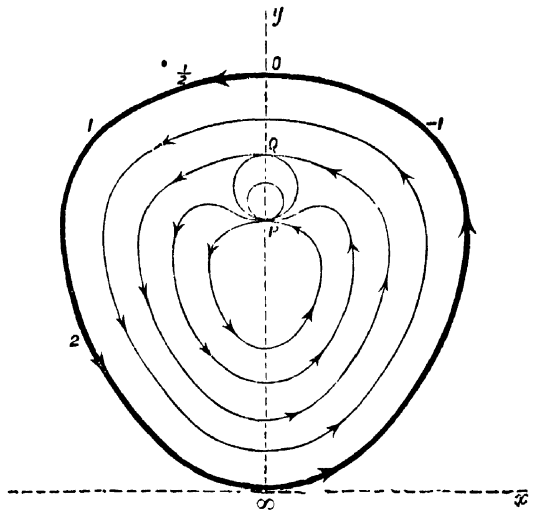


Fig. 3

As m diminishes a dimple forms in the boundary. This dimple deepens, and its sides approach until at last they overlap, as shown in Figs. 4 and 5.

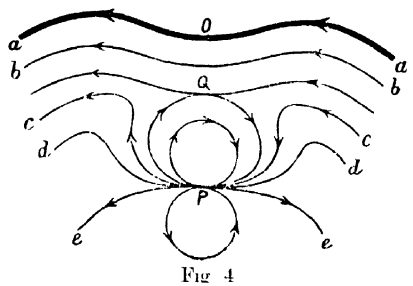


Fig. 4

In these figures the part of the area of motion in the neighbourhood of the point of rest and vortex is shown on an enlarged scale.

Such overlapping will often take place in the general case. By making one particular point in the z-plane correspond to a particular point in the u-plane, we can always get z as a one-valued function of u, but we cannot insure that u shall be a one-valued function of z.

A case of motion with overlap is, of course, only of analytical interest; though it is possible to conceive it as going on in a very thin layer.

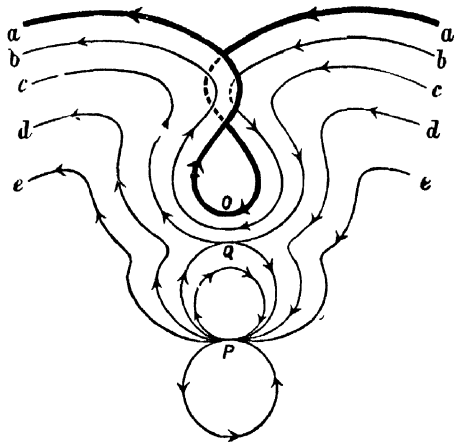


Fig. 5

As m further diminishes the overlap increases, until we reach the limiting case shown in Fig. 6, in which $m = 1$ and $\beta = 0$.

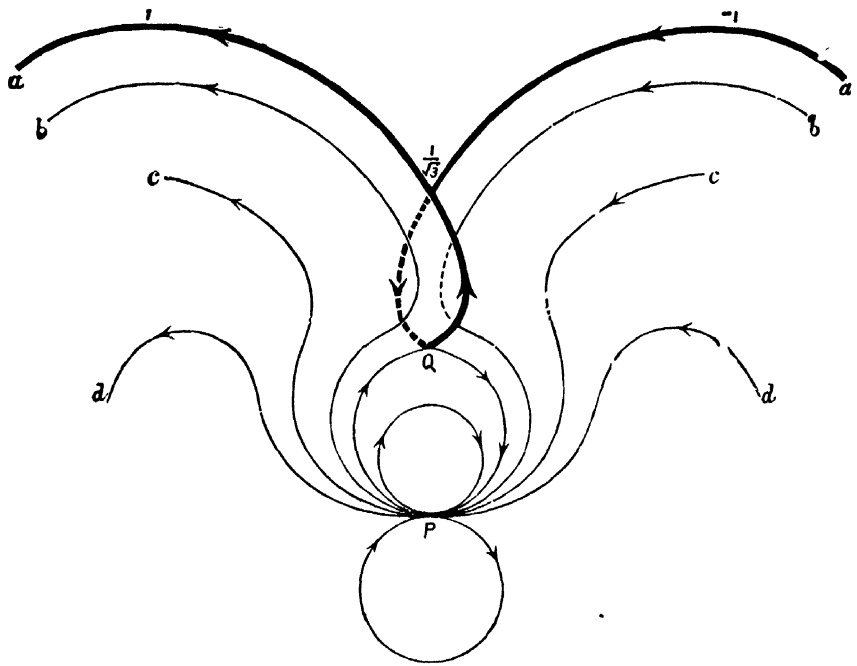


Fig. 6

In this case $$\frac{dz}{du} = \frac{2u^2}{(u+i)^4},$$

and $$\Omega = 2 \log \frac{u-i}{u+i}.$$

The point $u = \beta$, which in Figs. 3, 4, and 5 was the point of rest, is now on the boundary, and the fluid is no longer at rest there.

In Figs. 3, 4, 5, and 6, the boundary stream-line is drawn thick. The lighter lines show the general disposition of stream-lines. In Figs. 3 and 6 the boundary is drawn accurately. Figs. 4 and 5 are mere sketches to show how the motion develops as m diminishes. In these figures, as in Fig. 6, the complete drawing would be of unmanageable dimensions; so part only of some of the stream-lines is shown.

In each case P is the vortex and doublet ($u = i$), and Q is the point $u = i\beta$, which, in Figs. 3, 4, and 5, is the point of rest.

When there is overlap, the stream-lines of the underlying layer are shown dotted.

(*b*) If m still further diminishes, β becomes imaginary. We have

$$\frac{dw}{du} = \frac{2(1+m)(u^2-\gamma^2)}{(u^2+1)^2},$$

where γ is real and equal to $\sqrt{\frac{1-m}{1+m}}$.

Ω is now infinite at $u = i$, like $2\log(u-i)$, and at no other point. Its real part vanishes for real values of u, as before. We have, therefore,

$$\Omega = 2\log\frac{u-i}{u+i},$$

and now

$$\frac{dz}{du} = \frac{2(1+m)(u^2-\gamma^2)}{(u+i)^4}.$$

This integrates in the form

$$\frac{z}{2(1+m)} = -\frac{1}{u+i} + \frac{i}{(u+i)^2} + \frac{1+\gamma^2}{3(u+i)^3},$$

the origin being taken at $u = \infty$.

The coordinates of any point on the free stream-line are given by

$$\frac{3x}{2(1+m)} = -\frac{u\{3u^4 - (1+\gamma^2)u^2 + 3\gamma^2\}}{(u^2+1)^3},$$

$$\frac{3y}{2(1+m)} = \frac{6u^4 + 3(1-\gamma^2)u^2 + 1 + \gamma^2}{(u^2+1)^3}.$$

There is a cusp with the point turned inwards at the points $u = \pm\gamma$. Fig. 7 shows the case where $\gamma^2 = \frac{1}{2}$, $m = \frac{1}{3}$. Fig. 8 is a sketch similar to Figs. 4, 5, and 6, showing an intermediate stage where there is overlap.

It will have been noted that, in this example, the point of rest develops quite continuously with change of m into two cusps on the boundary. The same thing will happen in the same sort of way in the general case of discontinuous fluid motion considered at the beginning of this paper. Suppose that there is an unrepeated complex root σ in $\frac{dw}{du}$. Then $\frac{dw}{du}$ contains a factor $(u-\sigma)(u-\sigma')$ in the numerator, and the fluid is at rest

at the point corresponding to $u = \sigma$. It will also be remembered that, if

$$\frac{d\Omega}{du} = \frac{\chi}{\sqrt{(u - u_1)(u - u_2) \ldots (u - u_{2n})}},$$

then χ contains a term $\frac{\alpha}{u - \sigma} + \frac{\alpha'}{u - \sigma'}$, where

$$\alpha = \sqrt{(\sigma - u_1)(\sigma - u_2) \ldots (\sigma - u_{2n})}.$$

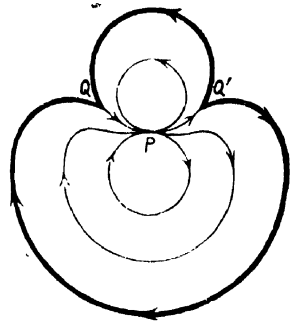


Fig. 7

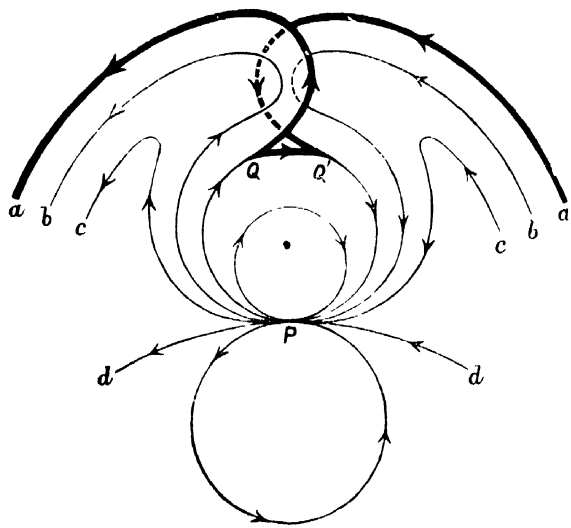


Fig. 8

Now let the strengths and positions of the sources or other singularities in the z-plane be varied, so that the root σ tends to approach a segment of the real axis of u which corresponds to a free part of the boundary. A change in the form of that part of the boundary will then take place, somewhat as shown in Figs. 3, 4, and 5. After a certain amount of this change, σ will become real and equal to σ'. At the same time α becomes a pure imaginary and equal to $-\alpha'$; so that the term in χ corresponding to σ disappears. Thus, at this stage, $\frac{dz}{du}$ vanishes, like const. $\times (u - \sigma)^2$,

when $u = \sigma$; for Ω and $\frac{dz}{dw}$—hitherto infinite at $u = \sigma$—are now finite at that point. It follows that, when σ has just become real, there must be overlap in the neighbourhood of the point corresponding to $u = \sigma$, somewhat like that shown in Fig. 6.

If the change be carried further, the two roots σ, σ'—now both real—separate. There is no term in χ corresponding to either, and so, at each of the points $u = \sigma$, $u = \sigma'$, $\frac{dz}{du}$ vanishes, like $(u - \sigma)$ and $(u - \sigma')$ respectively. Thus, at the point corresponding to each on the bounding curve in the z-plane, there is a cusp with its point turned inwards. In this way we get a succession of shapes such as those shown in Figs. 7 and 8.

The overlap will, in general, begin before σ has actually become real; and will not disappear until after σ and σ' have separated some distance.

Example III. A stream escapes from between parallel walls and proceeds to infinity with free surfaces, which are ultimately straight and parallel. The stream is disturbed by a doublet at any point in its path.

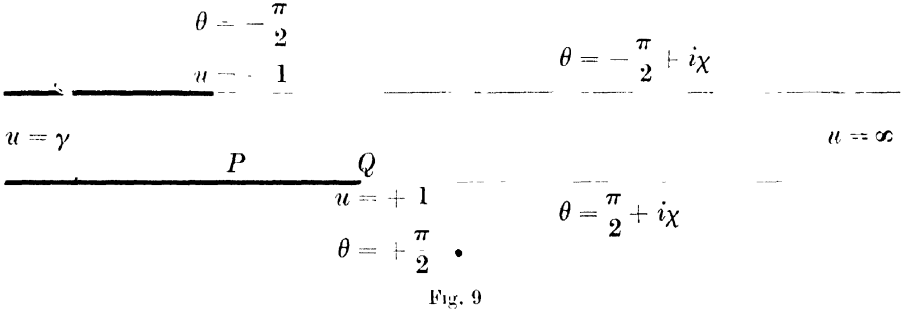


Fig. 9

Let the area of motion be conformally represented on the half-plane of u. The representation can always be effected so that the straight walls correspond to that segment of the real axis of u which lies between $u = -1$ and $u = +1$, while the point at infinity to which the stream proceeds corresponds to $u = \infty$. Let the point at infinity whence the stream comes correspond to $u = \gamma$ (see Fig. 9).

Suppose that the stream discharges an amount π of fluid per second. In the motion in the u-plane represented by w, there is a source at the point $u = \gamma$ discharging at the same rate as the stream. There is no other singularity on the real axis of u. There is also a doublet in the u-plane corresponding to that in the z-plane, say at the point $u = \tau$. We have, therefore,

$$\frac{dw}{du} = \frac{1}{u - \gamma} + \frac{m}{(u - \tau)^2} + \frac{m'}{(u - \tau')^2},$$

which may be written in the form

$$\frac{dw}{du} = \frac{(u - \sigma_1)(u - \sigma_2)(u - \sigma_3)(u - \sigma_4)}{(u - \gamma)(u - \tau)^2(u - \tau')^2}.$$

Now Ω is a function of u which becomes infinite when $u = \tau$ and when $u = \tau'$, and also at such of the roots σ_1, σ_2, σ_3, σ_4 as do not lie on the real axis of u outside the limits $\pm$ 1. Also, assuming that the velocity on the free surfaces is unity, Ω is a pure imaginary for real values of u outside the limits $\pm$ 1, and is real if u be within those limits. There are, therefore, five classes of cases, distinguished from one another by the number of cusps appearing on the free part of the boundary, such cusps corresponding in each case to the excepted roots of $\frac{dw}{du}$. The form of the relation between z and u will be different for these several classes; but any one of the classes may be continuously developed into any other in the manner illustrated in the last example.

These classes are as follows:

I. No excepted roots of $\frac{dw}{du}$. In this case we have,

$$\frac{d\Omega}{du} = \frac{1}{\sqrt{1-u^2}}\left[-\frac{\sqrt{1-\sigma_1^2}}{u-\sigma_1} - \frac{\sqrt{1-\sigma_2^2}}{u-\sigma_2} - \frac{\sqrt{1-\sigma_3^2}}{u-\sigma_3} - \frac{\sqrt{1-\sigma_4^2}}{u-\sigma_4} + 2\frac{\sqrt{1-\tau^2}}{u-\tau} + 2\frac{\sqrt{1-\tau'^2}}{u-\tau'}\right],$$

where it is to be noted that the roots σ_1, σ_2, σ_3, σ_4 may consist either of two conjugate complex pairs, one complex pair and two real roots, or four real roots. Thus, in this case there may be four points of rest on the boundary, or two on the boundary and one in the fluid, or two in the fluid. The form of Ω will be the same in all cases of this class.

II. One excepted root. Here

$$\frac{d\Omega}{du} = \frac{1}{\sqrt{1-u^2}}\left[-\frac{\sqrt{1-\sigma_1^2}}{u-\sigma_1} - \frac{\sqrt{1-\sigma_2^2}}{u-\sigma_2} - \frac{\sqrt{1-\sigma_3^2}}{u-\sigma_3} + 2\frac{\sqrt{1-\tau^2}}{u-\tau} + 2\frac{\sqrt{1-\tau'^2}}{u-\tau'}\right].$$

In this case there is one cusp on the free stream-line, corresponding to the excepted root. There are either two or three points of rest.

III. Two excepted roots, and two cusps on the boundary. Either one or two points of rest.

IV. Three excepted roots and three cusps on the boundary. One point of rest, which must be on the boundary.

V. Four excepted roots. In this case there are four cusps on the boundary, and we have

$$\frac{d\Omega}{du} = \frac{1}{\sqrt{1-u^2}}\left[2\frac{\sqrt{1-\tau^2}}{u-\tau} + 2\frac{\sqrt{1-\tau'^2}}{u-\tau'}\right].$$

The above equations are readily integrated by the substitution $u = \sin\theta$. If we put $\sigma_1 = \sin\theta_1$, etc., and $\tau = \sin\theta_0$, we shall get for the first class of cases

$$\frac{dz}{d\theta} = \frac{\cos\theta}{\sin\theta - \gamma} \frac{\cos^2\frac{1}{2}(\theta+\theta_1)\cos^2\frac{1}{2}(\theta+\theta_2)\cos^2\frac{1}{2}(\theta+\theta_3)\cos^2\frac{1}{2}(\theta+\theta_4)}{\cos^4\frac{1}{2}(\theta+\theta_0)\cos^4\frac{1}{2}(\theta+\theta'_0)},$$

and for the second class

$$\frac{dz}{d\theta} = \frac{\cos\theta}{\sin\theta - \gamma} \frac{\cos^2\frac{1}{2}(\theta+\theta_1)\cos^2\frac{1}{2}(\theta+\theta_2)\cos^2\frac{1}{2}(\theta+\theta_3)(\sin\theta - \sin\theta_4)}{\cos^4\frac{1}{2}(\theta+\theta_0)\cos^4\frac{1}{2}(\theta+\theta'_0)},$$

and so for the other classes, until for the fifth class we have

$$\frac{dz}{d\theta} = \frac{\cos\theta}{\sin\theta - \gamma} \frac{(\sin\theta - \sin\theta_1)(\sin\theta - \sin\theta_2)(\sin\theta - \sin\theta_3)(\sin\theta - \sin\theta_4)}{\cos^4\frac{1}{2}(\theta+\theta_0)\cos^4\frac{1}{2}(\theta+\theta'_0)}.$$

These five classes will exhaustively cover every possible case.

The quantities $\sin\theta_1$, $\sin\theta_2$, $\sin\theta_3$, and $\sin\theta_4$ are the roots of the equation

$$(u-\tau)^2(u-\tau')^2 + (u-\gamma)[m(u-\tau')^2 + m'(u-\tau)^2] = 0,$$

and are immediately expressible in terms of m, m', τ, τ', and γ. In any given case the data are the coordinates and direction of the doublet with reference to the straight walls, the magnitude of the doublet, and the amount PQ by which one wall projects beyond the other. In working out such a case, it is necessary to proceed successively on the five assumptions that the case belongs to each of the classes enumerated above. For each such assumption values will be obtained for m, τ, and γ in terms of the data. Thus we have

$$\text{strength of doublet} = \text{mod}\left\{m\left(\frac{dz}{d\theta}\right)_{\theta=\theta_0}\right\},$$

$$\text{inclination of doublet} = \text{argument of } m\left(\frac{dz}{d\theta}\right)_{\theta=\theta_0},$$

$$\text{doublet is at point corresponding to } \theta = \theta_0,$$

$$PQ = \text{real part of } \int_{-\frac{1}{2}\pi}^{+\frac{1}{2}\pi}\left(\frac{dz}{d\theta}\right)d\theta,$$

which are just sufficient equations for the determination of m, τ, and γ; and, consequently, of σ_1, σ_2, σ_3, σ_4.

If the motion is a possible one, it will be found that on one of these assumptions, and one only, the distribution of the roots σ_1, σ_2, σ_3, σ_4 is appropriate to that assumption. We then know that this is the correct assumption, and can proceed to work out the motion completely.

The effect of the transformation is to conformally represent the area of motion on that portion of the plane of θ which is shown in Fig. 1. The form of the free stream-lines is given by ascribing to θ values of the form $\pm\frac{\pi}{2} + i\chi$, where χ is real and positive.

2

THE "HUNTING" OF ALTERNATING-CURRENT MACHINES.

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MANY years ago the late Dr John Hopkinson showed that if a pair of alternating-current dynamos, A and B, mechanically separate but connected electrically in parallel, be running steadily on a constant load and with a constant driving power, and if the steady motion be slightly disturbed, say by momentarily retarding A, then A will do less and B more than its share of the work, with the result that there will be a balance of force tending to accelerate A and to retard B and so to restore the state of steady motion. In other words the two machines tend to keep in step. Similar considerations apply to a synchronous alternating-current motor worked from supply mains—it tends to keep in step with the generators supplying it.

It has been found in practice that as a general rule the paralleled alternators do keep in step, but in a not inconsiderable number of cases great trouble has been caused by a tendency in the machines to develop gradually increasing oscillations about the state of steady motion in which they are in step with one another. This oscillation or "hunting" leads to violent cross-magnetising currents, and sometimes the machines drop out of step altogether. This phenomenon has received a great deal of attention from the practical side, the object being of course to put an end to it. This experimental study has resulted in empirical rules as to fly-wheel effect, and in the various damping devices or "amortisseurs" which are now largely used on alternating-current machinery and generally give satisfactory parallel running.

Theoretical treatment of hunting has been confined (so far as I am aware) to the work of Kapp, and of others on similar lines*, who have ascribed it to resonance. Take the case of a synchronous alternating-current motor driven from a source of alternating E.M.F. of constant amplitude and periodicity. In all essentials this is the same problem as that of one alternating machine running in parallel with a number of others, but the results are simpler to express. The motor is supposed to work against a constant resistance, and corresponding to that resistance there will be a certain state of steady motion in which the motor runs with a

* See *Dynamomaschinen für Gleich- und Wechselstrom*, von Gisbert Kapp, p. 401; also a paper by Hans Görges, *Elektrotechnische Zeitschrift*, vol. 8, 1900.

constant speed equal to that of the generators supplying it and with a constant lag e behind them, and develops a torque T corresponding to the external resistance. If the angle of lag be increased to $e + \xi$, the torque will be increased to $T + \frac{dT}{de}\xi$, and if the external work done by the machine remains the same there will be a force $\xi \frac{dT}{de}$ or $c\xi$ tending to accelerate the motor. Kapp's argument then is that the equation of motion of the motor is $M\ddot{\xi} + c\xi = 0$, where M represents on a suitable scale the moment of inertia of the motor, from which it follows that it executes simple harmonic oscillations of constant amplitude and period $2\pi\sqrt{(M/c)}$ about the state of steady motion. It is easy to calculate c approximately from a knowledge of the magnetic properties of the machine. Kapp worked out the period and found it to agree fairly well with observation in certain cases. He ascribed hunting to approximate equality between the period of free oscillation and that of some variation in the turning moment of the engine. Such equality would of course give rise to forced oscillations quite out of proportion to the cause. It was stated in support of this explanation that in certain cases an increase in the fly-wheel effect of the machine—viz., an increase in the period of oscillation—was found to aggravate the evil, contrary to what would at first sight be expected. And indeed it is probable that some cases of hunting are due to resonance.

I believe, however, that there have been cases in which it has been difficult or impossible to discover any external disturbing cause of approximately the same period as that of the oscillation. One case of the kind has come under my notice. A small single-phase synchronous motor, to be presently described, hunted violently under certain conditions when worked off the Wimbledon supply mains. The period of the oscillations could be varied continuously from about 0·35 to 0·45 second by appropriate variation of the self-induction in series with the motor. Furthermore, the hunting occurred equally, and with the same period, with either of two different generators working in the Power Station, one being three times the size of the other. It was clear, therefore, that in this case at any rate the hunting was not due to resonance but to some essential instability in the motion of the motor itself.

It is easy to see how such instability could arise. In the argument given above it has been assumed that the torque is dependent only on the relative position of motor and generator and not on their relative velocity. As a matter of fact there is a term in the torque dependent on the velocity, and the equation of motion is $M\ddot{\xi} + b\dot{\xi} + c\xi = 0$. Higher differential coefficients than the second may and in fact do come in, but these are the most important terms as a rule. The solution of this equation is, if b is small,

$$\xi = \xi_0 e^{-b/2M} \sin\left[\sqrt{\left(\frac{c}{M}\right)}\, t + \eta\right].$$

If b be positive, the amplitude of the oscillations continually decreases. If, however, b were negative, even though very small, the oscillations would continually increase and the motion be essentially unstable. Most dynamical systems are affected with viscosity, in which case b is positive, but systems are not unknown in which the contrary is the case. Watt's Governor is such a system*. Its oscillations about steady motion are given by a cubic equation, the two complex roots of which have positive real parts and correspond to constantly increasing oscillations. There is no doubt that the motion of a synchronous motor is under certain conditions another instance of the same thing.

Suppose for the present that the motor has a permanent magnet or saturated field, and that it is working against a constant load.

Let θ be an angle defining the position of the armature in space, $I = A \sin\theta$ the induction linked with the field coils and with the armature when in position θ. In virtue of the above assumption, A is constant. Let t be the time, and $E\cos pt + F\sin pt$, the E.M.F. of the source of supply; L the self-induction and ρ the resistance of the armature and any conductors in series with it.

Assuming for the moment that the motor is moving with uniform angular velocity, let $u = \alpha_0 \sin pt + \beta_0 \cos pt$ be the current in the armature. The epoch of t is as yet unchosen; choose it so that in the steady motion $\theta = pt$. Now suppose that the state of steady motion is slightly disturbed so that the motor oscillates about it. Then we have in the disturbed motion

$$\theta = pt + \xi \quad \text{and} \quad u = (\alpha_0 + \alpha)\sin pt + (\beta_0 + \beta)\cos pt,$$

where ξ, α, β are small quantities varying periodically with the time. Experience shows that in all cases the period of the variation is long compared with $2\pi/p$ the period of the alternating current. The external E.M.F. remains the same—$E\cos pt + F\sin pt$—the induction $A\sin\theta$ is also undisturbed by the oscillation.

Forming the equation for the E.M.F. at the terminals of the motor in the usual way and equating it to the impressed E.M.F. we find

$$\begin{aligned} E\cos pt + F\sin pt &= \rho u + L\dot{u} + \frac{dI}{dt} \\ &= \rho\{(\alpha_0 + \alpha)\sin pt + (\beta_0 + \beta)\cos pt\} \\ &\quad + Lp\{(\alpha_0 + \alpha)\cos pt - (\beta_0 + \beta)\sin pt\} \\ &\quad + L\dot{\alpha}\sin pt + L\dot{\beta}\cos pt \\ &\quad + A(p + \dot{\xi})\cos(pt + \xi). \end{aligned}$$

This equation is rigorously accurate under the assumptions proposed. Now equate the coefficients of $\cos pt$ and $\sin pt$ on the two sides, neglect products of the small quantities α, β, ξ, $\dot{\alpha}$, etc., and separate the large terms

* See Routh's *Rigid Dynamics*, vol. 2 (1892), p. 74; also Maxwell's *Collected Papers*, vol. 2, p. 105.

corresponding to steady motion and the small terms corresponding to disturbed motion in the usual way.

Thus for steady motion

$$E = \rho\beta_0 + Lp\alpha_0 + Ap, \qquad F = \rho\alpha_0 - Lp\beta_0,$$

and for the disturbed motion

$$L\dot{\alpha} + \rho\alpha - Lp\beta - Ap\xi = 0, \qquad \ldots\ldots(1)$$

$$Lp\alpha + L\dot{\beta} + \rho\beta + A\dot{\xi} = 0. \qquad \ldots\ldots(2)$$

The torque developed by the motor is

$$u\frac{dI}{d\theta} = A\cos\theta\{(\alpha_0 + \alpha)\sin pt + (\beta_0 + \beta)\cos pt\}$$

$$= A\cos(pt + \xi)\{(\alpha_0 + \alpha)\sin pt + (\beta_0 + \beta)\cos pt\}$$

$$= \tfrac{1}{2}(A\beta_0 + A\beta - A\alpha_0\xi) + \text{terms of period } \pi/p \text{ and quicker period},$$

again neglecting products of the small quantities. The large part of this, $\frac{1}{2}A\beta_0$, is equal to the constant resistance, the small balance is available for accelerating the motor. Thus the third equation is obtained,

$$2M\ddot{\xi} + A\alpha_0\xi - A\beta = 0. \qquad \ldots\ldots(3)$$

To solve equations (1), (2), and (3), we write as usual $\alpha = Pe^{xt}$, $\beta = Qe^{xt}$, and $\xi = Re^{xt}$, and so get, after substitution and elimination of P, Q, and R,

$$\begin{vmatrix} Lx + \rho & -Lp & -Ap \\ Lp & Lx + \rho & Ax \\ 0 & -A & (A\alpha_0 + 2Mx^2) \end{vmatrix} = 0,$$

which reduces to

$$(A\alpha_0 + 2Mx^2)\{(Lx + \rho)^2 + L^2p^2\} + A^2L(x^2 + p^2) + A^2\rho x = 0. \quad \ldots(4)$$

The condition of stability of motion is, as explained in works on dynamics, that the real roots of this equation shall be negative, and that the real parts of the complex roots shall be negative. The criterion for this can be written down*, but in this case it is simpler to proceed by approximation. It is known that x is a small quantity, the period of the oscillation we are investigating being in any practical case at least fifteen times that

* The condition that the real roots and the real parts of the complex roots of the biquadratic

$$ar^4 + br^3 + cr^2 + dr + e = 0$$

shall all be negative is that the quantity

$$bcd - ad^2 - eb^2,$$

and the coefficients a, b, c, d, e shall all have the same sign (see Routh's *Rigid Dynamics* (1892), vol. 2, p. 192). In the case of the biquadratic here treated, this condition reduces to

$$2A(2L\alpha_0 + A) - \frac{8M}{L}(L^2p^2 - \rho^2) > 0,$$

which is equivalent to the condition $Lp < \rho$ found later on, if, as is the fact, the first term can be neglected in comparison with the second.

of the alternating current. Neglect x/p and $x\rho/Lp^2$ therefore altogether in the first instance. Thus we obtain

$$(A\alpha_0 + 2Mx^2)(L^2p^2 + \rho^2) + A^2Lp^2 = 0,$$

whence

$$x = i\sqrt{\left\{\frac{1}{2M}\left(A\alpha_0 + \frac{A^2Lp^2}{L^2p^2 + \rho^2}\right)\right\}} = i\delta, \text{ suppose,}$$

so that to this order of approximation the motor executes simple harmonic oscillations of constant amplitude and period $2\pi/\delta$. This, subject to an approximation which holds good in practical cases, is the result obtained by Kapp. Now, suppose that $x = \gamma + i\delta$, and substitute in the original equation (4). Neglect γ^2, x^2/p^2, and γ/p, but keep x/p, $x\rho/Lp^2$, etc.

The result is

$$4M\gamma i\delta(L^2p^2 + \rho^2) + (A\alpha_0 - 2M\delta^2)2Li\delta\rho + A^2\rho i\delta = 0,$$

whence

$$\begin{aligned} 4M\gamma &= \frac{A^2\rho + (A\alpha_0 - 2M\delta^2)2L\rho}{L^2p^2 + \rho^2} \\ &= -\frac{\rho}{L^2p^2 + \rho^2}\left(A^2 - \frac{A^2Lp^2 2L}{L^2p^2 + \rho^2}\right) \\ &= \rho A^2 \frac{L^2p^2 - \rho^2}{(L^2p^2 + \rho^2)^2}. \end{aligned} \qquad \ldots\ldots(5)$$

If, therefore, $Lp > \rho$, as is nearly always the case, γ is positive, and the motion is unstable, the oscillations if once started continually increasing in amplitude according to the law $e^{\gamma t}$.

In the above it has been assumed that the field is unaffected by the oscillations, remaining the same in the disturbed as in the steady motion. As a matter of fact this is not usually the case, the field is disturbed by the oscillation owing to the varying armature reaction. The general effect is easily expressed thus. The induction linked with armature and field coils in the steady motion is supposed, as before, to be $A \sin\theta$. In the disturbed motion the field is slightly altered in amount, and slightly distorted, in a periodic way, and becomes $I = (A + a)\sin\theta + b\cos\theta$, where a and b are small quantities dependent on α, β, and ξ. Terms involving $\sin 2\theta$ and $\cos 2\theta$ will also appear to some extent, but may for practical purposes be neglected. The current in the armature is

$$\begin{aligned} u &= (\alpha_0 + \alpha)\sin pt + (\beta_0 + \beta)\cos pt \\ &= (\alpha_0 + \alpha)\sin(\theta - \xi) + (\beta_0 + \beta)\cos(\theta - \xi) \\ &= \alpha_0 \sin\theta + \beta_0\cos\theta \\ &\quad + (\alpha + \beta_0\xi)\sin\theta + (\beta - \alpha_0\xi)\cos\theta, \end{aligned}$$

and the general effect of the varying current on the field is that

$$a = \kappa(\alpha + \beta_0\xi), \qquad b = \lambda(\beta - \alpha_0\xi),$$

where κ and λ are constant for any given state of steady motion. κ represents the change in total induction produced by varying the "wattless"

component of the current, and λ the change in distortion produced by varying the other component. Taking these expressions for the induction, it is easy to find the time of oscillation and rate of increase as in the simpler case investigated above. It will suffice here to give the results. The period of the oscillation is $2\pi/\delta$, where

$$2M\delta^2 = + A\alpha_0 - \lambda\alpha_0^2 - \kappa\beta_0^2 + \frac{\{(A - \lambda\alpha_0)^2 (L + \kappa) + \kappa^2\beta_0^2 (L + \lambda)\} p^2}{(L + \kappa)(L + \lambda) p^2 + \rho^2},$$

and the amplitude increases at the rate $e^{\gamma t}$, where

$$4M\gamma = \frac{\rho}{\{(L + \kappa)(L + \lambda) p^2 + \rho^2\}^2} [p^2 \{(A - \lambda\alpha_0)^2 (L + \kappa)^2 + \kappa^2\beta_0^2 (L + \lambda)^2\} - \rho^2 \{(A - \lambda\alpha_0)^2 + \kappa^2\beta_0^2\}]. \quad \ldots\ldots(6)$$

As a rule $\kappa\beta_0$ is small compared to $(A - \lambda\alpha_0)$, so that the criterion of instability in this case is approximately $(L + \kappa) p > \rho$, and is generally fulfilled. The value of $(L + \kappa)$, which will not vary greatly with load, may be determined by observing the current given by the machine when short-circuited, including, of course, in its circuit all resistances and self-induction up to the constant source of E.M.F., which has been postulated. The apparent resistance under these circumstances is approximately $\sqrt{([L + \kappa]^2 p^2 + \rho^2)}$. On the other hand, the value of L may be approximately obtained by observing the current when the machine is at rest, and connected to the mains, the field coils being short-circuited. The apparent resistance of the whole circuit under these circumstances is $\sqrt{([L^2 p^2 + \rho^2])}$. If the motor be under-excited α_0 is positive. β_0 is positive for a motor and negative for a generator. The E.M.F. $Ap \cos pt$ is equal to the impressed E.M.F. less that required to drive the current through the self-induction and resistance of the circuit; hence, unless Lp, or ρ, or the current, be large, $Ap/\sqrt{(2)}$ is not much different from the applied E.M.F. $[\sqrt{(\text{mean}^2)}]$. By reducing the exciting current Ap is somewhat reduced, owing to the increased armature current; but the reduction is far from being in proportion to the decrease in exciting current. On machines with large armature reaction and small self-induction, Ap is practically constant, and equal to $\sqrt{(2)}$ times the impressed E.M.F. $[\sqrt{(\text{mean}^2)}]$.

Damping Coils.

In practice there are, of course, many causes unconsidered in the above investigation which tend to damp out the oscillations, and which in all but exceptional cases overpower the forces making for instability, and render the motion stable. Air resistance and local currents in the armature give rise to forces of this nature, which increase roughly in proportion to the velocity and so appear in the equations of motion as true viscous terms. The most important damping effect, however, is that due to the variations of the field of the motor, which, as stated in the last paragraph, are generally produced by the oscillation. These variations give rise to periodically

varying electric forces in the substance of the magnets and in the field coils, which cause currents in the former and variations of the current in the latter. These induced currents re-act on the armature, producing changes in the torque which tend to damp out the oscillations. In many machines the effect is intensified by putting additional damping coils of copper or "amortisseurs," as they are called, round the field magnets. I propose shortly to investigate this damping effect.

The general effect of the induced currents in the magnets and in their surrounding coils (whether damping coils or field coils*) is equivalent to that of a circuit of a certain resistance R supposed to surround completely laminated magnets of the same size and shape magnetised by a constant field current. The induction linked with this circuit will be, taking the notation already used, νI or $\nu (A + a)$, where ν is a constant. The current round the circuit, produced by the oscillation, is, therefore,

$$-\frac{\nu}{R}\frac{dI}{dt}, \text{ or } -\frac{\nu}{R}\frac{da}{dt}.$$

This current tends to slightly demagnetise the magnets if a be increasing, and its effect on the induction linked with the field coils and armature may be represented by a term $-\mu\frac{da}{dt}$, where μ is a positive constant. The quantity μ is a time, it is the time in which the induction in the magnets falls to I/e of its initial value if the field coils be suddenly short-circuited. If the armature be forced to make small oscillations, given by $\xi = \xi_0 \sin \delta t$, about the state of steady motion, the induction will be as before (p. 26),

$$I = (A + a) \sin \theta + b \cos \theta,$$

where now, however,

$$a = -\mu\frac{da}{dt} + \kappa (\alpha + \beta_0 \xi), \qquad \ldots\ldots(7)$$

while, as before,

$$b = \lambda (\beta - \alpha_0 \xi).$$

The E.M.F. at the terminals of the motor is

$$\begin{aligned} E \cos pt + F \sin pt &= \rho u + L\dot{u} + \frac{dI}{dt} \\ &= \rho \{(\alpha_0 + \alpha) \sin pt + (\beta_0 + \beta) \cos pt\} \\ &\quad + Lp \{(\alpha_0 + \alpha) \cos pt - (\beta_0 + \beta) \sin pt\} \\ &\quad + (A + a) p \cos (pt + \xi) \\ &\quad - \lambda (\beta - \alpha_0 \xi) p \sin pt \\ &\quad + \left[L\dot{a} \sin pt + L\dot{\beta} \cos pt + A\dot{\xi} \cos pt \right. \\ &\quad \left. + \frac{da}{dt} \sin pt + \lambda (\dot{\beta} - \alpha_0 \dot{\xi}) \cos pt \right]. \end{aligned}$$

* It is worth noting here that the result, so far as damping is concerned, is the same whether the additional copper is put into a separate short-circuited winding or into the field coils. In the latter form it assists in reducing the ordinary losses in those coils.

We drop the terms in square brackets, and investigate the damping effect of the "amortisseur" coil separately as a small effect of the same order of magnitude as the opposite effect which is dependent on those terms. The result is

$$\rho\alpha - (L + \lambda)\, p\beta - Ap\xi + \lambda\alpha_0 p\xi = 0,$$

and

$$ap + Lp\alpha + \rho\beta = 0,$$

whence

$$\{L\,(L + \lambda)\, p^2 + \rho^2\}\,\beta + Lp^2\,(A - \lambda\alpha_0)\,\xi + ap\rho = 0,$$

and

$$\{L\,(L + \lambda)\, p^2 + \rho^2\}\,\alpha + a\,(L + \lambda)\, p^2 - p\rho\,(A - \lambda\alpha_0)\,\xi = 0.$$

These give α and β in terms of ξ and a, and substituting in (7), we find

$$a\left\{1 + \frac{\kappa\,(L + \lambda)\, p^2}{L\,(L + \lambda)\, p^2 + \rho^2}\right\} + \mu\frac{da}{dt} = \xi_0 \sin\delta t\left\{\kappa\beta_0 + \frac{\kappa p\rho\,(A - \lambda\alpha_0)}{L\,(L + \lambda)\, p^2 + \rho^2}\right\},$$

whence it follows that

$$a = a_0 \sin(\delta t + \epsilon),$$

where

$$\tan\epsilon = -\frac{\mu\delta}{1 + \dfrac{\kappa\,(L + \lambda)\, p^2}{L\,(L + \lambda)\, p^2\rho^2}}$$

and

$$a_0\left\{1 + \frac{\kappa\,(L + \lambda)\, p^2}{L\,(L + \lambda)\, p^2 + \rho^2}\right\} = \kappa\left\{\beta_0 + \frac{p\rho\,(A - \lambda\alpha_0)}{L\,(L + \lambda)\, p^2 + \rho^2}\right\}\xi_0 \cos\epsilon. \qquad (8)$$

The small periodic term in the torque, produced by the oscillation, is found, as before, to be

$$\tfrac{1}{2}\{a\beta_0 + (A - \lambda\alpha_0)\,\beta - (A - \lambda\alpha_0)\,\alpha_0\xi\}.$$

The value of this when $t = 0$ or $\xi = 0$, that is when the motor is in the position of steady motion, though not moving with the steady velocity, is

$$\tfrac{1}{2}\left\{a_0\beta_0 \sin\epsilon - (A - \lambda\alpha_0)\frac{p\rho a_0 \sin\epsilon}{L\,(L + \lambda)\, p^2 + \rho^2}\right\},$$

which from (8) is equal to

$$-\tfrac{1}{2}\frac{\kappa\xi_0\mu\delta\left[\beta_0^2 - \dfrac{p^2\rho^2\,(A - \lambda\alpha_0)^2}{\{L\,(L + \lambda)\, p^2 + \rho^2\}^2}\right]}{\mu^2\delta^2 + \left\{1 + \dfrac{\kappa\,(L + \lambda)\, p^2}{L\,(L + \lambda)\, p^2 + \rho^2}\right\}^2}. \qquad \text{.....(9)}$$

The velocity at this moment is $\left[\dfrac{d}{dt}(\xi_0 \sin\delta t)\right]_{t=0}$ or $\xi_0\,\delta$.

The oscillation about the state of steady motion is, therefore, resisted by a force proportional to the excess of the actual velocity over that of the steady motion, or by a true viscous force, and the magnitude of the force is, in the simple case when $\rho = 0$,

$$-\tfrac{1}{2}\frac{\kappa\mu\beta_0^2}{\mu^2\delta^2 + (1 + \kappa/L)^2}\dot{\xi} = -2M\gamma'\dot{\xi}. \qquad \text{.....(10)}$$

Taking this force into consideration, as well as the similar force of opposite sign whose existence was proved in the first paragraph, we find finally that the free oscillations of the system are given by the equation

$$\xi = \xi_0 e^{(\gamma - \gamma')t} \sin\delta t,$$

where γ and γ' have the following values nearly, if ρ is small,

$$\left.\begin{aligned}\gamma &= \frac{\rho A^2}{4M(L+\kappa)^2 p^2} \\ \gamma' &= \frac{\kappa\mu\beta_0^2}{4M\left\{\mu^2\delta^2 + \left(1+\frac{\kappa}{L}\right)^2\right\}}.\end{aligned}\right\} \qquad \text{.....(11)}$$

These results have been obtained for a single-phase motor, with a constant self-induction and a sine-wave E.M.F. A generator running in parallel with a number of others is, of course, covered by the same equations. The results are applicable, with slight modification, to a two-phase or three-phase machine, and it would be possible, if worth while, to find the alterations introduced by the varying self-induction and distorted wave-forms which exist more or less in all actual dynamos. In no case, however, could accurate quantitative results be arrived at without great labour, for the forces here investigated are small, and in actual work a great many small disturbing causes would have to be taken into account (such, for example, as small changes in the resistance overcome by the motor*) before an accurate quantitative criterion of stability could be arrived at. A good deal of valuable information can, however, be got out of the equations as to the general effect upon stability of running of varying the constants of the machine.

(1) γ' diminishes as δ increases or the damping increases with the period of the oscillation. Hence increased flywheel effect always results in better damping owing to the increase in the period. Increasing the self-induction has the same effect; and it also works in favour of stability by diminishing γ. A mere alteration of M without altering δ has no effect because it alters γ and γ' in the same ratio.

(2) The damping is proportional to $\kappa\beta_0^2$. Now the steady torque is $\frac{1}{2}A\beta_0$. Hence for a fixed field excitation the damping term is about proportional to the square of the load; the more the machines are loaded (within limits) the better they run in parallel. This is in accord with experience. Furthermore, with constant load, β_0 is inversely proportional to A, and a reduction of A results in better damping. This I have also found to be true on the machine with which I experimented, viz.; that if with a constant load you diminish the field current you get more stable running.

(3) The coefficient of instability, or γ, will in most cases (for which ρ is small compared with Lp) be proportional to ρA^2 and inversely proportional to L^2p^2.

(4) If μ be increased from zero, in other words if the resistance to the induced currents in and about the pole-pieces be diminished, the damping effect corresponding to γ' first increases to a maximum and then diminishes.

* A case of great practical importance, in which the changes of load can be calculated easily, is that of the rotary converter.

It is possible therefore to carry the application of "amortisseurs" too far. That this is so is obvious when one considers that a coil of no resistance round the pole-piece would completely destroy all variation of induction and all the damping effect which depends on such variations. It would in fact correspond to the case first investigated of a motor with a fixed field, or without armature reaction, which as shown is usually unstable.

(5) Referring to the general expression for γ' (equation 9) in which the resistance is taken into account, it appears that if

$$\beta_0^2 < \frac{p^2\rho^2 (A - \lambda\alpha_0)}{\{L (L + \lambda) p^2 + \rho^2\}^2},$$

γ' becomes negative, and the damping coils, instead of reducing the oscillations, actually tend to increase them.

$$\frac{(A - \lambda\alpha_0) p}{\sqrt{L (L + \lambda) p^2 + \rho^2}}$$

is about equal to $C (\sqrt{2})$, where C is the current in the machine when standing and connected to the mains. The limits outside which β_0 must lie in order that the damping coils may operate as such, and not as additional causes of instability, are therefore about

$$\pm \frac{\rho}{Lp} C (\sqrt{2}),$$

and are very narrow in machines of any size. The matter only becomes practically important in the case of small machines, and of motors on long transmission lines. The behaviour of the small motor with which I experimented is considerably influenced in this way.

Experimental Confirmation.

I have in a general way confirmed the results here obtained by experiment on a small alternating-current motor. This machine is a 4-pole generator, made by the Westinghouse Company, and intended to give an output of about 10 amperes continuous current at 110 volts. It was converted into a synchronous alternating-current machine by fitting slip rings on to it.

The machine was separately excited, and the no-load characteristic curve for a speed of 1500 revolutions per minute is shown on Fig. 1. The currents are in arbitrary units, the potential is the square root of mean square of the alternating potential. The lower parts of the characteristic are a good deal affected by the previous treatment of the magnets; the curve gives average values. The machine gives, at no-load and at all excitations, very approximately a sine-wave of E.M.F., the actual value being E ($\sin pt + \frac{1}{30} \sin 3pt$ + terms of higher order). With a heavy non-inductive load and low field, however, the field is a good deal distorted. In Fig. 2 is shown the curve of E.M.F. of the machine when excited with a current of 10·5 and working as a generator on a non-inductive resistance amounting together with the armature resistance to 1·5 ohms.

The machine was worked off the local supply mains, which gave a

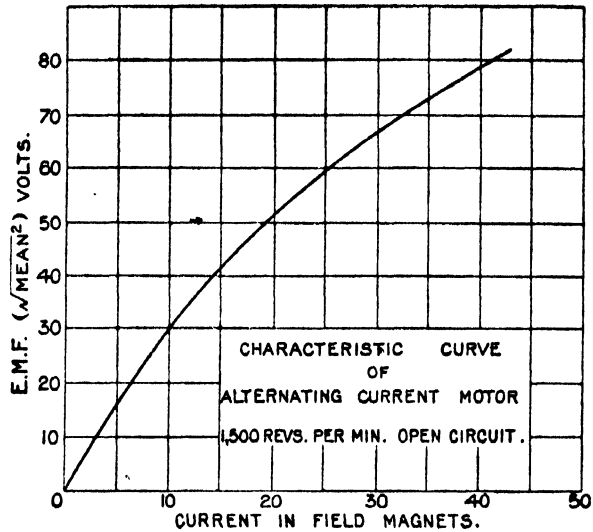

E.M.F. (√MEAN²) VOLTS.
80
70
60
50
40
30
20
10
0
10
20
30
40
50
CURRENT IN FIELD MAGNETS.
CHARACTERISTIC CURVE
OF
ALTERNATING CURRENT MOTOR
1,500 REVS. PER MIN. OPEN CIRCUIT.

Fig. 1

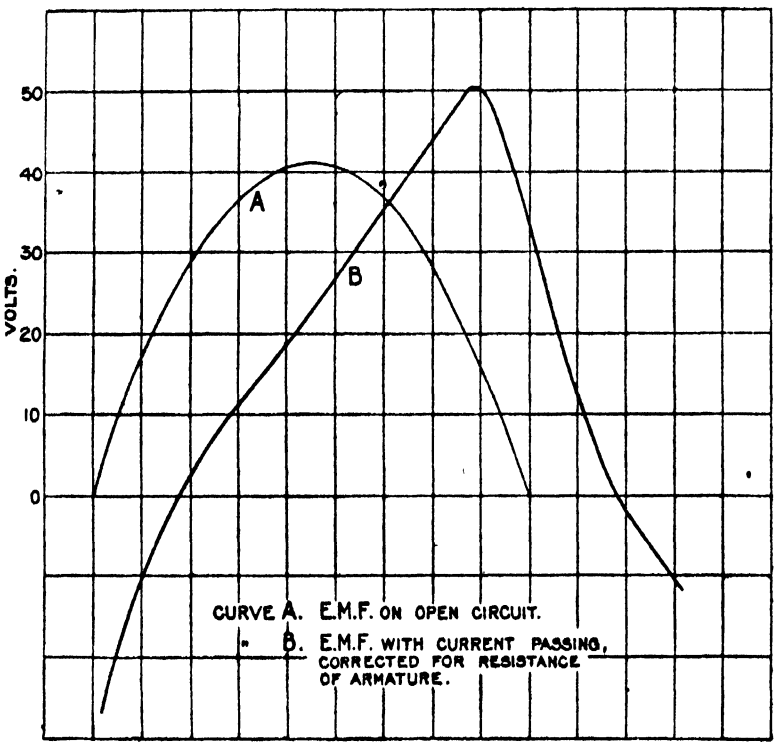

VOLTS.
50
40
30
20
10
0
A
B
CURVE A. E.M.F. ON OPEN CIRCUIT.
B. E.M.F. WITH CURRENT PASSING, CORRECTED FOR RESISTANCE OF ARMATURE.

Fig. 2

potential of from 94—98 volts $\sqrt{(\text{mean}^2)}$ with a frequency of 50 per second. The impressed E.M.F. is also approximately a sine-curve. The resistance and reactance (Lp) of the transformer, mains to the power house, etc., were determined from the drop in pressure at the terminals when giving a heavy non-inductive and inductive current respectively. The resistance was equivalent to about 0·1 ohm in the motor circuit, the reactance to about 0·12 ohm. The pole-pieces of the machine are laminated and cast into a solid yoke.

The pole-pieces are of $2\frac{1}{2}$-inch square section, and $3\frac{1}{2}$ inches deep from the pole-face to where they are set into the yoke. The diameter of the armature is $5\frac{1}{2}$ inches.

Rough determinations were made of the various constants of the machine; the resistance measured in the ordinary way was found to be 0·55 ohm cold. In normal working the resistance averaged about 0·7 ohm, making a total of 0·8 ohm with the resistance in the mains. The self-induction L, or that part of the induction which is linked with the armature but not with the field coils, was measured by observing the drop of potential across the armature when passing an alternating current, the field coils being short-circuited so as to eliminate the induction linked with them. The apparent resistance was assumed to be $\sqrt{(L^2p^2 + \rho^2)}$. The result was that Lp varied from 0·65—0·93 ohm, according to the position of the armature. The mean value of Lp was taken as 0·8 ohm, making 0·92 ohm total reactance with the reactance in the mains. For the determination of κ the machine was run as a generator on various non-inductive resistances varying from nothing up to 3 ohms. It was assumed that if V be the open circuit potential, C the current [$\sqrt{(\text{mean}^2)}$ in each case] and R the total resistance including armature, then $V = C\sqrt{[(L + \kappa)^2p^2 + R^2]}$. So determined the value of $(L + \kappa)\ p$ was found to range from 1·3 ohms in the first case (short-circuit) to 1·1 ohms in the second.

This method of measurement is only strictly accurate if $\kappa = \lambda$, in which case the value of κ should appear constant for all resistances. For present purposes we may take the higher value 1·3 ohms (which is the more nearly accurate) as a good enough approximation for $(L + \kappa)\ p$, or 1·42 ohms if the additional reactance of the mains is included, and 0·5 ohm for κp.

The power required to drive the motor unloaded against friction, hysteresis, etc. (taken as the applied watts less the C^2R losses in the armature), was found to average about 250 watts. This is the value of $\frac{1}{2}Ap\beta_0$ in all cases when the motor is described as unloaded.

Finally the value of μ was roughly determined in the following way: The current was passed through the field coils, the machine being at rest and the circuit of the armature open. The field coils were short-circuited at a definite instant of time, and the current in them was measured at various times after the short-circuit took place. The short-circuiting was effected by means of a long heavy pendulum which closed

a switch at the lowest point of its swing. The pendulum carried a contact maker which at a determinate later point in the swing connected a resistance included in the field circuit to an electrometer. The fall of potential in the resistance gave the current at the moment of making contact, and the time could be calculated from the length of the pendulum swing between closing the switch and making contact. It was found that with sufficient accuracy for present purposes the value of the current in the field magnets was halved in 0·07 second. Since in this case the current diminishes according to the law $e^{-t/\mu}$, it follows that μ is about one-tenth of a second; μ varies somewhat with the temperature of the field coils, etc., one-tenth can be taken as its order of magnitude.

Taking the above values of the constants, it appears that with the motor connected direct to the mains γ is negative. Furthermore, unless the machine is very much under-excited, we have

$$\frac{pp\,(A - \lambda\alpha_0)}{L\,(L + \lambda)\,p^2 + \rho^2} = 30\sqrt{(2)} \text{ amperes, roughly,}$$

taking $60\sqrt{(2)}$ volts as a rough value for $(A - \lambda\alpha_0)\,p$, ρ as 0·8 ohm, and $\sqrt{[L\,(L + \lambda)]}\,p$ as 1 ohm, so that γ' is also negative unless β_0 has a value comparable with $30\sqrt{(2)}$ amperes. We should, therefore, expect this machine to be violently unstable. This was in fact found to be the case; when connected direct to the mains as a motor with an exciting current of thirty or more the machine very rapidly developed oscillations, and finally dropped out of step altogether. The oscillations occurred just the same, whether a large or small generator was working at the Power Station. They were not affected by putting heavy rings of copper round the pole-pieces in addition to the field winding. It was only by reducing the field current to 13·5 and loading the machine with a brake until the applied power was about 1800 watts that the "hunting" could be stopped.

NOTE (added June 28th, 1903). That the hunting in this case was principally due to the negative value of γ' was shown by the fact that it was not possible to stop the hunting by the introduction of external resistance. Even with a resistance amounting to 2·5 ohms, with which ρ would certainly be greater than $(L + \kappa)\,p$, the motion was still unstable. The impossibility of making the motion stable by adding resistance puzzled me considerably at first, as I had not noticed the importance of the negative term in γ', but had treated it as a negligible quantity.

By inserting self-induction in series with the motor the violence of the "hunting" could be gradually diminished, and at the same time its period could be continuously increased from about 0·35 second to 0·45 second. When the self-induction was such that Lp amounted to about 6 ohms the motion became stable, the oscillations when once started rapidly dying away. This is in accordance with conclusion (1) above, and the continuous variation of the period though the hunting continues is proof of the essential instability of the motion.

With external self-induction giving a total reactance of about 3 ohms and resistance of 1·2 ohms the stability depended on the load and on the exciting current. The following observations were made:

(*a*) Exciting current 11·0, motor unloaded. The motion was stable, oscillations if started dying slowly away. Period of oscillation 0·42 second.

(*b*) Exciting current was increased to 18·5 when the motion became unstable. By loading the motor slightly with a brake the "hunting" could be stopped. Period 0·38 second.

(*c*) Exciting current increased to 30·5. The motion with the motor unloaded became violently unstable. By loading the motor with a brake until the power delivered to motor (excluding external self-induction) amounted to 1000 watts, the "hunting" could be stopped. It was curious to watch the effect of the load on the current taken by the machine. The ammeter had such a slow period that it could not follow the variations in current due to the oscillations, but remained steady, giving the average of the square of the current. When the machine was "hunting" the ammeter showed a current of about 27 amperes, and the wattmeter an applied power of about 710 watts, two-thirds of which were, of course, accounted for by the C^2R losses. On putting the load on, the current as shown by the ammeter gradually diminished until when the motion was steady it was 13·5 amperes. The power indicated by the wattmeter was now again about 1000 watts, but this time only about one-eighth was going in C^2R losses.

The last series of experiments is a good confirmation of conclusions (2) and (5) above. It is worth while in this case, the conditions of which approximate to those assumed in the theory, to determine the order of magnitude of γ and γ'.

The following measurements were made:

Total reactance (Lp) varied between 2·81 and 3·18 ohms, according to position of armature. Its value may be taken as constant, and equal to 3 ohms without serious error.

Total resistance (ρ) = 1·2 ohms.

In case (*c*) the potential across the motor terminals (that is excluding the external self-induction) was 75 volts. Hence, Ap is less than $75\sqrt{(2)}$ volts. On the other hand, Ap is greater than $\sqrt{(2)}$ times the open-circuit potential of the machine with exciting current 30·5, which was measured and found to be 65 volts. Take, therefore, Ap as equal to $70\sqrt{(2)}$. The power supplied to motor terminals (again excluding the external self-induction) was 1000 watts. The current was 13·5 amperes, hence the C^2R loss in armature is 110 watts (armature resistance taken as 0·6 ohm), and we have

$$\tfrac{1}{2}Ap\beta_0 = 890 \text{ watts},$$

and

$$\beta_0 = 12{\cdot}7\sqrt{(2)} \text{ amperes}.$$

Thus we obtain

$$4M\gamma p^2 = \frac{\rho A^2 p^2}{(L+\kappa)^2 p^2} = \frac{1{\cdot}2 \times (70\sqrt{2})^2}{(3{\cdot}5)^2} = 960.$$

(See formulæ 11 above.)

For γ' the more accurate formula (9) must be taken, since, as in all experiments with this motor, the resistance term is important. The formula may be written with sufficient accuracy

$$4M\gamma' p^2 = \frac{\kappa\mu p^2 \left[\beta_0{}^2 - \frac{p^2 A^2 \rho^2}{(L^2p^2+\rho^2)^2}\right]}{\mu^2\delta^2 + (1+\kappa/L)^2}.$$

We take $\mu = \frac{1}{10}$ sec., $2\pi/\delta$ (the period of oscillation) = 0·36 second, $\frac{2\pi}{p} = \frac{1}{50}$ second. Hence δ is 17 and $p = 314$.

We have $$\frac{Ap\rho}{L^2p^2+\rho^2} = \frac{70\sqrt{(2)} \times 1{\cdot}2}{10{\cdot}4} = 8\sqrt{(2)} \text{ amperes.}$$

Hence $$4M\gamma' p^2 = \frac{0{\cdot}5 \times 0{\cdot}1 \times 314\,[(12{\cdot}7)^2 - (8)^2] \times 2}{[(0{\cdot}1)^2 \times (17)^2] + (1{\cdot}2)^2} = 700.$$

This value is, of course, very rough; the most that can be asserted is that γ' is a positive quantity of the same order of magnitude as, and probably somewhat less than γ. The motion is only just stable with this load, however, and can be made unstable by a very small reduction of β_0, and I think the figures are a proof that there is an element of instability other than a possible negative value of γ'.

To get an approximation to the absolute value of γ, we note that

$$\delta^2 = \frac{A^2(L+\kappa)p^2}{2M\{(L+\kappa)^2p^2+\rho^2\}}, \text{ nearly.}$$

Hence

$$4Mp^2 = \frac{2A^2p^2(L+\kappa)p^2}{\{(L+\kappa)^2p^2+\rho^2\}\delta^2} = \frac{4 \times (70)^2 \times 3{\cdot}5 \times 314}{13{\cdot}6 \times (17)^2} = 5500 \text{ nearly,}$$

and $$\frac{1}{\gamma} = \frac{4Mp^2}{960} = \text{about } 5{\cdot}7 \text{ secs.,}$$

which is the sort of magnitude one would expect from the rate of development of the oscillations when the motor is unloaded and γ' is small or slightly negative.

NOTE (added June 28, 1903). It is useful to consider the effect of increasing the dimensions of the machine on the results here obtained. Suppose that the linear dimensions of every part except the field coils are increased n-fold, that the speed remains the same. Then

A	becomes	n^2A,
ρ	,,	ρ/n,
L	,,	nL,
κ	,,	$n\kappa$,
M	,,	n^5M

The rated output is multiplied by between n^3 and n^4. To get the same magnetization we require n-times the ampere turns in the field coils. Hence if the number of turns in the field coils and the current density be kept the same, we require n-times the section of wire, and the wire is n-times as long. Since all the other linear dimensions of the coils and of the magnetic circuit are increased n-fold, it readily follows that μ (if the magnets are completely laminated and there are no amortisseur coils) is increased n-fold.

Taking these values, we find that

$$\delta\left[=\sqrt{\left(\frac{A^2}{2M(L+\kappa)}\right)}\right] \text{ becomes } \frac{\delta}{n},$$

$$\gamma\left(=\frac{\rho A^2}{4M(L+\kappa)^2 p^2}\right) \quad \text{,,} \quad \frac{\gamma}{n^4}.$$

Hence $\mu\delta$ is unaltered and the damping term

$$\gamma' = \frac{\kappa\mu\beta_0{}^2}{4M\left\{\mu^2\delta^2 + \left(1+\frac{\kappa}{L}\right)^2\right\}}$$

becomes $n\gamma'$, assuming that corresponding values of the current are as $n^2:1$. As a fact corresponding values of the current (that is, values which are the same fraction of the maximum rated output) are in a somewhat less ratio than $n^2:1$, so that γ' increases but very little with n.

It appears, therefore, that the coefficient of instability decreases very rapidly with increasing size, while the damping coefficient γ' increases somewhat. At the same time, owing to the rapid decrease of ρ/Lp, the critical value of the load at which γ' becomes negative, rapidly becomes smaller in relation to the output of the machine. It may be inferred that a machine similar to that experimented with and but very little larger, would run stably at practically all loads.

In actual practice the dimensions of a pair of adjacent poles and of a corresponding piece of armature in a section perpendicular to the axis are rarely more than two or three times those on the machine here experimented with even in big alternators; the increased output is obtained by increasing the number of poles and by increasing the length of the machine. The number of poles and the length of the machine, provided the peripheral speed remains the same, have but little effect on the quantities γ, γ' and δ. In other words the performance of a machine as regards hunting is determined almost wholly by the form and dimensions, in a section perpendicular to the axis, of a pair of poles and the corresponding bit of armature. The weight of a corresponding bit of flywheel (if there is one) must, of course, be added to that of the bit of armature. Thus it is quite conceivable that machines of large size, but with small armature reaction and self-induction, might be constructed in which the quantity γ would be important and the running unstable, at any rate at low loads, though if the magnetic circuit of the machine were similar to that of the one experimented on (in which the self-induction and armature reaction are pretty high), and of double its linear dimensions or more, the motion would undoubtedly be stable.

3

THE PARALLEL WORKING OF ALTERNATORS.

Read before the British Association, Sept. 1903.

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THE question of the parallel working of alternating-current dynamos, under which term may be included the working of synchronous motors and of rotary converters, has been much discussed during the 20 years which have elapsed since it was first shown that it could be done. No very definite result, however, has been arrived at from the theoretical point of view, though engineers have evolved various empirical rules of design, and have produced the well-known "amortisseurs," in consequence of which the difficulties of parallel running have ceased to be serious, and the theoretical problem has lost much of its importance. Of late, however, a new interest has been given to the discussion of this question by the introduction of large gas engines, with which it is more difficult than with steam to satisfy the requirements of the dynamo designers as to the uniformity of angular velocity necessary for satisfactory running. In this Paper I do not propose to add much new matter to the discussion, but rather to attempt to coordinate the mathematical treatment of the subject with the practical rules which have been framed, and which are now regarded as good standard practice, and to point out the reason for certain phenomena which have been observed. It would appear that hitherto the mathematical treatment of the subject has been carried out without sufficient reference to practical conditions, and on the other hand it is possible that the empirical rules of practical engineers, though their success is a sufficient guarantee of their correctness under the conditions to which they have been applied, may not be fully applicable without modification to the new conditions which have followed the introduction of the new types of prime mover.

Dynamically, the question of the parallel working of alternators, or the running of a synchronous-motor or converter, is a problem of oscillation about a state of steady motion. Consider an alternating dynamo connected to a source of supply of constant amplitude and periodicity, and driven by a constant power or working against a constant resistance. For simplicity of expression we may suppose that the dynamo, and the generators giving the supply, have all the same number of poles. Then the dynamo will run with the same angular velocity as the supply generators, but with a certain difference of phase which we may call ϵ dependent on its excitation and on its load or driving power. This is the state of steady motion. If that state be disturbed—say by temporarily varying the load

on the dynamo if working as a motor—the dynamo will make oscillations about it, sometimes running ahead of and sometimes lagging behind the position of steady motion. When ahead of that position it will take less, and when behind it will take more, than its proper supply of energy from the mains.

The practical problem is to keep these oscillations, with their accompanying fluctuations in the flow of energy to or from the mains or 'bus bars, within moderate limits.

The oscillations may be either free or forced. To clear the ideas we may consider the case of a dynamo working as a synchronous motor, but the results are immediately applicable to the case of a generator. Suppose that the load on the motor is slightly increased. The motor will take up a position in which the phase difference between it and the generators is rather greater than before, becoming, say, $\epsilon - \xi$ where ξ is reckoned positive in the direction of motion of the motor. The torque developed by the motor which was T, is now $T - \xi \frac{dT}{d\epsilon}$, or, say, $T - c\xi$. The constant c is readily calculable from the electrical constants of the machine. If now the additional bit of load $- c\xi$ be removed there will be a balance of torque of that amount available for accelerating the motor, which, accordingly, will proceed to catch up, so to speak, the state of steady motion. After a time it will arrive at the position of steady motion corresponding to torque T (for which, of course, $\xi = 0$), but will then be moving with more than the steady motion velocity. In consequence of the difference of velocity, the motor will not exert quite the steady motion torque T, but there will be a balance of torque tending to accelerate it or retard it. This, if the difference in velocity is small, will be proportional to that difference and equal, say, to $b \frac{d\xi}{dt}$. Adding the effect of this and of the term $c\xi$, we find the equation of motion of the motor

$$M \frac{d^2\xi}{dt^2} + b \frac{d\xi}{dt} + c\xi = 0,$$

where M represents on a suitable scale its moment of inertia. Under a rigorous mathematical treatment, differential coefficients of higher order will appear, but the terms here given are the most important.

The solution of this differential equation is

$$\xi = \xi_0 e^{-\frac{b}{2M}t} \sin(\delta t + \eta),$$

where $\frac{2\pi}{\delta}$ is the period of the oscillation and δ is equal to $\sqrt{\frac{c}{M}}$ nearly, if (as is usually the case) b^2 is small compared with $4cM$.

The oscillations represented by this expression are the free oscillations executed by the dynamo when disturbed by a temporary change of conditions—*e.g.*, if its load be suddenly thrown off, or if, being a generator,

it be switched into parallel with its fellows. Dr Hopkinson showed many years ago that the quantity c is positive—in other words, that if the state of steady motion be disturbed the forces so called into being tend to restore it, or the motion is stable. Since then Kapp and others have worked out the value of c. It is easy to show that, sufficiently nearly for all practical purposes,

$$c = \sqrt{\frac{A^2}{2L}},$$

where $\frac{Ap}{\sqrt{2}}$ is the open circuit E.M.F. of the dynamo ($\sqrt{\text{mean}^2}$) ($2\pi/p$ being the periodic time of the alternations) and L the self-induction of the armature, including therein the armature reaction, and the self-induction of any conductors in series with the armature. Another expression for c, at once deducible from the above, is $c = \sqrt{\frac{Ei_0}{p}}$, E being the open circuit E.M.F. and i_0 the current in the machine when short-circuited and driven at the proper speed.

It is obvious that the importance or otherwise of these free oscillations depends on whether they are damped out or not. It has usually been assumed that b is a positive quantity—*i.e.*, that the oscillations are resisted by a sort of viscous force, and if that is so the oscillations will, of course, rapidly disappear. I have, however, recently shown that under certain circumstances b may be negative, in which case the oscillations will continually increase in amplitude and the running of the dynamo will be unstable, even though b be very small.

Suppose that the dynamo is working as a generator and is driven by a force with a certain periodic variation—*e.g.*, by a reciprocating engine. It is supposed to be coupled in parallel with such a number of other machines that the 'bus bar potential is practically constant. The equation of motion then is

$$M\frac{d^2\xi}{dt^2} + b\frac{d\xi}{dt} + c\xi = P\sin\delta' t,$$

where $2\pi/\delta'$ is the period of the variation in the force. The solution of this equation is (leaving out the free oscillations)

$$\xi = \frac{P}{\sqrt{b^2\delta'^2 + (c - M\delta'^2)^2}}\sin(\delta' t - \eta),$$

where

$$\tan\eta = \frac{b\delta'}{c - M\delta'^2}.$$

I shall presently show that in practical cases $b\delta'$ is small compared to c, so that the equation for ξ is, unless c is nearly equal to $M\delta'^2$,

$$\xi = \frac{P}{c - M\delta'^2}\sin\delta' t$$

$$= \frac{P}{c\left(1 - \frac{\delta'^2}{\delta^2}\right)}\sin\delta' t,$$

where $2\pi/\delta$ represents, as before, the period of the free oscillations. These are the forced oscillations produced by uneven turning moment in the prime mover. Superposed on them there will be the free oscillations already considered.

The possibility of parallel working, or of the working of a synchronous motor, therefore depends on two conditions. First, the quantity b must be positive, so that any free oscillations are damped out within a short time of being set up. Second, the forced oscillations must be of such moderate amplitude as not to interfere with practical working. I will deal first with the last-named condition, as it admits of much simpler treatment than the other.

For an ideal machine, without any flywheel effect at all, δ is infinite, and we have $\dot{\xi} = \frac{P}{c} \sin \delta' t$. In this case, the phase displacement is always such as to correspond exactly with the torque exerted by the prime mover. Such a machine would run fairly well in parallel with others, simply delivering to the 'bus bars at all times exactly the energy given to it by its engine. But now if a flywheel be added, δ becomes finite, and so long as it remains greater than δ', that is so long as the natural period of the alternator is less than that of the variation in turning moment—the amplitude of the oscillations becomes greater with increase of flywheel until, when $\delta = \delta'$, there is perfect resonance, and the oscillations are only restrained by the viscous damping. Further increase in the flywheel effect results in a continuous diminution of the oscillations. If δ is so far diminished by increasing the flywheel that δ' is several times as large as δ, the formula for ξ becomes

$$\xi = -\frac{\frac{P}{c\delta'^2}}{\delta^2} \sin \delta' t$$

$$= -\frac{P}{M\delta'^2} \sin \delta' t.$$

Now, the practical rule always adopted by designers for working in parallel is that the angular displacement of the machine in a revolution when running unloaded as compared with a uniformly revolving wheel shall not exceed a certain fraction (varying from $\frac{1}{140}$ to $\frac{1}{70}$) of the angular pitch of the poles. If the machine is unloaded the equation of motion is

$$M \frac{d^2\xi}{dt^2} = P \sin \delta' t,$$

or

$$\xi = -\frac{P}{M\delta'^2} \sin \delta' t.$$

ξ is the phase-angle of displacement measured in such units that the angle between a pair of adjacent poles (or pitch-angle) is π. The practical rule, therefore, directly limits the angular amount of the forced oscillations, provided that δ' is large compared with δ, or that the natural period of

oscillation of the alternator is large compared with the period of variation in turning moment.

Limitation of the angle by which the machine deviates from perfectly uniform rotation is not, however, precisely what is needed. What should be limited is rather the amount of the fluctuation in the rate at which energy is delivered by the machine to the 'bus bars. This, since the speed is practically constant, is proportional to the variation of torque—that is, to $c\xi$. Now, c is equal to $\sqrt{\frac{A^2}{2L}}$, and depends upon the electrical properties of the machine. Hence a machine with small self-induction will operate worse in parallel under a given amount of angular displacement than will another of large self-induction, always supposing the above-mentioned condition of long natural period to be fulfilled. Here, therefore, is a sufficient explanation of the wide variation in angular deviation permitted by different designers; what suits one machine will not suit another, and a machine with small self-induction and armature reaction, or what is called a machine with good inherent regulation, must be subject to more stringent rules as regards angular deviation than one with large self-induction.

Another cause which makes a fixed rule as to angular deviation inapplicable to all cases, is the fact that in some designs the natural period of oscillation may not be long, compared with the period of the engine impulses. Should these two periods become even approximately equal—if, for example, δ were anywhere within 20 per cent. of δ', then the rule quite ceases to be applicable, and the amount of angular deviation in actual working as a generator bears no fixed relation to, but is much greater than, the same deviation when unloaded. As a matter of fact, in machines as now designed, in which the self-induction and armature reaction are usually sufficient to allow the machine to be short-circuited when partially excited without damage, the natural period of oscillation, even without a flywheel is usually greater than the period of engine impulse. The latter period, even in a slow-speed engine, will not, as a rule, exceed one-third of a second, while the natural period will be found to be of the order of half a second or more, and very much greater with a heavy flywheel. Probably, therefore, a designer will generally be justified in assuming that the two periods are sufficiently different to ensure that he will get about the same angular deviation when delivering energy to the 'bus bars as when the machine is running disconnected. He will, however, have to take account of the change in the rate of flow of energy to which that deviation corresponds on his particular machine, and it is in neglecting that factor that the practical rule seems to be illogical.

We have, so far, considered that particular component of variation in turning moment which is responsible for the major part of the phase displacement when the machine is running disconnected, and which is, accordingly, alone regarded in the practical rule for flywheel effect. That,

however, is not in fact the only component of the variations; there will be other components of both longer and shorter period. Take for example the case of a single-cylinder double-acting Körting gas engine running at 90 revolutions. The principal variation in turning moment, and the only one producing any appreciable effect on the phase displacement when running light, will be due to the explosions, and will have a period of one-third of a second. As stated above, the natural period of oscillation of the alternator will probably be much greater than this. But there will also be a variation in turning moment due to the weight of the crank, with a period the same as that of the revolution of the engine, or two-thirds of a second. Now it is not impossible, with a light flywheel, for the period of oscillation of the alternator to have that value, and if that were so violent hunting would be set up, even though the variation in turning moment due to this cause were too small to produce an appreciable effect on the phase displacement when running light. Only, in this case of a small disturbing force, the approximation of periods must be fairly close to produce serious effects. The ordinary rules as to flywheel effect generally result in the natural period of oscillation being substantially greater than the period of revolution of the engine, but in a somewhat roundabout and accidental way. It seems highly desirable on many grounds to pay more attention than has hitherto been usual to the natural period of oscillation of the alternator when specifying flywheels. The period of oscillation of the governor of the engine should not be lost sight of; this is generally fairly long, and may not impossibly give rise to resonant oscillations.

So far, we have considered the forced oscillations caused by the uneven turning moment of the prime mover in a generator working in parallel with a number of others. A very similar, and equally important problem, is that of a synchronous motor or rotary driven from mains in which the potential varies periodically owing to unevenness of rotation in the supply generators. Here, if the motor or rotary had no flywheel effect at all, it would follow the generators precisely in their oscillations, and it is easy to see that if ξ_0 be the maximum phase displacement in the generators and ξ_0' that in the rotary, then

$$\xi_0' = \xi_0 \Big/ \left(1 - \frac{\delta'^2}{\delta^2}\right),$$

where $2\pi/\delta'$ is the period of the variation in the generators and $2\pi/\delta$ the natural period of oscillation of the rotary. In the case of a rotary, however, it is unusual to have any flywheel, and consequently δ, though usually less than δ', may be sufficiently near it to cause the oscillations of the rotary to exceed in amplitude those of the generator. Furthermore, the danger of actual resonance with the revolution of the engine is in this case very much greater. I came across a case recently of a motor-generator which could not be worked owing to resonance. Its natural period happened to be very nearly equal to that of the revolution of the generator—about

two-thirds of a second—and it dropped out of step in a very short time, though the variation of turning moment of the period in question must have been quite small. By altering the field current so as to increase the natural period about 25 per cent. the forced oscillations disappeared almost completely, showing that the difficulty was entirely due to resonance. This suggests the reflection that it might possibly be better in some cases to put flywheels on the comparatively high-speed rotaries, and allow a small flywheel and a bigger angular deviation in the slow-speed generators.

The question of free oscillations is a very much more complicated and difficult one than that of forced oscillations. The period is, of course, easily determined, but, as already indicated, the importance or otherwise of these oscillations depends almost entirely on the way in which they are damped, or on the sign of the quantity b. If b be positive, the oscillations are damped out and are unimportant. On the other hand, if b be negative, even though very small, the oscillations will continually increase according to an exponential law, and the running of the machine will be impossible as a practical thing. The stability or otherwise, therefore, depends in this case on the sign of a small quantity and may be affected by a very small change in the conditions. I have recently investigated this question, and I have shown that under certain circumstances* b may be negative. I will not reproduce the mathematics here, but will merely give the conclusions.

Consider an ideal synchronous motor in which the exciting current is maintained absolutely constant, while the field magnets and the armature core and conductors are so perfectly laminated that no appreciable eddy currents flow therein. I find that the motion of such a motor is unstable or b is negative, provided that $Lp > \rho$. Here ρ is the resistance and Lp is the reactance of the armature and any conductors in series with it. The actual value of b I find to be

$$-\frac{1}{2}\rho A^2 \frac{L^2p^2 - \rho^2}{(L^2p^2 + \rho^2)^2}.$$

The condition $Lp > \rho$ is generally fulfilled, so that an ideal motor such as that described is in general unstable, and all motors have this element of instability. In actual machines, however, there are a number of causes which give rise to real viscous forces, and generally overcome this tendency to instability. Such are the local currents induced in the substance of the armature conductors, and (generally but not always) the "amortisseur" effect of currents induced by the oscillations in the pole-pieces, the field coils, and in any special "amortisseur" coils put there for the purpose. It will be found on working out the value of b, that in machines of modern design and any considerable size, it is insignificant under ordinary conditions of working. In small machines, however, and in large ones on long transmission lines it may be important. I have myself experimented with a small motor, the running of which was actually unstable from this cause.

* Paper read before the Royal Society, June 18, 1903. See *The Electrician*, August 7, 1903.

Of the causes mentioned above as making for stability by producing a viscous damping effect, the most important is that described as giving rise to "amortisseur" action. Consider an ideal motor, of the type described in the last paragraph coupled to mains giving constant alternating potential, moving steadily and doing external work. For simplicity we may suppose the open-circuit potential of the motor to be equal to that of the mains. The effect of the armature current on the field will be to decrease the induction in the field magnets somewhat and also to distort it, so that the point of maximum induction and zero back E.M.F. is displaced backward against the direction of rotation. Suppose, now, that the motor executes oscillations about the position of steady motion. Then the field will experience variations of like period both as regards its magnitude and its distortion. To put the matter in symbols, suppose that $A \cos\theta$ is the induction linked with armature and field coils in the steady motion (θ being an angle defining the position of the armature). Then in the oscillation which is given by $\xi = \xi_0 \sin \delta t$ the induction will be

$$(A + a_0' \sin \delta t) \cos\theta + b_0' \sin \delta t \sin\theta,$$

the quantities a_0' and b_0' being positive, since when ξ is positive there is a greater total induction and a less displacement of point of maximum induction than when ξ is zero.

Now suppose that the pole-pieces are surrounded by short-circuited copper conductors, and that the pole-faces are covered with copper plates, or have copper conductors threaded through them to form a grid. Then the changes of magnetising force due to the armature current will cause induced currents to flow in these conductors, and the general effect of the currents will be to somewhat reduce the amplitude of the changes of induction in the pole-pieces, and to cause them to lag rather behind the changes in armature reaction which give rise to them. The consequence is that the induction linked with armature and field coils is now

$$\{A + a_0 \sin (t - \eta)\} \cos\theta + b_0 \sin (\delta t - \zeta) \sin\theta,$$

where a_0 and b_0 are (for the motor) positive quantities less than a_0' and b_0' respectively, and η and ζ are positive angles which depend upon the resistance of the "amortisseur" conductors and on the armature reaction. Thus, at the time when the armature is passing the position of steady motion ($\xi = 0$), but moving with more than the steady motion velocity, the induction, instead of having its normal value $A \cos\theta$, has the value $(A - a_0 \sin\eta) \cos\theta - b_0 \sin\zeta \sin\theta$. In other words, the currents then circulating in the "amortisseur" coils are such as to reduce the magnetization somewhat and to displace the point of maximum induction or zero open-circuit E.M.F. rather behind the position which it would occupy in ordinary steady motion. We have now to consider the effect of these changes in the induction on the torque at this moment, since it is the then deviation of the torque from its steady value that determines the value of b.

First, as regards the demagnetisation, due to diminution of current in the field coils or to currents in short-circuited windings surrounding the pole-pieces. It is easy to show that the effect of reducing the field current in a synchronous motor may be either to increase or diminish the torque—it depends on the load on the motor. Let β_0 be that component of the current which is in phase with the back E.M.F., and Ap the back E.M.F. of the motor, so that the rate of working of the motor is $\frac{1}{2}Ap\beta_0$. Then a reduction in the field current will result in a reduced torque provided that

$$\beta_0 > \frac{Ap\rho}{L^2p^2 + \rho^2},$$

where, as before, ρ is the resistance, and Lp the reactance of the armature and any conductors in series with it. Similar considerations apply when the dynamo is working as a generator, only in this case the effect of the induced currents when $\xi = 0$ is to increase the magnetisation while the sign of β_0 is changed. The general result is that if β_0 be outside the limits $\pm \frac{Ap\rho}{L^2p^2 + \rho^2}$ the induced currents tend to damp out the oscillations. On the other hand, if β_0 be within these limits, the induced currents tend to increase the oscillations, and under such circumstances damping coils surrounding the pole-pieces are additional causes of instability. In the case of a machine connected direct to 'bus bars in parallel with others, these limits for β_0 are very narrow; but in motors or converters on long transmission lines they may be so far apart as to give rise to instability at light loads. There is little doubt that trouble has been experienced from this cause. It was one reason for the instability of the small motor to which I have referred, and the constants of that motor might easily be repeated on a much larger machine if there were considerable self-induction and resistance in circuit with it.

We may now consider shortly the damping effect of solid pole-pieces, copper plates over the pole-faces, or copper bars threaded through the pole-face to form a grid. As already pointed out, the effect of the currents in such conducting bodies at the moment when the motor is in the position of steady motion, but moving with more than the steady motion velocity, is to so distort the field as to move the position of maximum induction somewhat behind that which it occupies in steady motion. This is in effect the same as reducing the phase difference between motor and generators, and it therefore in all cases results in a reduction of the torque of the motor as compared with the normal steady motion torque, or on the balance in retardation of the motor. Hence damping coils of this kind invariably exert a true damping or viscous effect. Moreover, I have worked out the amount of the effect, and find that it is much greater than that due to the variation of current round the pole-pieces. It is these conductors across the face of the pole-pieces that are the really useful "amortisseurs," and by their use it would seem that b can always be made positive and the motion stable.

I do not propose here to go into the actual value of the damping effect produced by these "amortisseur" coils. One thing, however, may be noted here, and that is, that as the resistance of the coils is diminished, the effect increases to a maximum and then diminishes. It is obvious that if the pole-pieces were surrounded by perfectly conducting coils and faced with perfectly conducting plates there could be no changes of induction produced by an oscillation about steady motion, and, therefore, no damping effect. The point is that the coils diminish the amplitude of the changes of induction produced by armature reaction approximately in proportion to their conductivity, and at the same time give rise to a difference of phase between those changes and their cause. The amount of damping effect is proportional to the amplitude of the changes and to the phase difference. It is therefore possible to increase the conductivity of the coils too far—that is, to such an extent that the diminished amplitude more than compensates the increased phase difference. Whether this happens in practice I am not prepared to say, as it depends on many elements, such as the period of oscillation, amount of armature reaction, etc., but I am clear that it is not impossible with constants such as one meets with in ordinary designs.

One other point is of interest in connection with "amortisseur" coils. They have been regarded here as a means of making b positive, or of removing that tendency to instability as regards free oscillations which is inherent in all machines. Of their great value for that purpose there can be no doubt whatever. It has often been suggested that they also play an important part in reducing the forced oscillations which were considered in the first section of this Paper. My own belief, however, is that their action in that respect is insignificant. Looking back for a moment to the expression for the forced oscillations, damping being taken into account, we found that it was

$$\xi = \frac{P}{\sqrt{b^2\delta'^2 + (c - M\delta'^2)^2}} \sin(\delta' t - \eta)$$

while the free oscillations were

$$\xi = \xi_0 e^{-\frac{bt}{2M}} \sin(\delta t + \epsilon).$$

Now, by the courtesy of the Westinghouse Company I have been able to observe the free oscillations on a rotary converter fitted with heavy dampers round the poles and across their faces. The design of these coils was the outcome of much experience, and they may probably be taken as giving the most powerful attainable damping effect. I found that if the machine were paralleled when a little out of phase one could hear it make six or eight complete oscillations. There can be no doubt that the ratio of two successive oscillations was much greater than $1/e$. Assuming it to be equal to $1/e$, we readily find that

$$\frac{b}{2M} \cdot \frac{2\pi}{\delta} = 1.$$

Hence in the expression for the forced oscillation

$$b\delta' = \frac{\delta \cdot \delta' M}{\pi}.$$

Now I have already pointed out that the effect of the ordinary flywheel rules is to make δ considerably less than δ'. Hence $b^2\delta'^2$ is but a very small fraction of $M^2\delta'^4$, and unless c is nearly equal to $M\delta'^2$—that is unless there be resonance—the forced oscillations are almost unaffected by the damping. If there be resonance we have $\delta' = \delta$ and

$$b\delta' = \frac{M\delta^2}{\pi} = \frac{c}{\pi}.$$

Hence the amplitude of the forced oscillations is

$$\xi = \frac{P}{b\delta} = \frac{P\pi}{c}.$$

In other words, in case of resonance the forced oscillations would be about three times as big with this amount of damping as they would if there were no flywheel effect at all and no damping. It would seem, therefore, that damping coils are not of much use for dealing with cases of resonance; a slight variation of the period is a much better solution. To sum the whole matter up, the proper way to deal with forced oscillations is to use the right flywheel, while damping coils must be used for giving stable free oscillations.

4

THE EFFECTS OF MOMENTARY STRESSES IN METALS.

["PROCEEDINGS OF THE ROYAL SOCIETY," Vol. LXXIV, 1905.
Communicated by Professor EWING, F.R.S.]

IN 1872 the late Dr John Hopkinson published an investigation into the effect of a blow delivered by a falling weight on the lower and free end of a wire, the upper end of which is fixed*. It is unnecessary to repeat the mathematical analysis in full, but its main features appear in the following argument: As soon as the weight strikes the stop at the lower end a wave of extension starts up the wire, and the velocity with which it is propagated is $\sqrt{E/\rho} = a$, where E is Young's modulus, and ρ the density of the wire. At a time t after the weight has struck, so short that its velocity is not appreciably diminished, the lower end of the wire has moved through a distance Vt, where V is the velocity of the weight immediately after striking. That is to say, the wire as a whole is lengthened by an amount Vt. This extension is felt over a distance at from the lower end, that being the distance through which the wave of extension initiated by the blow has travelled. The mean strain in this portion of the wire is therefore V/a, and the remainder of the wire is not extended. The wave now travels up the wire to the fixed end, and when it reaches there a reflected wave of equal amplitude starts down the wire. There results momentarily at the top end of the wire a strain equal to $2V/a$ with a corresponding tension $2EV/a$. This is the maximum tension experienced by any part of the wire until the reflected wave again reaches the lower end.

Each bit of the motion of the weight after striking contributes an element to the wave of extension, which is proportional to the then velocity of the weight. The weight is continually being retarded, and the amplitude of the wave therefore continually diminishes as you go back from its front.

In Fig. 1 the abscissae are distances measured from O, the free end of the wire; the ordinates are the strains, and any one of them, say $P'N'$, is equal to v/a, where v is the velocity which the weight had when the bit of wave at P' left it. If t be the time, reckoned from the moment of striking, $ON = at$, and it is easy to show that

$$P'N' = PNe^{-\frac{\mu}{M}.NN'} = \frac{V}{a}e^{-\frac{\mu}{M}(at-x)},$$

where μ is the mass of the wire per unit length, and M the mass of the weight. The wave of extension represented by the curve and its dotted

* *Original Papers*, Hopkinson, vol. 2, p. 316.

continuation travels up the wire without change of type to the upper end, where it is reflected, and a similar wave travels down the wire, the effect of which is added to that of the original wave. The strain at any point of the wire, such as N', is zero till the wave reaches it. The strain then becomes V/a, and gradually diminishes according to the exponential law $e^{-\mu at/M}$ until the reflected wave reaches N', when the strain increases by V/a again. Further reflection will occur at the moving weight, but in my experiments this is not considerable, and the maximum strain experienced at any point of the wire, at any rate in the upper half, occurs when the reflected wave reaches it. If x be the distance of the point from the *upper* end, the total strain due to the up-going and down-coming waves then is $(1 + e^{-2\mu x'/M})\frac{V}{a}$. The movement in space of any point N' before the reflected wave reaches it is equal to the area of the curve $PNN'P'$. For the point in question this is $\frac{MV}{\mu a}(1 - e^{-2\mu x'/M})$. The strain caused by the blow is added to any initial strain in the wire. When, as is usually the case, the

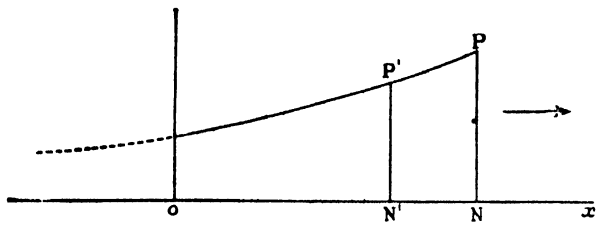


Fig. 1

wire is under tension at the moment of the blow, and the tension is released by the blow, the initial strain in the wire is somewhat diminished by the time the wave reaches the top end; superposed upon the extension caused by the blow there is a slight contraction due to the release of the tension at the lower end at the moment of striking. The ultimate result is that the total increase of length, caused by the blow, of a piece x' at the top end of the wire is

$$\frac{MV}{\mu a}(1 - e^{-\frac{2\mu x'}{M}}) - \tfrac{1}{2}\frac{T}{M}\left(\frac{2x'}{a}\right)^2, \qquad \text{.....(1)}$$

where T is the initial tension. In my experiments $2\mu x'/M$ is small, and its square may be neglected. The expression then becomes

$$2x'\frac{V}{a}\left(1 - \frac{\mu x'}{M}\right) - \tfrac{1}{2}\frac{T}{M}\left(\frac{2x'}{a}\right)^2. \qquad \text{.....(2)}$$

The second term is a small correction, but cannot in all cases be neglected. The piece of wire lengthens continuously as the wave passes over it, and begins to contract when the reflected wave arrives at its lower end. The extension then has the value given by expression (1). These results are all to be found in Dr Hopkinson's papers cited above, or follow at once

from the results there given; and so it does not seem necessary to repeat the proofs here.

In the same papers Dr Hopkinson gave the result of some rough experiments which went to confirm the principal conclusion from this analysis, namely, that the power of a blow to rupture a wire should be measured rather by the velocity with which it is delivered than by its energy or its momentum. It also appeared, as might be expected, from the mathematics, that the wire was most likely to break at the upper end.

In these experiments, made over 30 years ago, the only available means of estimating the momentary stresses produced by the blow was the effect they left upon the wire, *e.g.*, rupture. As the mathematical treatment proceeds upon the assumption that the stress and strain are everywhere and always proportional, it was not to be expected that it could give more than a very general indication of the impulse necessary to rupture the wire. With the appliances now available, however, I think that experiments on these lines are capable of yielding a good deal of information about the effect of stresses applied for a very short time, such as are met with in most cases of shock. The practical importance of such information need not be insisted upon.

I have, therefore, made some experiments of the same kind, but instead of rupturing the wire I have used blows which leave but little permanent extension. I have measured the momentary extension of a few inches at the top of the wire, and compared this with the extension as calculated from theory and given in expression (1) above. If the two agree, and if not much permanent extension is left, it is clear that the theory is correctly applied, and that the stresses in the material may be calculated from it. Moreover, we know that the material must be substantially elastic up to the maximum stress so calculated if applied for the time given by the theory.

The general result that I have obtained is that iron and copper wires may be stressed much beyond the static elastic limit and even beyond their static breaking loads without the proportionality of stresses and strains being substantially departed from, provided that the time during which the stress exceeds the elastic limit is of the order of 1/1000 second or less.

The wire was in each case of No. 10 gauge, and about 30 feet long; it was hung in a vertical chase in a wall, the upper end being firmly fixed in a block of iron, weighing about 20 lbs., the ends of which were built into the wall. This block carried a vertical steel rod, at any point of which could be clamped the contact-making device for measuring the momentary extension. The construction of this sufficiently appears from the figure. The light hard steel point A is fixed to the wire at a certain distance, usually 20 inches, from the upper end. The wire having been drawn taut preparatory to the experiment, the insulated spring S is pushed up by the

continuation travels up the wire without change of type to the upper end, where it is reflected, and a similar wave travels down the wire, the effect of which is added to that of the original wave. The strain at any point of the wire, such as N', is zero till the wave reaches it. The strain then becomes V/a, and gradually diminishes according to the exponential law $e^{-\mu at/M}$ until the reflected wave reaches N', when the strain increases by V/a again. Further reflection will occur at the moving weight, but in my experiments this is not considerable, and the maximum strain experienced at any point of the wire, at any rate in the upper half, occurs when the reflected wave reaches it. If x be the distance of the point from the *upper* end, the total strain due to the up-going and down-coming waves then is $(1 + e^{-2\mu x'/M})\,\frac{V}{a}$. The movement in space of any point N' before the reflected wave reaches it is equal to the area of the curve $PNN'P'$. For the point in question this is $\frac{MV}{\mu a}(1 - e^{-2\mu x'/M})$. The strain caused by the blow is added to any initial strain in the wire. When, as is usually the case, the

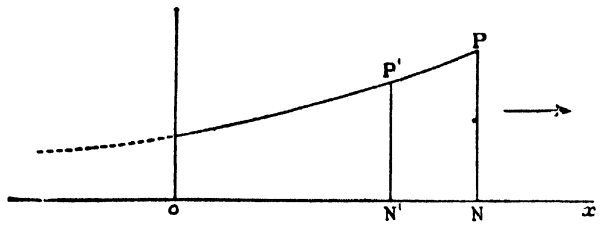


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I have, therefore, made some experiments of the same kind, but instead of rupturing the wire I have used blows which leave but little permanent extension. I have measured the momentary extension of a few inches at the top of the wire, and compared this with the extension as calculated from theory and given in expression (1) above. If the two agree, and if not much permanent extension is left, it is clear that the theory is correctly applied, and that the stresses in the material may be calculated from it. Moreover, we know that the material must be substantially elastic up to the maximum stress so calculated if applied for the time given by the theory.

The general result that I have obtained is that iron and copper wires may be stressed much beyond the static elastic limit and even beyond their static breaking loads without the proportionality of stresses and strains being substantially departed from, provided that the time during which the stress exceeds the elastic limit is of the order of 1/1000 second or less.

The wire was in each case of No. 10 gauge, and about 30 feet long; it was hung in a vertical chase in a wall, the upper end being firmly fixed in a block of iron, weighing about 20 lbs., the ends of which were built into the wall. This block carried a vertical steel rod, at any point of which could be clamped the contact-making device for measuring the momentary extension. The construction of this sufficiently appears from the figure. The light hard steel point A is fixed to the wire at a certain distance, usually 20 inches, from the upper end. The wire having been drawn taut preparatory to the experiment, the insulated spring S is pushed up by the

micrometer screw until contact is made with the point as shown by the deflection of the galvanometer. The spring is then withdrawn by the amount of extension expected; the blow is delivered and the galvanometer shows whether contact between the point and the spring has occurred or not.

By using a sensitive ballistic galvanometer without any resistance in series with it, it was found quite easy to determine the instantaneous

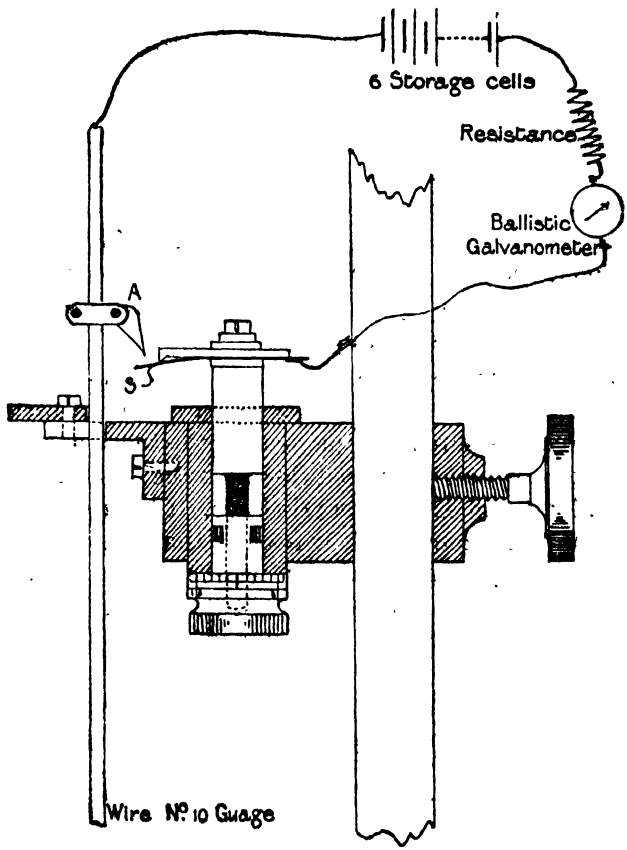


Fig. 2

extension of 20 inches of wire correct to 1/1000 of an inch, that amount of difference in the position at which the spring is set converting a big throw of the galvanometer into no deflection at all. In a few cases a second point was added with a similar contact spring close to the upper end of the wire, in order that any displacement of the wire relative to its support might be detected. But I found that if the wire was soft soldered into a bol about 3 inches long screwed into the block, this precaution was unneces sary.

The falling weight was a cylindrical piece of steel weighing 1 lb., with a hole drilled along its axis which was a loose fit on the wire. The wire was kept taut by a spring balance attached to its lower end, the tension in which could be varied from 20 to 200 lbs. The stop struck by the falling weight was a metal sleeve slipped over the wire and soldered to the lower end. It was made as light as possible so that the velocity of the weight should not be much diminished at impact. The velocity at impact was calculated from the height of fall, being taken as $\frac{M}{M+m}\sqrt{2gh}$, where M is the mass of the weight, and m that of the stop.

From time to time the permanent extension on the 20 inches was measured by again pushing the spring into contact with the wire when under steady tension. No analyses were made of the materials, because, at the present stage, all that I desire to do is to compare the effects in the same material of impulsive and of long continued stress. The nature of the material, however, sufficiently appears from the static tests.

Iron Wire. This was bought as iron wire, No. 10 gauge. Its diameter was 0·1275 inch. After placing it in the chase it was heated to redness and stretched, to straighten it. The annealing softened it materially, and somewhat unequally in different parts.

The following is a set of observations on this wire, which is typical:

Steady tension 50 lbs.—Height of fall 5 feet.

Contact point 20 inches from the upper end.

One division on micrometer head = 1/2000 inch = 1/40,000th part.

Micrometer reads 34·5 with a steady load of 50 lbs. when contact first made on the spring being pushed up.

Micrometer set at 110·5 (76 divisions extension). No contact when weight let fall.

Repeat the blow.—No contact. Repeat.—No contact.

Micrometer with steady load of 50 lbs. now reads 34·5.

Micrometer set at 109·5 (75 divisions extension). No contact.

Altered to 108·5. Contact occurred; the galvanometer spot went half across the scale.

Repeat.—Contact again.

Steady micrometer reading after this 36·0.

Micrometer set at 111 (75 divisions extension). No contact.

Repeat.—No contact. Repeat.—No contact.

Steady reading now 36·0.

Micrometer set at 110 (74 divisions extension). No contact.

Repeat.—Contact occurred.

Steady reading now 37·0.

Hence, the instantaneous extension in this case is 74 micrometer divisions, and the permanent extension produced by 11 blows is about 2·5 divisions.

Then followed a set of 4 blows with a 10-foot fall, and with 20 lbs. tension. They resulted as follows:

Steady reading 7·0.	Set at 110 (103 divisions).	Contact.
Steady reading 20·0.	Set at 125 (105 divisions).	No contact.
Steady reading 29·5.	Set at 132 (102½ divisions).	Contact.
Steady reading 42·0.	Set at 148 (106 divisions).	No contact.
Steady reading 52·5.		

Hence, the extension produced by the blow is probably between 103 and 105 divisions, and almost certainly between 102 and 106. Permanent extension produced by 4 blows = 45·5 divisions, or just over 1/1000th part.

Now static tests on this wire showed that a load of 390 lbs. extends it by 1/1000th part. Also μ its mass per foot is 0·0435 lb. Hence

$$a = \sqrt{\frac{E}{\rho}} = \sqrt{\frac{390{,}000 \times 32}{0{\cdot}0435}} = 17{,}000 \text{ f.s. nearly.}$$

The mass of the stop was 0·04 lb. Hence V, the velocity just after impact, is, with a 5-foot height of fall, 17·2 f.s. Also since $x' = 1{\cdot}66$ feet,

$$\frac{\mu x'}{M} = \frac{1{\cdot}66 \times 0{\cdot}0435}{1} = 0{\cdot}07.$$

Further, $T = 50$ lbs. $= 1600$ poundals, and it will be found that $\frac{1}{2}\frac{T}{M}\left(\frac{2x'}{a}\right)^2$ is about 0·4 thousandth of an inch, or 0·8 micrometer division. Substituting these figures in the expression (2) above, the extension on the 20 inches, as calculated, is 37·2 thousandths of an inch, or, say, 74½ micrometer divisions. The observed extension is 74 divisions, which is close agreement. This, coupled with the fact that the permanent extension produced on the 20 inches is negligible, is fairly conclusive evidence that the theory is applicable in this case, and that the material is almost perfectly elastic up to the highest stress as calculated from the theory. The maximum strain at the top of the wire is $2V/a$, or 34·4/17 thousandths. The corresponding tension is about 790 lbs. To this must be added the 50 lbs. steady tension, making a total of 840 lbs. as the maximum stress experienced by any portion of the wire. The mean tension in the top 20 inches, where the extension is greatest, is $\left(\frac{74}{40} \times 390\right) + 50$, or 770 lbs.

Now, after the completion of the experiments, the top 20 inches of the wire were cut out and tested statically with an Ewing's extensometer. There was perceptible failure of elasticity at 500 lbs., very marked yielding at 700 lbs. (which produced a permanent extension of nearly 1 per cent.), and at 800 lbs. the wire drew out very rapidly and finally broke.

With a fall of 10 feet and a steady tension (T) of 20 lbs., the calculated extension will be found to be 53 thousandths of an inch, or 106 micrometer

divisions. This, again, agrees very well with the observed extension of 104 divisions. In this case the calculated maximum tension at the top end is 1150 lbs., and the mean tension on the 20 inches about 1000 lbs. Of this extension, however, about 11 per cent. is permanent, so that there is some failure of elasticity, and it is improbable that the maximum stress quite reaches the calculated value. It is practically certain, however, that it exceeds the mean stress corresponding to the *elastic* part of the maximum extension in the top 20 inches, viz., about 900 lbs.

Next as regards the time for which these stresses are applied. The strain at the top of the wave is $2\frac{V}{a}e^{-\mu at/M}$, where t is the time which has elapsed since the wave first arrived there. In the case of the 5-foot fall it will be found that the stress falls from 840 lbs., its initial and maximum value, to 500 lbs., which may be taken as the elastic limit, in about 0·8 thousandth of a second.

These results were fully confirmed by a large number of experiments in which different steady tensions were applied. The general conclusion is that in this material, which has an elastic limit of 40,000 lbs., or 17·8 tons per square inch, and breaks at 28·5 tons, a stress momentarily exceeding 75,000 lbs., or 33½ tons, and exceeding the static elastic limit for a time of the order of 1/1000 second, may be applied without any very great failure of elasticity*.

It may be further noted that a blow from a height of 10 feet, giving a tension momentarily exceeding 900 lbs., produces a permanent extension of 1/3500th part, or 1/30 of the ultimate extension caused by a steady load of 700 lbs.

Copper Wire. This was 0·129 inch diameter, and of the kind used in electric light cables. It was set up without preparation of any kind. A load of 220 lbs. stretched it by 1/1000th part, corresponding to $E = 7500$ tons per square inch. The wire weighed 0·0503 lb. per foot. The steady tension was 200 lbs. Mass of falling weight (M) 0·945 lb.; mass of stop, 0·023 lb. Velocity of propagation waves, a, = 11,800 f.s.

With a fall of 12·6 inches the additional extension observed on the top 20 inches was 41 micrometer divisions. The calculated extension (formula (2) above) is 43 divisions. The permanent extension produced by 20 blows was about 2 micrometer divisions.

With a fall of 2 feet 6 inches the observed extension was 67 divisions. The calculated extension is 70½ divisions. Ten such blows extend the 20 inches by 13·5 micrometer divisions, or 6·7 thousandths of an inch.

The elasticity is therefore practically perfect up to the stresses caused by a fall of 2 feet 6 inches. The greatest strain at the top end caused by

* The absolute stress is, as usual, calculated on the uncontracted area of the test-piece. The statical breaking stress at the moment of breaking is, of course, greater than this figure, but then the material is hardened by the drawing out.

this blow is $2/Va = 2{\cdot}1$ thousandths. The tension due to this is 460 lbs., and the resultant tension, including the initial 200 lbs., is 660 lbs. The mean tension on the top 20 inches is 570 lbs. (calculated from the observed extension of 67 divisions).

Tested statically with the extensometer, this wire showed failure of elasticity at 500 lbs. With a load of 590 lbs. it yielded rapidly and finally broke.

With a fall of 5 feet, and the same initial tension of 200 lbs., the observed extension on the top 20 inches was between 100 and 105 micrometer divisions, as against 103 calculated; of these about 30 divisions were permanent. The calculated maximum tension in this case (including the 200 lbs.) is 890 lbs. But the elasticity here is far from perfect, and the actual stress is probably somewhat less than the calculated value.

The observed extensions are, in the case of the copper wire, about 5 per cent. less than the calculated. I think that this is more than can be accounted for by errors of observation, or by such causes as friction between wire and weight, especially having regard to the much closer agreement in the other wires with which I have experimented. A possible explanation is that in the copper wire the value of Young's modulus for these extremely rapid extensions is 10 per cent. greater than for slowly applied forces. The difference between the adiabatic and isothermal elasticities as calculated from the coefficient of expansion and Young's modulus, is not sufficient to account for the effect, which must be a true time effect if it exists.

The history of the stress in a section of the wire after one of these blows is rather complicated, and it is difficult to deduce from the results anything more than the general conclusion stated above, that the wire is substantially elastic up to stresses much beyond the static elastic limit, and that the mathematical theory gives correct results. I hope, however, by suitable modifications of the experiment, to simplify the conditions, and obtain by this method more detailed information as to the properties of materials when subjected to shock. It seems to me quite possible that the stress-strain relations for stresses beyond the elastic limit may be much simplified if the stresses are applied for exceedingly short times, because the complication of hardening, due to over-straining, will be to a large extent removed.

5

THE ELASTIC PROPERTIES OF STEEL AT HIGH TEMPERATURES.

By BERTRAM HOPKINSON, M.A., M.I.C.E., and F. ROGERS, B.A. (Cantab.).

["PROCEEDINGS OF THE ROYAL SOCIETY," VOL. LXXVI, A, 1905. Communicated by Professor EWING, F.R.S.]

HITHERTO, investigations into the elastic properties of metals have been confined to comparatively low temperatures. Gray, Dunlop, and Blyth have measured the modulus of rigidity and Young's modulus for wires up to temperatures of 100° C., and found that both these quantities decrease as the temperature rises*. Martens determined the influence of heat on the strength of iron up to temperatures of 600° C., but his experiments were the ordinary tensile tests carried to rupture, and though he also found a substantial diminution of Young's modulus with rise of temperature, he did not go into the point fully, being mainly concerned with breaking stress and elongation†.

In the experiments here described the elastic properties of steel and iron have been investigated at higher temperatures, ranging up to 800° C., and for stresses greatly below that required to rupture the material. We have found that as the temperature rises the stress-strain relations undergo a remarkable change, which may best be expressed by saying that what is variously called the "time-effect," or "elastische nachwirkung," or "creeping," increases greatly with the temperature. Steel, at high temperatures, behaves like india-rubber or glass; if it is stressed for a time, and the stress removed, it does not at once recover, but after the immediate elastic recovery there is a slow contraction perceptible for many minutes. Such "creeping" can be detected at ordinary temperatures, but at a red heat it attains a different order of magnitude, becoming (in its total amount) a substantial fraction of the whole deformation.

The test-piece was 4 inches long, about 0·2 inch diameter, and had enlarged ends which were screwed into two steel bars each 1⅛ inches diameter and 10 inches long. The whole was set up in a vertical electric resistance furnace, wound with three coils of nickel wire. The currents in these coils could be separately controlled, and in this way the temperature along the test-piece could be made very approximately uniform. The temperatures were measured by three thermo-couples, placed one at each end and one

* *Roy. Soc. Proc.* vol. 67, p. 180 (Oct. 1900).

† *Proc. Inst. C. E.* vol. 104, p. 209 (1891).

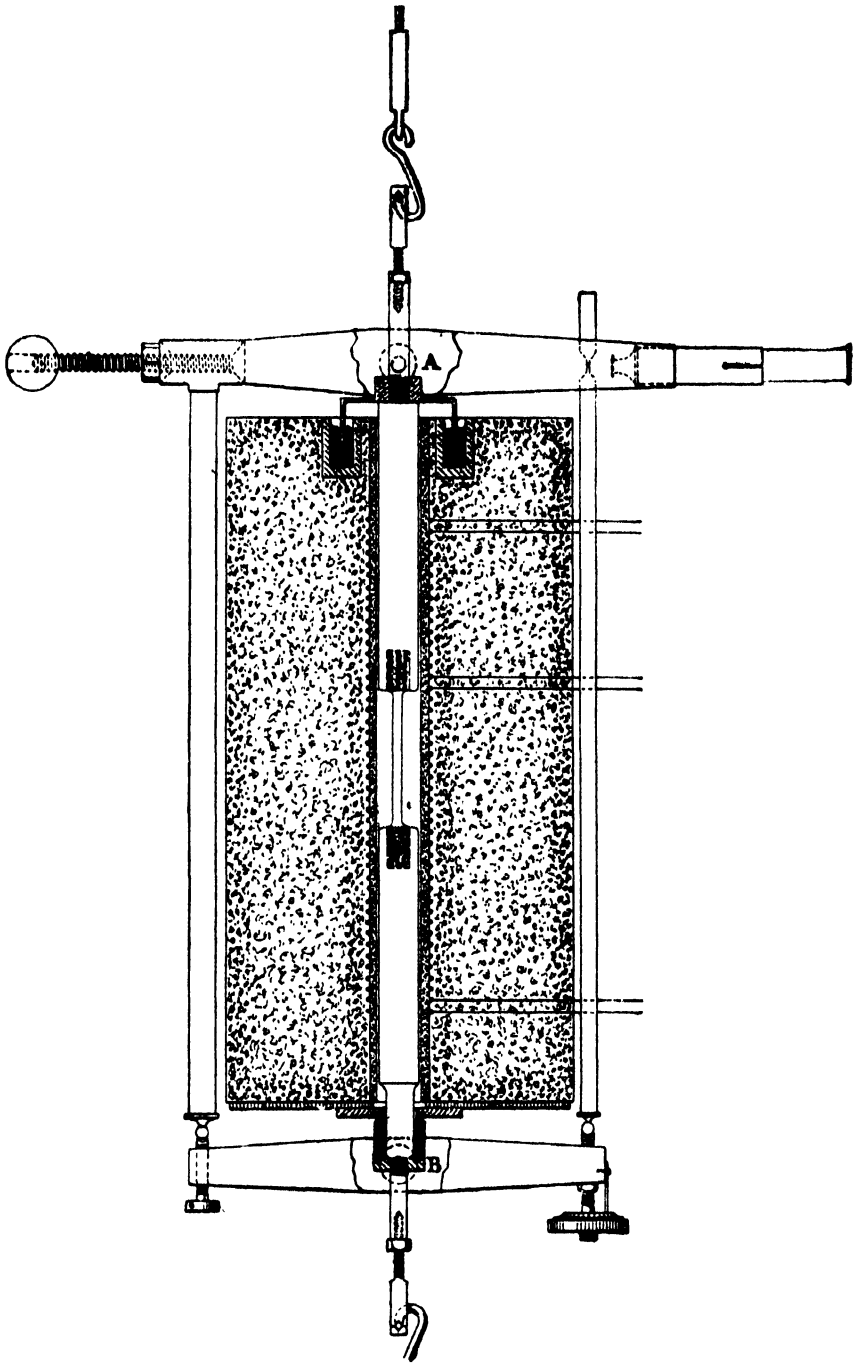


Fig. 1

in the middle of the test-piece. Fig. 1 shows the apparatus with one side of the furnace removed. Changes in length between the points A, B could be measured correctly to 1/50,000 of an inch, by means of an extensometer of Professor Ewing's pattern. The furnace was supported separately, and the test-bar, with the attached extensometer, was hung free within it. Tension up to 112 lbs. ($1\frac{1}{2}$ tons per square inch) could be very rapidly applied or removed by means of a foot lever at the lower end. The interior of the furnace was closed from the atmosphere by means of mercury locks, and the test-piece was kept surrounded by an atmosphere of nitrogen so as to avoid oxidation.

It will be seen that the extension observed included the elongation of the end-pieces, as well as that of the test-piece. The area of the latter being 1/30 of that of the ends, and its length one-fifth, it appears that of the total extension 87 per cent. is contributed by the test-piece and the remainder by the end-pieces, if the elastic properties of the two are the same. At low temperatures this is approximately the case, but at higher temperatures the average temperature of the ends is less than that of the test-piece, and they, therefore, contribute a less proportion to the total extension. In stating the results in this paper, the total extension is alone referred to, and it is stated in extensometer divisions, each of which is 1/5000 of an inch, or 1/20,000 of the length of the test-piece. It is probable that at high temperatures over 90 per cent. of this extension should be credited to the test-piece.

Two materials were tested, one being steel containing about 0·5 per cent. of carbon, and the other Low Moor Iron.

Fig. 2 shows the results of a series of tests carried out on a steel bar at 750° C. The bar was at no time heated much beyond that temperature. It was loaded with 85 lbs. for one minute, then unloaded for two minutes and so on, and the curve shows the resulting changes of length in terms of the time. It will be seen that even at this low stress (about $1\frac{1}{4}$ tons per square inch) the metal flows fairly rapidly, and that the overstraining has a considerable hardening effect, as shown by the diminishing amount of the permanent set produced by successive loadings. We found that this hardening disappeared with rest; that is, if the bar were left unstressed at 750° C. for a couple of hours after having been hardened by successive loadings, it was restored to its original soft state. With a slightly less load (about 79 lbs.) the flow of metal was very much slower, the permanent set produced by load applied for one minute amounting to only about 0·5 extensometer division, against 1·5 divisions for the load of 85 lbs.

In respect of all the features hitherto mentioned the properties of the material differ only quantitatively from those of the cold bar. Fig. 2 might, but for one remarkable difference, apply to a cold bar stressed to its yield-point. The difference lies in the behaviour of the bar after the removal of the load. The cold bar does not contract appreciably; there is the instan-

taneous elastic contraction, then it stops *. The hot bar, on the other hand, goes on shortening for two minutes or more after the load is off, as shown by the dotted line on the diagram, and the total amount of such shortening amounts to roughly one-third of the instantaneous contraction, or one-quarter of the total contraction, or one-fifth of the average total extension after hardening has taken effect.

The iron bar behaved in much the same way, but the metal flowed at

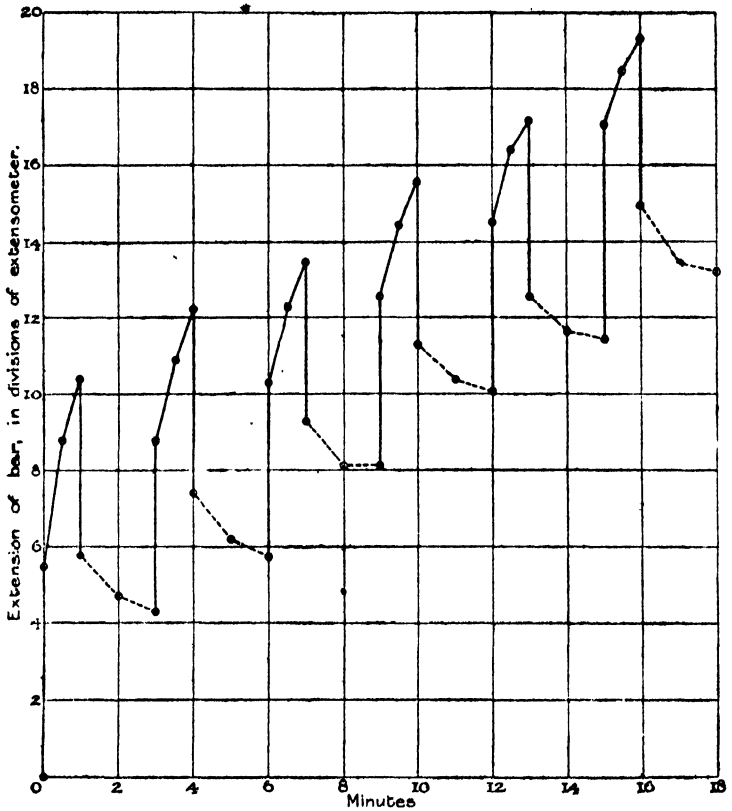


Fig. 2. Temperature of bar, 750° C. Load, 85 lbs. 1 division of extensometer = $\frac{1}{500}$″ = $\frac{1}{4000}$ part of length of test-piece. During the times covered by the dotted lines the bar was unstressed.

a lower stress. There was considerable flow with a stress of but half a ton per square inch. The shortening after removal of load was also perceptible at that stress.

At 600° C. both bars exhibited greater tenacity. A load of 112 lbs. (1·6 tons per square inch) applied to the steel bar for one minute produced an immediate extension of 3·8 divisions, followed by a slow drawing out,

* Professor Ewing, *Roy. Soc. Proc.* vol. 58, p. 123, found a certain amount of creeping after the removal of the load from a cold bar which had previously been stressed beyond its yield-point; but the effect was extremely small, the total creep never amounting to more than one-seventieth of the total extension of the bar when loaded. In a bar which had not previously been overstrained no such effect was observed.

which amounted in one minute to about 0·9 division. On removal of load there was an immediate shortening of 3·8 divisions, followed by a slow contraction amounting in two minutes to 0·7 division. The permanent extension produced was very small, if, indeed, there was any at all. The iron bar behaved in a similar way, but as at 750° C. it yielded appreciably at a stress which was not sufficient to permanently deform the steel bar.

The experiment on the steel bar at 600° C. shows pretty conclusively that this slow recovery after release from stress is not solely, or even mainly, dependent on overstrain. It seems to exist to a large amount with stresses which leave practically no permanent effect; the strain develops slowly under application of stress and disappears slowly after it is removed.

This phenomenon is, of course, analogous to residual charge in glass and other dielectrics; the stress corresponding to the electric force, and the strain to the electric displacement. Whether the law of linear superposition of the effects of stresses—closely followed in the electrical analogy—is true for hot steel or iron, is an interesting question which our apparatus was hardly sufficiently delicate to answer.

The magnitude of this effect in steel may best be gauged by comparing it with other cases of the same kind, *e.g.*, with the slow recovery of a glass fibre after twisting; if such a fibre be twisted through a considerable angle for several hours, it will recover all but one-fiftieth of the twist within two or three seconds of the removal of the stress*. The remaining slow "creep," amounting to one-fiftieth of the whole deformation, corresponds to the slow return of the steel. In india-rubber, under certain circumstances, 10 per cent. of the strain disappears in time after the removal of the stress†. But in steel, at 600° C., the proportion is about 15 per cent.

The apparatus used was not entirely satisfactory, having been designed for the purpose of measuring larger strains than have been dealt with in this paper. The principal difficulty lay in the slow variations of temperature in the bar and end-pieces, which could not be completely controlled, and which produced changes in length masking to some extent the changes due to stress, especially when the latter were spread over considerable times. We cannot do more at this stage, therefore, than assert the existence of a large time-lag between the stress and the strain in steel and iron at temperatures of 600° C. and over; and the figures which we have given must be taken as indications of its order of magnitude only.

One effect of such a time-lag will be to cause dissipation of energy if the material be subjected to alternating stress, for it will lead to a difference of phase between the stress and the strain; and the amount of the dissipation will depend on the period of the oscillations.

Gray, Dunlop, and Blyth found an increase in the rate of decay of the torsional oscillations of an iron wire as its temperature was increased to

* Dr J. Hopkinson, *Original Papers*, vol. 2, p. 350.

† Phillips, *Phil. Mag.*, April, 1905, p. 513.

100° C. On the other hand, Horton* has found a decrease in the rate of decay under circumstances which were apparently the same, except that the period of the oscillations was very much less than in Gray's experiments. These results might be reconciled and explained by the existence of such a time-lag as we have observed at higher temperatures. With our apparatus we could detect no time-lag at temperatures lower than 400° C.; but it is quite possible that it exists to the small extent necessary to account for the decay of oscillations.

Young's Modulus.

Another effect of "creeping," such as we have observed, is to make the determination of Young's modulus a matter of some uncertainty. Thus the extension of the bar at 600° C. produced by a given load varies 15 per

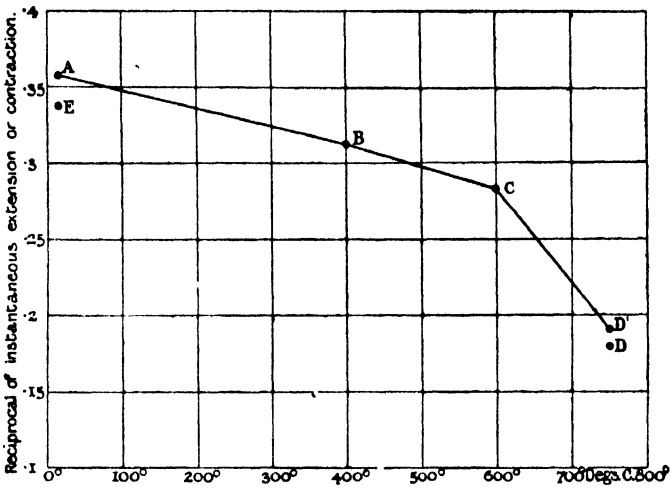


Fig. 3. Variation of Young's modulus with temperature. Load, 112 lbs.

cent. or more, according to the time of application of the load. When, however, the load is applied for a very short time, say of the order of one or two seconds, the strain produced seems to approach to a definite limiting value, which is the instantaneous extension or contraction of the bar observed in our experiments when the load is applied or removed. It seems reasonable to define Young's modulus for a metal in this state, as the stress divided by this limiting instantaneous strain. It is then independent of the manner of loading, and is a definite physical constant; otherwise not. We have shown in Fig. 3 the relation between Young's modulus, so defined, and the temperature. The ordinate is the reciprocal of the instantaneous extension produced by the load of 112 lbs. Owing to the effect of the ends, the reciprocal of the extension is not quite proportional to Young's modulus, their ratio being somewhat greater at high temperatures than at low,

* *Phil. Trans.*, A, vol. 204, p. 1.

as already explained. With regard to this figure it should be noted that the error in the determinations on the cold bar and at 400° C. is probably not more than about 2 per cent. At higher temperatures the error is greater, as, owing to the rapid drawing out of the bar, it was difficult to be sure of the instantaneous extension. It is, however, fairly certain that the ratio of Young's modulus in the cold bar and at 750° C. (as shown by the point D') is not more than 10 per cent. in error. The points were observed in alphabetical order, and it was found that, in spite of our efforts to secure a neutral atmosphere, the bar had scaled somewhat after heating to 750° C. This accounts for the larger extension shown by the point E, which was taken in the cold, after heating. After taking this point the bar was taken out, cleaned and gauged, when its area was found to be reduced by about 6 per cent. Allowing for the reduced area, the points E and A are in good agreement; but, of course, there is some little uncertainty from this cause as to the position of the point D. If the full reduction of 6 per cent. in the area be allowed for, the corrected position is at D', and this is probably not far from the truth.

In the iron bar the change of Young's modulus with temperature was of the same character but greater. The value in the cold being taken as unity, that at 600° C. was about 0·6, while at 750° C. it was about 0·5.

6

BRITTLENESS AND DUCTILITY.

Lecture delivered January 24th, 1910 to the SHEFFIELD SOCIETY OF ENGINEERS AND METALLURGISTS.

THE broad nature of the distinction between ductile and brittle materials is familiar to everyone. A ductile material, such as lead, is one which can change its shape largely without any rupture of continuity; whereas a brittle material such as glass cannot do so. Ductile objects flow; brittle objects cannot flow, but can only break. The practical significance in engineering of the property of ductility is also readily apparent. Structures are ordinarily designed with factors of safety so large that, if the calculations could be absolutely relied upon, and if no circumstances arose which were not allowed for in the calculations, no structure could by any possibility fail. But calculation cannot be exact, nor can it take account of all possible circumstances, and the structure as made is never exactly the same as the structure designed. The stresses to which it is subjected may exceed the maximum allowed for in the calculation, and errors of manufacture, initial stresses, etc., invariably cause the stresses in some parts of the structure when put together to exceed those predicted in the calculation; while in other parts they are less. A slight amount of adjustment in which some parts become permanently stretched and others compressed always occurs, and the possibility of yielding without rupture is necessary for this. The structure must also be capable of bearing forces much in excess of that for which it is designed, such as may be caused by an accidental blow, without being crippled; and for this it is necessary that the material of which it is made should bend rather than break.

A more detailed analysis of the property of ductility, however, reveals many difficulties and gaps in our knowledge. In the first place it is not at once apparent why the amount of ductility required by engineers in their materials, and shown by experience to be essential, is so great. In the case of boiler plates, for example, it is usual to specify from 20 per cent. to 25 per cent. elongation, and it is hardly necessary to observe that the permanent extensions of the material which occur in the adjustment of internal stresses due to the inequalities of temperature or other causes never exceed a very small fraction of this. It would appear at first sight that an elongation of, say, 5 per cent., which would admit of correspondingly greater strength, would amply cover all contingencies. There are also the "mysterious fractures," to which many references are to be found scattered about in technical literature, when a material presumably ductile has not behaved as such, but has broken short. The breakage of materials under alternating stresses slightly exceeding the elastic limit is

now fairly well understood, but the weakness caused by punched holes or by the sheared edges of plates has not, I think, been satisfactorily explained, though it is well known that the cause of fracture is the local treatment of the metal, and that danger can be avoided by removing the metal so treated. Other cases remain entirely mysterious, such, for example, as the fracture of boiler plates under the hydraulic test observed by Milton. In spite of the brilliant report of Professor Arnold on those occurrences, I cannot think that their cause has ever been satisfactorily cleared up. In one case the plate of a boiler cracked right across like glass under hydraulic pressure which imposed a stress of 12 tons per square inch, yet the material which had so cracked practically without any elongation broke, when tested in the testing machine, under a load of 28 to 30 tons on the square inch with an elongation of 27 to 28 per cent. I believe that the material of this plate could be distinguished from reliable steel by the alternating stress test devised by Professor Arnold; but though this is so far satisfactory that it suggests a means by which such occurrences may be prevented in future, it leaves us entirely in the dark as to their real cause.

I purpose in this paper to attempt to give a connected account of the phenomena comprised under the terms ductility and brittleness. In doing so I shall not be able to claim any novelty for most of the facts or ideas which I put forward. The subject has engaged the attention of the first engineers and scientific men for a very long time, and it is probable that almost everything that can be said about it in the light of known facts is to be found somewhere or other in technical or scientific literature. I have not seen, however, any connected account of the matter in which the facts are viewed from some definite standpoint, such an account as might properly appear in a text-book. And even if such exists somewhere, I think that the attempt to construct one may usefully occupy our attention for an hour. The mere attempt at the classification of phenomena clears the ideas, and if it does not fill gaps in our knowledge, at least reveals where they are. The point of view which I shall adopt is not the only possible one, and necessarily contains a considerable element of hypothesis, but I think that it is consistent with the facts, at least so far as they are known to me, and it is not repugnant to reason, which is perhaps all that can fairly be asked.

Let us consider the ordinary case of a bar subjected to tension in a testing machine (Fig. 1). Across any section at right angles to the bar the stress consists of a tension T tons per square inch, which means that if the bar were divided at that section the force necessary to hold the two parts together would, if distributed uniformly over the section, be T' tons per square inch; that force is in fact supplied by the cohesive attraction between the two parts of the bar. Simple considerations of statics show that across an oblique section inclined at 45° to the axis, the stress

consists of two parts, a shear and a tension, each equal to $\frac{1}{2}T$ tons per square inch. If the bar were divided on the oblique section the two parts would have a tendency to slide over each other, and also to pull apart; the first tendency must be resisted by a tangential force and the second by a force normal to the two faces; and these forces when distributed are respectively equivalent to the shearing stress and the tensile stress. Across intermediate sections there will be varying amounts both of shear and of tension stress, but the shear is nowhere greater than $\frac{1}{2}T$, nor is the tension anywhere greater than T.

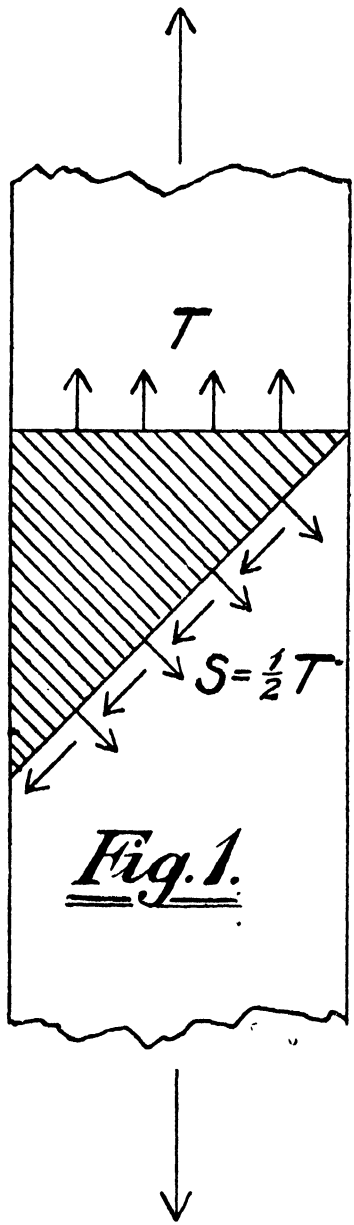


Fig. 1.

Other possible distributions of stress may be briefly referred to. In pure torsion a cubical block of the material bounded by planes at right angles to the axis of twist is subjected to shearing forces only on its four faces, equal, say, to S tons per square inch; while across a plane inclined at 45° to the planes of shear there is a tension or pressure S and no shearing stress. In the case of a boiler plate subjected to two equal pulls at right angles we have tension and no shear across any plane at right angles to the plane of the plate, practically no stress across planes parallel to the faces of the plate, and shear combined with tension across other planes, such shear reaching a maximum of $\frac{1}{2}T$ when the plane is inclined at 45° to the plate. Finally it is possible to conceive, though not practically to realize, a state of stress in which the stress across any plane consists of tension only without any shearing stress. Such a distribution might be produced in theory by subjecting each face of the plate of a spherical boiler to a normal distributed pull, equal in amount to the tension set up by the pressure in the boiler (Fig. 2). The distribution of stress in the material would then be similar to that of a fluid under pressure, the only difference being that the stress is a tension instead of a pressure, and such a distribution may be called a hydrostatic tension.

In all these cases so long as the force applied to the material does not

exceed a certain limit, which is the elastic limit corresponding to the particular distribution of stress, the deformation or change of volume of the material is very small and only measurable by refined methods. For example, in the case of a tensile test of mild steel, the increase of length within the elastic limit will be of the order of one-thousandth part, and this will be accompanied by a lateral contraction of about one four-thousandth part, the change of volume being of the order of one two-thousandth part. This small change of form or volume is proportional to the load and disappears when it is removed. Most structures are designed so that the stress is well within the elastic limit, and all experiment goes to show that such stresses, for however long a period they be applied

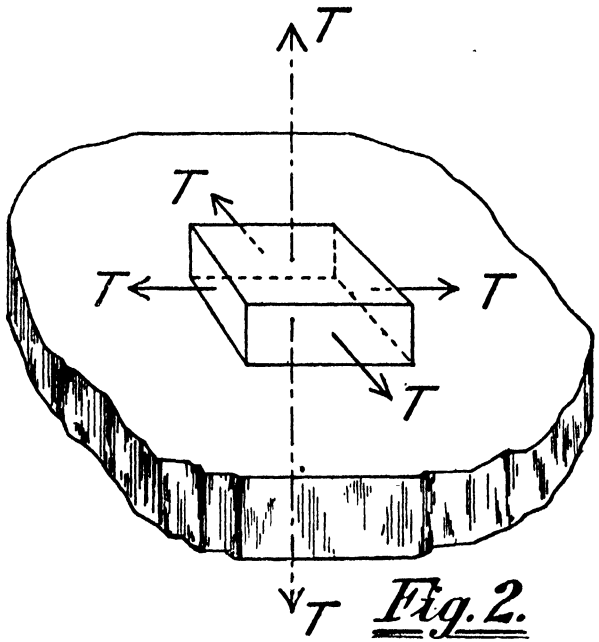


Fig. 2.

and however often applied or removed, or reversed, have no permanent effect whatever upon the material.

If, however, the load be increased beyond a certain limit, a point is reached when the cohesive attractions between the molecules are unable to withstand the stresses. Yielding will occur because the stresses across some particular plane are in excess of the cohesive force which the material can exert across that plane, and in a homogeneous isotropic material it may be expected that yielding will begin either at a point and across a plane where the tension is great or at a point and across a plane where the shearing stress is great. In the first case the material yields by tearing or rupture; it is torn at the place of large tension. In the second case it yields by sliding, the parts of the material in the neighbourhood of the plane of yielding slide over each other in a direction parallel to that plane. For example, in the ordinary tensile test a piece of glass or other brittle

homogeneous solid will, apart from local inequalities of stress, start breaking by the formation of a crack in a plane at right angles to the direction of the pull; on the other hand a ductile material under the same circumstances would begin to pull out by the sliding of its parts over one another in directions inclined at 45° to the axis.

The nature of the initial yield is obviously of great importance, since upon it depends in large measure whether the material is destroyed, that is, rendered incapable of resisting ordinary working stresses by the excess stress. The slightest yield of the first kind, that is, tearing, has the effect of destroying the cohesive forces, and therefore the continuity of the material, for practical purposes. How small is the separation of the parts of the material which will practically destroy this continuity is shown by the smallness of the force with which the most perfectly worked surface plates can be made to adhere. I believe that the maximum force of this kind that has been observed is of the order of 100 lbs. on the square inch. The effect of yield of this sort is therefore to reduce to zero the stress over a certain area, possibly a very small area, at the place and in the plane where it occurs, and this is accompanied by a corresponding increase of stress elsewhere. The redistribution of stress is such that at the edge of this area very high local stresses are developed, as in the case of a crack. Thus the parted area tends to extend, leading in most cases inevitably to the destruction of the material.

If, on the other hand, yield occurs by sliding, continuity of the material is not necessarily destroyed; and if continuity be not destroyed, the stresses in the neighbourhood of the place where sliding takes place, if altered at all, are changed in such a way that the greater tensions are diminished and the lesser tensions increased, thus reducing the tendency of the material to rupture. In a fluid which cannot permanently sustain any shearing stress at all, this equalizing effect of flow will persist until the stress is everywhere reduced to that uniform tension or pressure which I have illustrated in Fig. 2, and called hydrostatic tension. In a solid body which can sustain some shearing stress, the process does not go so far, but is always in the same direction. It equalizes up the tensions in different directions until the difference between the greatest and the least nowhere exceeds twice the limiting shear. Since the effect of flow is always to reduce the maximum tension, it follows that if the material does not at first break by parting, it will never do so unless the effect of the flowing is in some way to change its properties. In many materials an amount of flow sufficient to cause deformation easily perceptible to the eye can occur without much change in their properties. For example, lead may be caused to flow to almost any extent without any great change. In all metals, however, flow has some tendency to increase the rigidity or limiting shearing stress, and in iron or steel this effect is very considerable. It is probably for this reason that an iron bar breaks off short when a

certain extension has been reached instead of drawing out to a point, as lead can be made to do.

Brittle bodies such as stone or glass may yield by sliding if subject to such forces that there is only pressure and shear, as in the ordinary case of crushing by compression. But in such cases a small amount of slide seems to cause the disintegration of the material where it occurs. Breakage occurs before much deformation has taken place. It may be that this is only an extreme case of the hardening by pronounced slide, which happens in ductile metals.

We are thus led to recognize two kinds of brittleness. In one case the material yields under tension by tearing because it has a relatively large resistance to shear. In the other it yields by sliding because the tensions are insufficient to tear it, but disintegrates in the act of sliding. The same substance may exhibit both kinds. There is evidence that glass under tension tears and does not slide. I have not tried the experiment, but I have little doubt that a block of homogeneous glass, if compressed with proper precautions to avoid friction or inequality of pressure on the compressing surfaces, will yield suddenly by slide in or near the planes of greatest shear, as stone is known to do. Tearing is precluded, therefore it must slide, but as soon as it slides it breaks down. The mode of yielding in such cases can be discriminated by the planes of fracture, which will in one case be across the lines of greatest tension, and in the other in the plane of greatest shear. It is quite possible that there are substances which will be brittle under tension but will flow under pressure, because in the one case the relatively large rigidity precludes sliding and causes tearing, whereas in the other only sliding is possible, but such slide does not as in glass cause disintegration, but leaves the continuity of the material unimpaired.

The nature of the initial yield, that is, whether it is to be by rupture or by sliding, is determined by two factors:

(1) The distribution of the stress;

(2) The capacity of the material to resist tensile stress and shearing stress respectively.

Following Rankine, I propose to call the capacity of the material to resist tensile stress its "tenacity." For the capacity to resist shearing stress there is, so far as I am aware, no widely accepted term. Possibly "rigidity" is as good as any, and I will use it for the purpose of this lecture. It is necessary first to consider whether a definite numerical measure can be assigned to these two properties, and how it can conceivably be determined.

It is clear that tenacity and rigidity are definite and measurable quantities, at any rate under certain circumstances. Taking first the tenacity, we may suppose that a material is subjected to hydrostatic tension, as in Fig. 2. Whatever its nature, it is hardly conceivable that it can sustain an indefinite amount of such tension without breaking down, and it seems

certain that when it does break down it will do so by rupture—it will burst. This will occur at some definite tension, and that tension may be taken as a measure of the tenacity of the material. This is an experiment, however, which it is impossible practically to carry out, because there is no known means of producing and measuring a hydrostatic tension of amount sufficient to rupture a solid. But a definite conception can be formed in this way of what is meant by the numerical measure of tenacity.

The simplest numerical measure of rigidity is obtained by subjecting the material to shearing stress as in torsion, and finding the stress at which it just begins to yield; in other words by determining the elastic limit in torsion. This is an experiment which is easy to carry out in the case of a material sufficiently ductile to yield by slide when twisted, and the rigidity of many materials can thus be determined.

The numerical measures of tenacity and of rigidity arrived at in this way, however, refer only to particular distributions of stress. The tenacity is the maximum tensile stress which the material will stand when the tension is equal in all directions and there is no shearing stress; the rigidity is the maximum shearing stress which it will stand when there is no tension or compression across the plane of shear. The important question at once arises, whether the same numbers will serve to measure "tenacity" and "rigidity" under other distributions of stress. For example, let us suppose that a brittle material possessing relatively great rigidity, such as glass, has a tenacity as above defined of 10 tons per square inch. A mass of the material will then burst when subject to a hydrostatic tension of 10 tons per square inch. Will that material also break at a stress of 10 tons per square inch if tested in the ordinary way in a testing machine? We are assuming that it is sufficiently rigid to be able to sustain the maximum shearing stress of 5 tons per square inch, and that it is still subject to a tension of 10 tons per square inch across the planes at right angles to the line of pull. This tension is, however, now unaccompanied by the lateral tensions of equal amount which were present in the supposed experiment for determining the tenacity. The question is whether the presence of these other tensions affects the strength of the material. This is the difference between the theories of ultimate strength initiated by Lamé (who was followed by Rankine) and by St Venant respectively. According to Rankine, in a case of this kind, that is, where the material yields not by slide but by rupture, the rupture occurs in the plane across which the tensile stress is greatest and occurs at the same tension whatever the other stresses may be. According to St Venant's theory, on the other hand, the lateral tensions, if present, increase the strength of the material to resist longitudinal tension in a certain ratio depending upon Poisson's ratio for the material. I am not aware of any definite experimental evidence bearing upon this point, and it is one, I think, which ought to be investigated. The most obvious way of doing it would be to compare

the breaking loads of a tube or cylinder constructed of glass or other brittle material under tension and under torsion respectively.

Similar questions arise in regard to rigidity. The rigidity as defined above is the maximum shearing stress which the material will stand without sliding when there is no tension across the planes of shear, as in the case of pure torsion. Is this limiting stress affected by the existence of normal tension or pressure, and if so, how?

It was suggested by Rankine, and has been suggested by many others since, that resistance to shearing stress is analogous to friction, and that pressure across the plane of stress increases the limiting value of the shear, while tension diminishes it. According to this view sliding in a tensile test would not occur in the planes of maximum shear inclined at 45° to the axis, but in planes where the shear is rather less and the tension rather greater. It is found, in fact, that the lines (Lüder's lines) appearing on the surface of a specimen stretched in a testing machine are not inclined exactly at 45° to the axis; and this observation has been interpreted as confirming Rankine's suggestion. It may be doubted, however, whether this evidence is quite satisfactory, and other experiments capable of furnishing an answer to the question readily suggest themselves. The most obvious is, perhaps, to compare the elastic limits in tension and compression; in equal tension and compression the shearing stress will be the same, but in one case it will be accompanied by pressure and in the other by a tension of equal amount at right angles to the plane of stress. If the existence of this normal stress has any effect, it will be expected that the elastic limit should be lower in tension than in compression. The experiment is not so easy to carry out as it might at first sight appear, because it is difficult in a compression test to be sure that the stress is of the same simple character as in tension. On account of the shortness of the specimen, it is probable that the unequal loading and the friction at the end which occurs when the specimen begins to yield seriously affect the stresses in the interior. So far as I am aware, however, there is no evidence in such tests as have been carried out that there is any difference between the elastic limits in a specimen which has been thoroughly annealed so as to eliminate any effects of previous overstrain.

Another method of testing this question is to compare the elastic limits in torsion and in tension. In a torsion test there is no tension across the planes of shear, whereas in a tensile test the shearing stress is accompanied by a tension of equal amount. Many experiments of this kind have been made in recent years. The first of any importance were those of J. J. Guest, who found that the elastic limits in torsion and in tension were nearly the same. Further work was done by Mr L. B. Turner, at the Engineering Laboratory in Cambridge, who worked with mild steel tubes, carefully annealed. The result of his experiments was to show that the shearing stress at the elastic limit was probably the same within 5 per cent.

in the two cases. More recently, Mason and Smith have confirmed this result for mild steel, and have extended the experiments to other materials. Taken collectively, this work forms an important body of evidence that the materials ordinarily called ductile yield by slide in or near the planes of greatest shear and at a specific shearing stress which is independent of the existence of tensile or compressive stresses of the same order of magnitude as the shearing stress. They do not, however, afford any evidence as to whether much greater normal stresses might not affect the limiting shearing stress. They also leave unsettled the question whether any tension, and if so, how great a tension, unaccompanied by shearing stress exceeding the limit, would cause the material to tear, and thus to break like a brittle substance.

Effect of Cracks on Apparent Ductility.

It is known that near the edge of a crack or sharp-edged cavity the stresses are excessive. A piece of paper with a cut in the edge tears far more readily than if the edge be continuous; the same is true of india-rubber. The breaking of glass by scratching with a file or diamond is also a common experience. A few rough experiments which I made lately show that a scratch with a file just perceptible to the naked eye reduces the strength of a glass tube to resist bending to about half that of the unscratched tube, if the forces be applied in such a way that the scratch is under tension. If the scratch be on the pressure side the strength is not altered. The weakness of notched bars under impact is also familiar to engineers, so that the effect of re-entrant angles is not confined to brittle materials.

As far as I am aware the distribution of stress in the neighbourhood of a crack has not been investigated by mathematicians with the completeness which its importance deserves*. It is a well-known deduction from the theory of elasticity that in the neighbourhood of a spherical flaw the stress is about twice what it is at a distance; and a somewhat similar result holds for a cylindrical hole with its axis at right angles to the line of pull. These results, however, do not throw much light on what happens near the edge of a crack, the essential feature of which is that one of its dimensions, that is, the width, is very small compared with the others. It may be inferred from the elastic theory that if the crack have an absolutely sharp edge, the stress actually at the edge will be infinitely great; but this again does not carry us very much further, because it leads to the conclusion that a material with a really sharp edged crack must fail under any force, however small. Even cracked glass is capable of sustaining some stress, which shows that the cracks occurring in nature are not sharp edged in the mathematical sense. What is wanted is an investigation of the distribution of stress, near a thin lens-shaped cavity with slightly rounded edges, say, of the shape of a very flat spheroid, and I think that

* See "Stresses in a Plate due to the presence of Cracks and Sharp Corners," by C. E. Inglis. *Trans Inst. Naval Architects*, vol. 55, 1913, p. 219.

the full mathematical treatment of this question would well repay the considerable trouble it would involve.

Without investigating this problem in detail, however, one may from general considerations arrive at some notion of the kind of stresses which may arise near a crack. Let us suppose first that in a bar subjected to tension there is a slit having parallel faces and rounded edges, the plane of the slit being at right angles to the direction of the pull. The slit is supposed to be very narrow compared with its length, and also compared with the thickness of the plate. The distribution of stress will then be roughly as indicated in Fig. 3. Across a plane (AA) at a distance from the slit comparable with its length, the stress will be nearly uniform and will be normal to the plane; it will be unaffected by the existence of the crack. Across the plane of the crack (Ox) there will be the same total

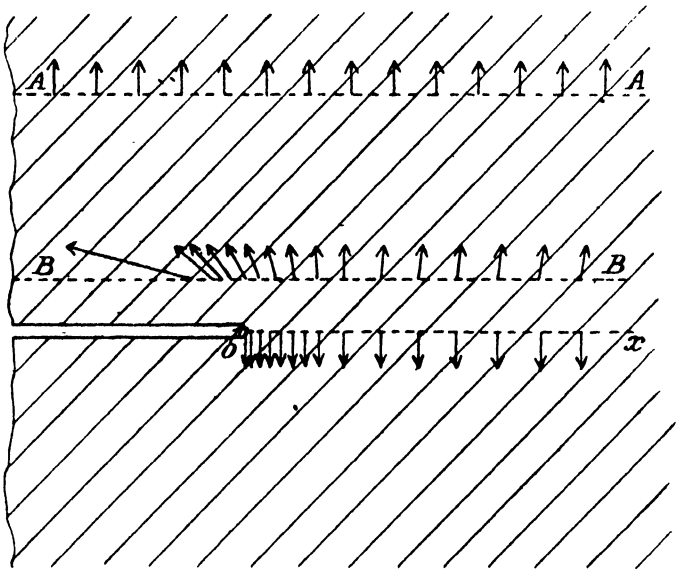
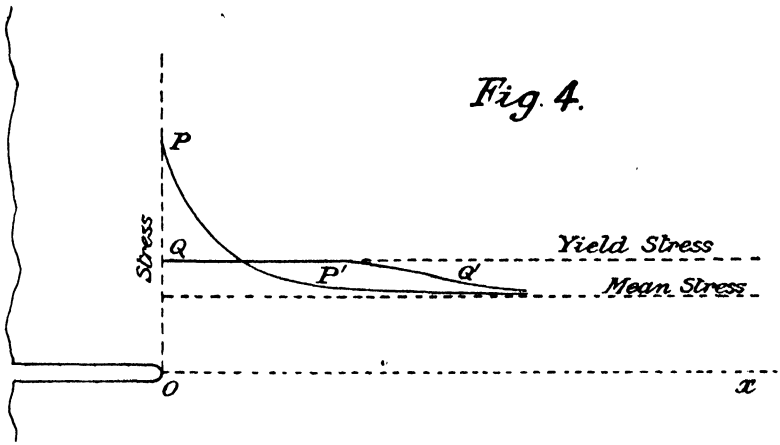


Fig. 3

stress, but it will be redistributed, being reduced to zero over the area of the slit and correspondingly increased elsewhere. The additional stress sustained by the reduced area will be concentrated more or less in the neighbourhood of the end of the slit, reaching a maximum value actually at the edge. The extent of the concentration will depend upon the relation between the width of the slit and its length, increasing indefinitely as this ratio is reduced. This is roughly indicated in the figure by the thickening of the arrows in the neighbourhood of the end of the slit. At a distance from the slit the stress across the plane will have the normal value, equal to the mean for the whole section of the piece. Across a plane (such as BB) nearer the crack, the stress distribution will be of an intermediate character, the lines of principal stress being inclined in such a way that they transfer the pull from the plane of the crack to more distant planes where it is uniformly distributed.

The state of stress at the point O will be the same as in a testing machine, the maximum shearing stress being half the tension. Assuming that the properties of the small portion of material near the end of the crack are the same as the average of the whole mass, it will start yielding at this point in the same manner as when pulled in a testing machine, but at a much lower mean stress than would a piece without any crack. If the material be brittle in the machine, it will fail by tear at the point O, and the tear will be in the plane of the crack, which accordingly will tend to extend in that plane. The tear having once started, being thinner than the original cavity, will have at its edge still greater tension and will consequently extend with great rapidity, keeping, however, generally speaking, the same direction. This is characteristic of the mode of fracture of glass, in which a crack once started, though it may curve slowly about in consequence of want of homogeneity in the material, does not, as a rule, suffer



those sharp changes of curvature which give what is called a crystalline fracture. To my mind this circumstance is strong evidence that glass does in fact fail by tearing when subject to tension, for if it failed by sliding the extension of the crack from the point O would take place along the planes of greatest shear at that point which are inclined at 45° to the plane of the cavity; the new crack thus started would again tend to propagate itself in a direction making 45° with its own plane, and would thus zigzag about, giving a crystalline fracture.

If, on the other hand, the material be ductile in the machine, it will start yielding at O by slide as soon as the shearing stress there reaches the limiting value. This will happen when the mean stress for the whole piece is only a fraction of the elastic limit. The effect of the slide is illustrated in Fig. 4, in which the curve PP' shows the stress distribution in the plane of the crack in a material which has not yielded at all. The form of the curve might be calculated from the elastic theory; this has not been done so far as I am aware, but its general features would be as shown.

The stress will be a maximum (OP) at the point O, and will diminish rapidly at first and then more gradually, becoming at a distance equal to the mean stress over the whole area of the piece (shown by the dotted line). If now by some means the rigidity of the material is reduced, so that it yields, the effect will be to reduce the stress everywhere to below the limiting value; the stress will be redistributed more or less in the manner shown by the curve QQ'. For some distance the stress will be equal to the yield stress of the material, and the curve will follow a straight line. It will then fall away, remaining however higher than the corresponding parts of the elastic curve, because the yielding of the portions under the straight line QQ' will increase the stress on the more distant parts which have not begun to yield. Finally this curve also will become asymptotic to the dotted line marked "mean stress." The areas under the two stress curves PP' and QQ' are the same, since there is no change in the total load.

It is clear that this process of redistribution brought about by yielding or sliding is only limited by the capacity of the material to be deformed. As the mean stress on the piece increases the yielding portions of material will extend further and further into the mass, until ultimately the yield stress will be reached everywhere in the plane of the crack. The piece will then be uniformly stressed up to the yield-point all over the plane of the crack, and in parallel planes at a distance from the crack the stress will be less than the yield-point stress in the ratio of the area of sound metal to that of the whole section. The permanent extension of the piece as a whole will be hardly perceptible, yet this slight extension, being in the neighbourhood of the crack concentrated on a length comparable with the width of the crack, may there amount to a very large percentage. The amount of local deformation or flow which would occur under these circumstances has not so far as I am aware been calculated. It is, however, clear that, generally speaking, the amount of slide near the ends of the crack may be of excessive amount, though it will extend over a small area only, and this will be the case even though the piece as a whole shows no change of length except the minute elastic extensions. It is also probable that the finer the crack the greater will be the local slide required to adjust a given amount of stress, but that it will be spread over a less area. This severe local deformation implies hardening and renders the material locally brittle. It imposes an excessive local tax on ductility. I suggest that the effect of a fine crack on the strength of a ductile material is an important field for investigation in which the services of the mathematician may be of great value to the engineer. It is at least possible that we shall find here the explanation (or at any rate an important part of it) of the paradox that a 20 per cent. elongation under test is necessary for the safety of a material which to all appearance is never stretched in use by more than a fraction of 1 per cent., and of the occasional failure of even this ductile material.

Such an investigation must, in the first instance at any rate, deal with a homogeneous isotropic solid, and will not be directly applicable to the assemblage of crystals of which metals ordinarily consist, though it will undoubtedly yield results of value in connection with the more complicated problem. The researches of Ewing and Rosenhain have shown that the plastic deformation of a metal ordinarily takes place by finite slips within the individual crystals instead of by the continuous flow which would probably be assumed in a theoretical investigation. Professor Ewing has kindly lent me some lantern slides showing microphotographs of this process. It would appear that in each crystal there are certain directions related in a definite way to the orientation of the crystal in which slip takes place, and that deformation can only occur by slide in one or more of this finite number of directions. An assemblage of such crystals, with random orientations, will certainly be indistinguishable in its elastic properties from an ordinary isotropic solid so long as the stresses and strains are so distributed that the stress in the neighbourhood of any point may be regarded as substantially constant over a considerable number of crystals. The theory of an isotropic elastic solid is thus applicable in ordinary cases to iron and steel. But at the edge of a crack we may be concerned with large variations of stress within the limits of a single individual crystal, indeed the edge of the crack may be actually within the substance of such a crystal, and under such circumstances the theory must be modified so as to take account of the minute anatomy of the metal. The number of planes in which slipping is possible is, however, so great that the freedom of motion of a crystal must be practically equal to that of an isotropic solid, and I think that it will probably be found that it will behave in much the same way even when cracked. Greater difficulties may arise when considering the propagation of cracks from one crystal to another, the effect of the connecting substance between crystals, and of the intimate mixture (as in all annealed steels) in the same material of two or more substances having different elasticity, different strength.

Effect of Speed of Loading on Ductility.

I have endeavoured in this lecture to put before you a way of regarding the phenomena of brittleness and ductility. I am not without hope that from the point of view which I have suggested an insight may be obtained into some of the difficult and complex practical questions about the mechanical properties of metals with which Sheffield is so deeply concerned. My own knowledge of such questions is too limited to admit of my attempting the discussion of them, even if time permitted; but I may perhaps allude to one point which I happen to have studied in some detail, namely the effect of rapid loading in causing brittleness.

If a stick of sealing-wax be supported horizontally at its ends, it will

slowly sag down under a comparatively small load; indeed its own weight will, in the course of a few days, cause it to bend considerably. In fact it behaves like a fluid of great viscosity, since a very small shearing stress is competent to cause unlimited flow. Yet for short periods—say for a few minutes or seconds—the stick of sealing-wax is capable of sustaining considerable loads without the permanent set becoming perceptible. There must of course be *some* permanent set, but there is no time for it to reach a perceptible amount. There is, however, quite a measurable *elastic* deformation, which disappears when the load is removed. It is easy to perceive this in a thread of sealing-wax of small diameter, which behaves like a springy piece of wire. Finally, the same stick which showed under moderate forces an unlimited ductility will snap like a piece of glass if the load hung upon it exceeds a certain amount.

The explanation of these facts is simple enough in terms of the theory which I have outlined above. Sealing-wax may be regarded as a solid possessing a definite tenacity, but in which the rigidity or limiting shearing stress is dependent partly on the speed of flow. Possibly the rigidity depends only on the speed of flow, and vanishes altogether when that speed vanishes. But in any event a solid of this kind subjected to tension will flow at such a rate that the shearing stress developed is just that corresponding to the tension. In a bar subjected to a straight pull, the shear stress will be half the tensile stress. So long as the tensile stress is less than the tenacity of the material, there will be unlimited ductility, but if it exceeds that value, the material will break like a brittle body.

These properties of sealing-wax have precise analogies in the behaviour of metals, but on a very different scale as regards time. Just as a stick of sealing-wax will exhibit almost perfectly elastic strain under a short-lived tension, which if prolonged would cause it slowly to pull out and ultimately break; so an iron wire is almost perfectly elastic under a load exceeding its static breaking load provided only that the load is applied for a sufficiently short time. I have measured the actual instantaneous extension of an iron wire subjected to a blow at one end by means of a falling weight. The maximum tension produced could be calculated, and it was found that the corresponding extension was exactly that corresponding to the hypothesis of perfect elasticity, being equal to the stress divided by Young's modulus. There was, moreover, no measurable permanent extension following on the blow, though a number of blows in succession did produce a minute permanent set, showing that at each blow there was some slight flow. This same maximum tension, however, when applied statically, rapidly drew the wire out and broke it. Moreover, iron or steel may become brittle under sufficiently great forces applied for very short times. No ordinary hammer blows will do, but the blow delivered by a high explosive is quick enough. If a slab of wet gun-cotton be detonated in contact with a mild steel plate, a piece will be blown

out and the edges will show a sharp crystalline fracture with hardly any contraction of area, though the plate will be bent slightly in the neighbourhood, showing that some flow had to take place before the velocity of shear was big enough to cause the material to fail by tearing instead of by flowing. Yet this same plate could be bent double in a hydraulic press. A hard steel rail, treated in the same way, fractures like glass without measurable deformation, though it would be capable of considerable permanent set under a steady load. Thus the analogy between the behaviour of metals and that of sealing-wax, as regards the effect of rapidity of loading, is very close, with this difference, however, that whereas the sealing-wax will behave substantially as a brittle elastic body if the stress lasts for several minutes; in the iron or steel the corresponding time is of the order of one-thousandth of a second.

7

ON HOLES AND CRACKS IN PLATES.

[From the discussion on papers by Prof. E. G. COKER and Mr C. E. INGLIS. "TRANS. INST. NAVAL ARCHITECTS," Vol. LV, Part I, 1913, pp. 232—4.]

PROFESSOR HOPKINSON said: Mr Chairman and Gentlemen, I have not much more to say than that I echo Professor Dalby's admiration of the work done here by Professor Coker. This is the first time I have had the opportunity of seeing the beautiful coloured diagrams that he gets when he strains his pieces and brings to the eye, in a way mathematics cannot do, the manner in which the stress is distributed. I have only one remark in the way of criticism to make about the paper, and it merely concerns the title. I think that what Professor Coker has determined is not the stress in the neighbourhood of a rivet, but rather the stress in the neighbourhood of a pin joint. I believe that when two plates are riveted together the strength of the joint against pull is not the shear strength of the rivet itself, but is due to the fact that the plates are pinched together by tension in the rivet which may not fill the hole at all, and, practically speaking, the joint holds by friction. I cannot cite any evidence on that at the moment, but I think that is the general impression of people who have had to do with riveting joints of the ordinary kind such as are found in structural steel work, and I am pretty sure there is a good deal of experimental evidence in favour of it. Of course, that does not in the least detract from the value of the paper, but it seems to me that the title is perhaps not quite correct. Mr Inglis just now suggested to me that the real boundary condition, looking at it as a problem in mathematical elasticity, in the neighbourhood of a riveted joint is that the metal just round the rivet is held so that it cannot move radially. It is the same as though the rivet were completely inelastic, and filled the hole, adhering to the metal of the plate all round, so as to prevent all radial displacement.' That seems to me to be the correct boundary condition, expressing, as far as mathematics can, what happens in the neighbourhood of a riveted joint.

Turning now to Mr Inglis' paper, it is, I think, one of great importance. Most failures in engineering structures originate, I suppose, in a crack of some sort, whether they be started, as he suggests, by the presence of some hard material or possibly by a cavity containing some impurity such as slag. They originate in a centre of high stress such as exists at the end of a crack. Mathematicians, of course, have told us before now that the

stress in the neighbourhood of a circular hole is as stated in Professor Coker's paper, and we have all learned that if the edge of the crack is an absolutely sharp corner, then you get an infinite stress, but the intermediate condition of a crack that has a very high curvature at the edge but is not infinitely sharp, has, so far as I am aware, been tackled by no one until now, and that, of course, is the important case. Mr Inglis has shown us exactly how the stress at the end of the crack varies with its curvature and size. He has shown that it is proportional to the square root of the length of the crack, and inversely proportional to its radius of curvature. He has pointed out, however, that his results being based on the mathematical theory of elasticity apply only within the limits of elasticity of the material. I hope that he will carry the investigation a stage further in the near future, and tell us what happens in a ductile material when the elastic limit is locally exceeded. Taking an elliptical cavity in mild steel, the material will begin to yield and stretch at the end of the cavity as soon as the stress there exceeds the yield point, and, as Mr Inglis points out, that will lead to a redistribution of stress, the result of which can be represented by taking off the top of the sharp-pointed curve representing the stress and raising the stress at points from there to the right so as to keep the total area the same. Unless the curvature of the crack is more than a certain amount this will, no doubt, save the material and the crack will not go any further. At the same time, it is obvious that that process of stretching puts an excessive local tax on the ductility of the material in the immediate neighbourhood of the end of the cavity. Whether the crack spreads or not depends on the relation of the amount of the local stretch to the ductility of the material. It would be a valuable sequel to Mr Inglis' work to find out the dimensions of a cavity which would cause the material to stretch, say, 30 per cent. at the ends when the metal as a whole is stressed to near its elastic limit. Such a cavity would presumably spread as a crack even in mild steel. It is known that good mild steel cannot be torn by any ordinary mechanical means. If a very thin slot be cut with a hacksaw at the edge of a mild steel plate, at right angles to the length of the plate, and the plate be broken in a testing machine, although, of course, the stretch is very much localised, the plate does not break by a tear starting from the slit, but the metal in and near the plane of the slot pulls out just as a longer piece would pull out with a good reduction of area. On the other hand, it is known that ductile material does under some circumstances break by tearing, and Mr Inglis in his paper cites the commonest instance of that, viz., where a crack starts from a punched rivet hole, or from the edge of a sheared plate. Owing to the greater elastic stress which exists in the neighbourhood of the hole, a crack is started there in the hardened material, and, as he says, that material has sufficient hold on the ductile material beneath it to cause the crack to spread. Once started in that way a crack may spread to any extent even in ductile material, as has sometimes happened in the plates of boilers.

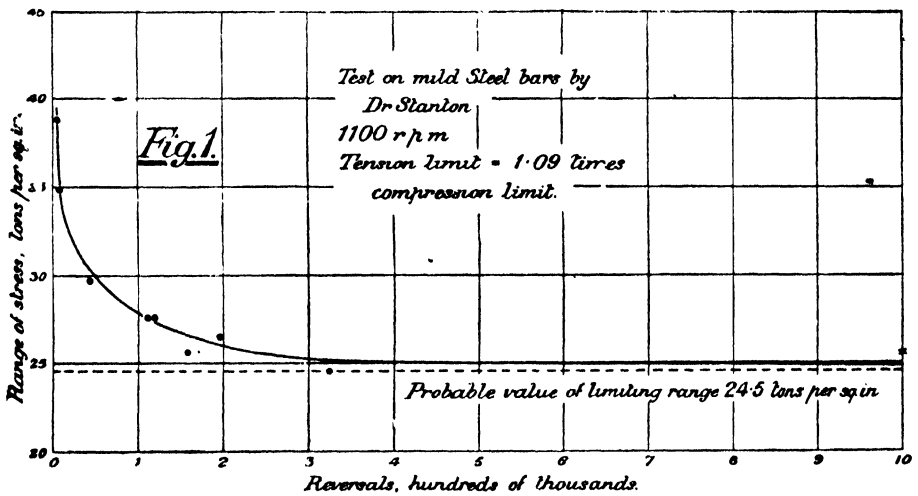
The effect of case-hardening in making mild steel brittle is another instance of the same thing on which I have made a few simple experiments. The Wolseley Tool and Motor Company were good enough to supply me with some bars case-hardened according to their ordinary process for motor car parts. The bars were of good mild steel (about ·12 per cent. of carbon) $\frac{3}{4}$ of an inch in diameter, and they were case-hardened, some to a depth of 1/32 inch, and some to 1/16th inch. They were broken in a 5-ton machine by bending. A bar case-hardened like that, although the central part is entirely ductile, snaps like a carrot under bending stress, and does not yield at all. That, I suppose, is a common experience. I then took a bar that had been case-hardened to a depth of 1/16 inch and had all the hard metal ground off. The piece so treated was first bent in the testing machine, and then finally bent double in a vice without showing any defect. That shows that the interior still possesses all the ductility of mild steel. This was one half of the same bar which, before the case-hardening was removed, had broken quite short by bending. It broke without a trace of permanent set. Then I tried the experiment of grinding off most of the case-hardening, but not all of it. I left a little strip of hard material on the tension side of the bar when it was bent. When a bar so treated is loaded by bending the process of tearing in ductile material is very well shown. At a certain load the hardened tension part at the bottom cracks with an audible snap, and when the load is a little increased the ductile stuff tears across like a bit of paper. That is, as far as I know, the only way in which mild steel can be made to tear. I have not tried the same experiment with iron instead of steel, but it is generally believed that iron is not affected by case-hardening in the same way. If so, it is perhaps due to its greater ductility, in virtue of which it can stretch sufficiently to relieve the stress even at the base of a fine crack starting from the hard part. A full experimental and mathematical examination of this problem of the ductility necessary to prevent cracks from spreading would, I think, be of great scientific and practical interest. Mr Inglis has taken the first step, and a very important one, towards its solution, and I hope that he will be able to carry it further.

8

A HIGH-SPEED FATIGUE-TESTER, AND THE ENDURANCE OF METALS UNDER ALTERNATING STRESSES OF HIGH FREQUENCY.

["PROCEEDINGS OF THE ROYAL SOCIETY," A, VOL. LXXXVI, NOV. 1911.]

THE deterioration of metals under the action of stress varying rapidly between fixed limits has been the subject of much experimental investigation. It is found that the important factor in the rate at which this "fatigue" goes on is the algebraic difference of the limits between which the stress varies, usually called the "range of stress"; and that the absolute position of these limits matters little, provided, of course, that the



mean stress is not too large. The number of applications of a given range of stress required to fracture the piece increases as the range is diminished, the general nature of the relation between the two being as shown in the curve (Fig. 1), which represents the results of a series of tests made by Dr Stanton on mild steel. In these observations the stress alternated between compression and tension, the ratio of the compression and tension limits being 1·09. The form of the curve suggests that a range of stress not much below 25 tons, which in an average specimen would just cause fracture after a million reversals, could never break the bar, however often applied. One of the chief objects of the fatigue tests hitherto made has been to discover this "limiting range."

At an early stage in these investigations the question was raised whether the endurance by the material of a given cycle of stress is affected by the rate of repetition of the cycle. Besides its intrinsic interest, this question is of importance because on the answer to it depends the possibility of reducing the excessive amount of time taken to carry out fatigue tests. The determination within a few per cent. of the limiting range requires several separate tests in which the cycle is repeated at least a million times, and even that number is not always sufficient to give a reasonably close approximation. Wöhler worked with 60 to 80 reversals per minute, and he found that the same wrought iron which could just sustain a million applications of a range of 23 tons broke after 19 million repetitions of a range of 17½ tons. The more recent machines have been run at much higher speeds, and there is now a machine of the Wöhler type at the National Physical Laboratory which gives 2000 cycles of bending stress per minute. Even at this speed, which I believe is the highest yet reached under conditions admitting of accurate measurement, it takes eight hours to do a million reversals.

Within these limits it does not appear that frequency of application has much effect on endurance, that is, it takes the same number of repetitions of stress alternating between given limits to rupture a bar whether they be performed fast or slow. It is true that the experiments of Reynolds and Smith* suggested that endurance was less at high speeds, but this has not been confirmed by more recent work†. The weight of evidence seems to favour the conclusion that speed is without effect on endurance, at any rate up to 2000 applications per minute.

For reasons which I discuss in detail below I think that this independence of endurance and speed is to be expected *a priori* so long as the speed does not exceed a certain limit, but that if that limit be passed the number of repetitions of a given range required to produce fracture will begin to increase, and it seemed probable that the increase might become noticeable if the speed were so high as 7000 per minute. It was mainly with the object of testing this point that I constructed the machine described in this paper, which gives alternations of stress at the rate of over 7000 per minute, or three times as fast as the highest speed hitherto attained. I may say at once that, in the case of the mild steel which has been examined, the endurance is undoubtedly greater at the higher speed, not only in number of reversals, but also in the actual time taken to produce fracture. Hence the machine does not, as I thought it might (if the speed effect revealed were zero or small), serve the practical purpose of reducing the time required for fatigue tests. It seems rather to open a new field for investigation, which differs from that covered by the older slow-speed machines.

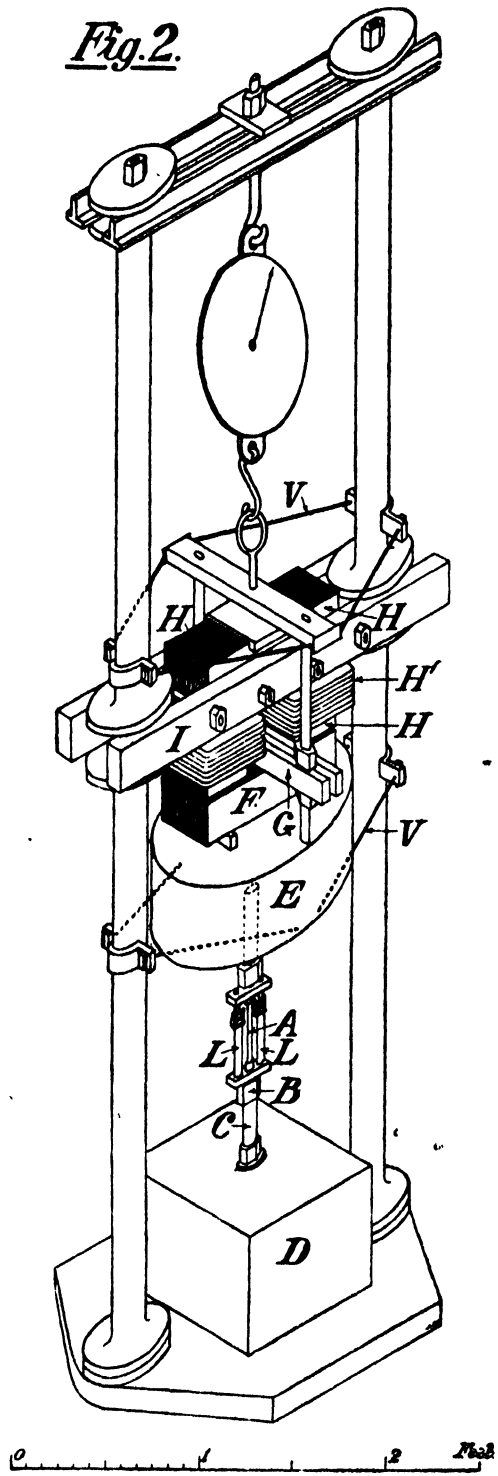
* *Phil Trans.*, A, vol. 199, p. 265.

† See "The Endurance of Metals," by Messrs Eden, Rose, and Cunningham, Institution of Mechanical Engineers, October 20, 1911.

DESCRIPTION OF THE MACHINE.

The apparatus (which is shown semi-diagrammatically in Fig. 2) is designed to give alternations of direct stress between any desired limits of tension and compression at the rate of 100 complete cycles per second, or more. The test-piece *A* is fixed at the lower end, by means of a nut *B*, to the screwed pillar *C*, which again is fixed into a massive block of cast-iron *D*, forming part of the base. The base is bolted to a concrete foundation. At its upper end the test-piece carries an iron weight *E* (about 180 lbs.), to the upper side of which is fixed the laminated armature *F* by means of yokes *G*. An electro-magnet *H* is carried by the pillars and cross-pieces *I*, above and independently of the armature, so as to leave a small air-space across which the magnetic pull acts. The magnet is excited by alternating current in the winding *H'* and exerts a periodically varying pull along the axis of the piece, whose frequency is twice that of the current*.

The test-piece behaves as a spring, the lower end of which is held fixed by the inertia of the masses to which it is attached,



* When the experiments described in this paper were practically complete I learnt that Prof. Kapp had also constructed a fatigue-tester, in which direct stress was produced by the pull of an electro-magnet excited by alternating current. The principle of resonance is not, however, used in this machine. A description of Prof. Kapp's machine appeared in the *Zeitschrift des Vereines Deutscher Ingenieure*, Aug. 26, 1911.

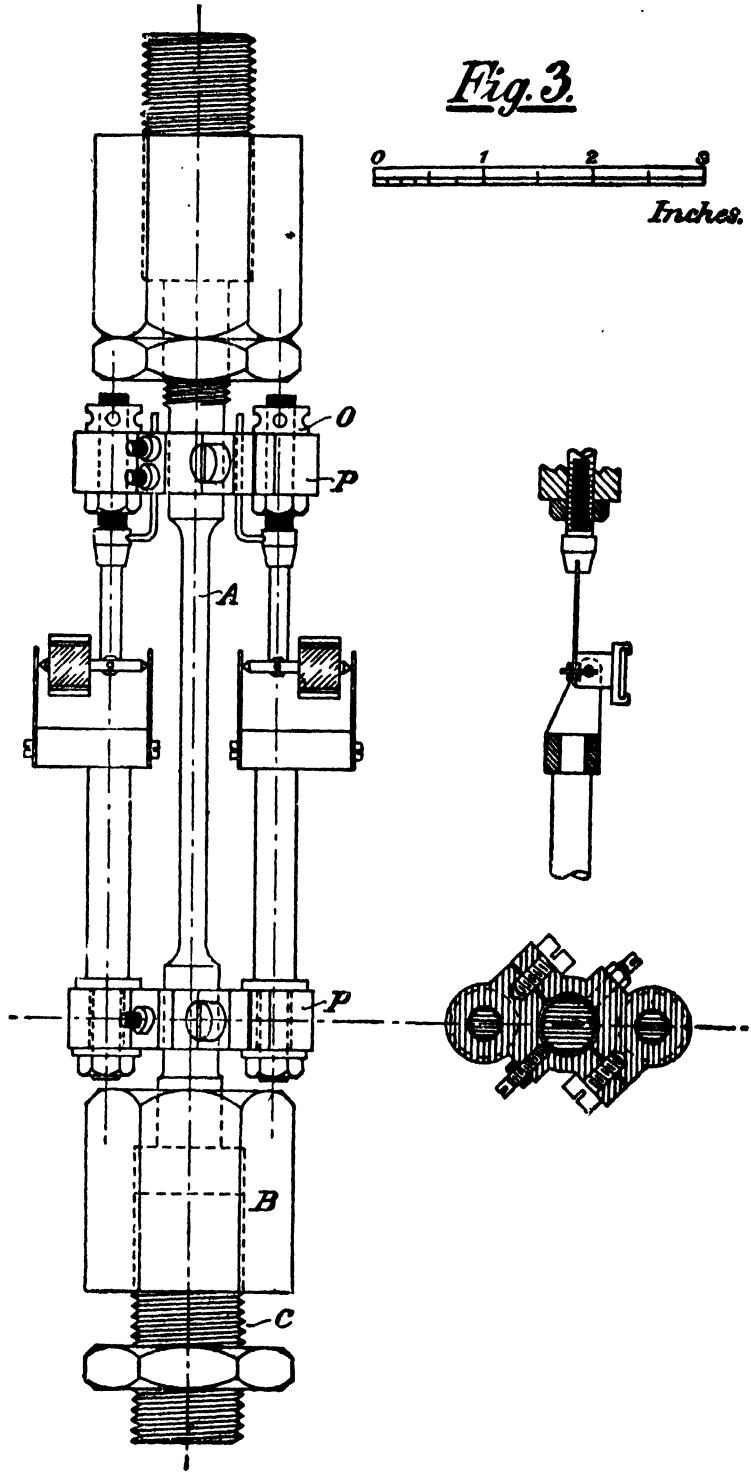
while the upper end carries the mass E. The adjustments are such that the natural period of vertical oscillations of this system is nearly equal to the period of the varying magnetic pull. The latter then sets up large forced oscillations of its own period. If the two periods are so far different that the dissipative forces have not much effect, the amplitude of the range of pull experienced by the piece is to the range of pull exerted by the magnet approximately in the ratio $1/(1 - n^2)$, where n is the ratio of the two periods. The range of stress in the piece can be adjusted coarsely by variation of the frequency of the magnet current, and more finely by changing the E.M.F. applied to the coils.

By means of wire guys V the weight is constrained to move parallel to the axis of the piece. Owing to the smallness of this movement the wires exert no appreciable pull in a vertical direction. The weight is carefully balanced so that its centre of gravity is in the axis of the test-piece.

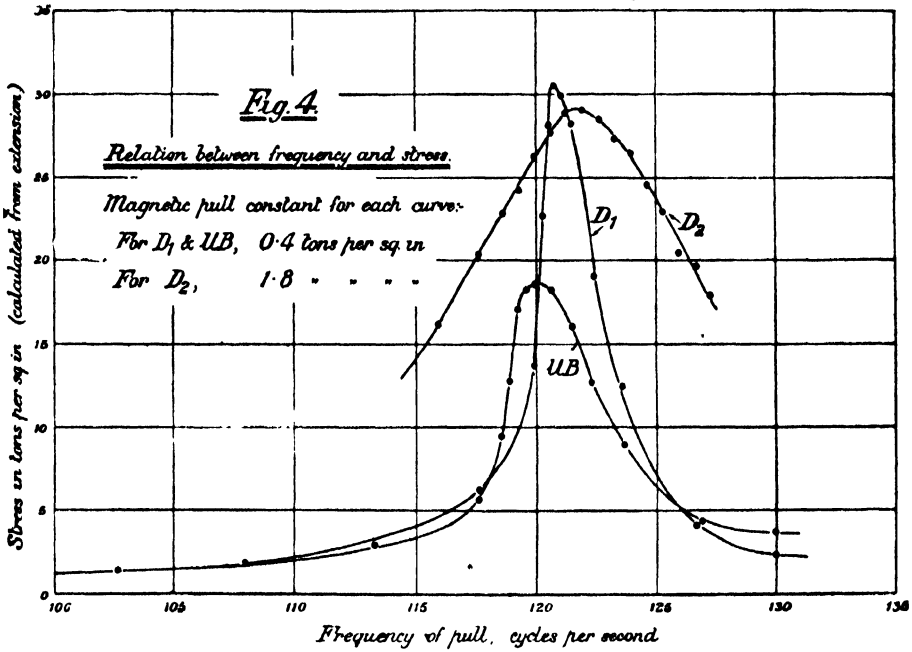
The extension of the piece is measured by means of a pair of extensometers L, which are shown in greater detail in Fig. 3. The concave mirror of each of these instruments casts on to a transparent scale an image of a horizontal lamp filament. When the instrument is in action the line image is drawn out into a band, whose ends are sharply defined, and whose length measures the range of extension in the piece. The extensometer readings are calibrated by means of the micrometer screws O (Fig. 3), and were checked when the machine was in action by observing, with microscopes, the movement of needle points attached to the blocks P, P. The extensometer always agreed with the microscopes within 3/10,000 inch, or 3 per cent. of the range, and the measurements of extension are certainly correct within that amount.

The form of test-piece used is shown in Fig. 3 with the extensometers attached. The section is 0·049 square inch. Current is supplied to the machine from a 4-pole rotary converter, which takes continuous current at 100 volts from a storage battery. Constant frequency in the supply of alternating current is, of course, of vital importance, and this is perfectly secured if the converter is driven from a battery which is not being charged or doing other work. The converter has a weak field, and is therefore largely self-compensating as regards speed, a drop in battery voltage producing a corresponding drop in the field current. It is found that, once set going, the machine will continue to run for several hours, giving a constant stress within 2 per cent., without being touched in any way. The converter takes about 10 amperes at 100 volts when the machine is in action. Test-coils of 100 turns each of fine wire placed round the magnet poles, just above the air-gap, are connected to an electrostatic voltmeter, and serve to measure the flux density, whence the magnetic pull can be calculated.

Typical curves showing the relation between frequency of supply current and the corresponding stress for a given magnetic pull are given in Fig. 4. The pressure of supply was raised in each case in proportion to


Fig. 3.
0
1
2
3
Inches.
O
P
A
P
B
C

the frequency, so that the flux density, and therefore the range of pull applied by the magnet, remained constant. The frequency was accurately measured by counting the beats between the notes produced by the machine and by a standard tuning fork. In one case (D_1) the maximum pull produced in the piece at the point of resonance was about 75 times, and in another (UB) (taken under similar conditions) 35 times the magnetic pull. The ratio between these quantities at the resonance point depends mainly on the rate of dissipation of energy by elastic hysteresis in the test-piece and its attachments, and by vibrations communicated to the ground. A bar of steel subjected to alternations of stress at the rate of 100 cycles or more per second in this machine gets perceptibly warm even when the stress is apparently within the elastic limit. It was observed that much



more heat was developed in the bar UB than in D_1 (which was of a different brand of steel), and it is probable that the difference between the two curves is mainly due to this cause. When these trials were made the machine was bolted to a very heavy block of concrete, and it is improbable that much energy was dissipated by vibration. When the third curve (D_2) was taken, the machine had been shifted on to a lighter foundation. The vibration in the surrounding floor was then perceptibly greater, and this is probably the cause of the smaller magnification of pull observed in this case (about 17 times). It will be observed that the resonance point is much less marked, the curve having a flatter maximum. This is in practice an advantage, though more power is taken to drive the machine, because the stress is less affected by slight alterations of speed.

The pull of the magnet varies between zero and a maximum value, and is always such as to give tension in the piece. The mean value of this pull is half the maximum value or range of pull, and (in consequence of the magnification by resonance) is small compared with the total range of stress. Such as it is, it is more or less counteracted by the weight of the mass E, so that in the normal working of the machine the mean stress is almost zero, and the limits of stress are nearly equal in tension and compression. The machine has been used in this way throughout the experiments described in this paper. It is easy to apply any desired steady tension by means of a screw and spring balance (see Fig. 2), and thus to alter the ratio of the tension and compression limits.

Measurement of Stress.

As already indicated, the quantity directly measured in this machine is the change of length of the piece in inches. By applying a static pull to the same piece in a testing machine the relation between pull in pounds and extension in inches can be directly determined, and the stress can then be calculated from a measurement of the section of the reduced portion of the bar on the assumption that Young's modulus is the same for the rapid alternating stress as for the steady pull. The experiments of Sears on the longitudinal impact of rods* justify this assumption completely if the stresses are within the elastic limit. In the present instance the stresses may be rather outside that limit, and the fact that fatigue, resulting ultimately in fracture, occurs, shows that at each application there must be a small extra-elastic extension or compression. Bairstow†, measuring the strain in a piece subjected to cycles of stress slowly performed (about two per minute), and rather outside the limiting range, so that fracture would ultimately result, found that the range of strain, which was at first that corresponding elastically to the stress, slowly increased, owing to the formation of a hysteresis loop of the type shown in Fig. 5. It consists of the two parallel elastic portions AB and CD which correspond to the normal Young's modulus of the material and the curved portions BC and DA. The error committed in calculating the stress from the strain on the assumption of perfect elasticity is represented very approximately by LM. After a good many thousand reversals this loop settled down to a constant form which apparently persisted till fracture. The extra-elastic strains at these slow speeds are considerable; for instance, in a steel whose limiting range was probably about $\pm$ 13 tons, the application of a range of $\pm$ 14 tons gave ultimately a hysteresis loop whose width (LM) was about $1\frac{1}{2}$ tons, or 10 per cent. of the range.

It is, however, quite certain that the non-elastic portion of the strain in the high-speed machine is nothing like so great, because, if it were, the magnification of the pull at the resonance point would be much less than is actually found. The magnetic pull, by its action on the moving weight,

* *Camb. Phil. Soc. Proc.* vol. 14, p. 257. † *Phil. Trans.*, A, vol. 210, p. 35.

provides all the energy dissipated mechanically in the system, whether by defective elasticity of the piece, vibrations of the floor, or other causes. The work done per cycle is easily calculated from the range of pull P and the movement of the weight d; it is, in fact, $\frac{1}{4}\pi Pd$ if the frequency corresponds to maximum resonance. This sets an upper limit to the area of the hysteresis loop, and thus to the amount of extra-elastic strain. It is not difficult to show that, even if the whole work done by the magnet is accounted for by hysteresis in the piece, the range of stress differs from the value corresponding to perfect elasticity by an amount not exceeding about $2P$*. For instance, in the curves D_1 and UB (Fig. 4), the error in measuring the stress must have been less than 0·8 ton per square inch, and was

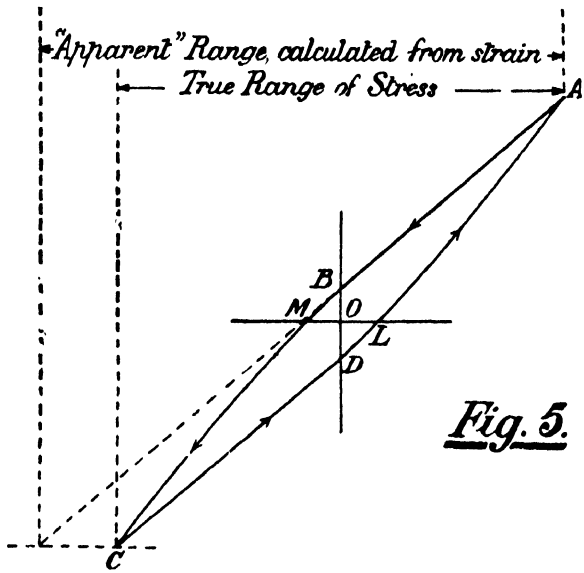


Fig. 5.

probably a good deal less. In D_2, which was exactly the same steel as D_1, the magnetic pull is much greater, but this, as has already been explained, is due to the greater vibration of the floor.

These considerations seemed to justify the view that, while the stress calculated from the change of length of the piece in this machine must be rather too high, the error could not be important, so long as the magnetic pull was but a small fraction of the range of stress. It was, however, thought desirable to determine the acceleration of the moving mass, and thus to obtain a direct measurement of the pull exerted by it. For this purpose a needle was fixed horizontally at a convenient point on the mass E, and a microscope, magnifying about 60 diameters, was sighted on the reflection, in the surface of the needle, of an electric lamp. By properly

* If d' be the change of length of the piece in a cycle, and P' the amount (LM in Fig. 5) by which the apparent stress calculated from the strain exceeds the true stress, the area of the hysteresis loop is greater than $\frac{1}{2}P'd'$. Hence $\frac{1}{4}\pi Pd$ (work done by magnetic pull P per cycle) is greater than $\frac{1}{2}P'd'$. Now in this particular machine d is about $\frac{1}{3}d'$, hence P' must be less than $2P$.

adjusting the position of the lamp, a very sharp line could be obtained, whose position, when the machine was at rest, could be defined within 1/10,000 inch. With the machine in action, this line was drawn out vertically into a band, whose length could be measured against a transparent scale in the field of the microscope correct to about 1/5000th of an inch. The microscope was supported for this purpose, with its axis horizontal, on a table loaded with iron plates, and resting on thick blocks of rubber placed on the heavy block of concrete to which the machine was bolted. In this way movement of the microscope itself was completely eliminated, and the apparent movement of the needle in its field corresponded to the actual motion of the weight. The wire guys V, being stretched fairly taut, prevent any angular motion of the weight, so that the movement of its centre of gravity is equal to that of the needle, wherever the latter be attached. The sufficiency of this constraint was proved by attaching a mirror to the weight and observing through a telescope the reflection of a distant lamp filament. Not the slightest tremor could be seen when the supply of current to the machine was suddenly thrown on, though an angular movement, giving a displacement of the needle (relative to the centre of gravity) of 1/5000th of an inch, would certainly have been detected.

Having thus obtained the range of movement a of the weight, its acceleration f at each end of its travel can be calculated from the formula $f = p^2a$, where $2\pi/p$ is the periodic time of the cycle. It is here assumed that the motion of the weight is simple harmonic. The mechanical conditions of the problem are such as to justify this assumption. We are dealing in effect with a weight attached to a spring, the other end of which is fixed, and this simple system has practically only one degree of freedom, so that its natural small oscillations will be nearly simple harmonic. Further, the wave form of the alternating E.M.F. supplied is nearly simple harmonic, and the corresponding magnetic pull, which depends only on the flux, will be of a similar character, and will force approximately simple harmonic vibrations in the mass of a period equal to its own period. Finally, under normal circumstances, the frequencies are adjusted so that the natural period of the mass is approximately equal to that of the force, and the selective action of resonance has the effect of accentuating that type of motion of the weight whose period corresponds with the fundamental period of the applied force.

A record of the extension of the piece in terms of the time was taken by causing one of the extensometer mirrors to throw a spot of light on to a revolving drum. The curve so traced showed no appreciable departure from a sine-wave. It must, however, be remembered that in consequence of the increased importance in the acceleration of high frequency terms in the displacement, a departure from simple harmonic motion which could not be detected on the record might produce a considerable change

in the acceleration. The justification of the assumption that for this purpose the motion may be treated as simple harmonic therefore rests mainly on the theoretical considerations mentioned above. The photographic records, however, were useful as showing the very regular character of the cycle of stress produced in the machine.

The forces acting on the weight are the pull in the bar and the pull of the magnet. At the point of resonance these two forces will differ in phase by a quarter of a period, and consequently at the two ends of the travel of the weight, when its acceleration is a maximum, the pull exerted by the magnet will be zero. At these points therefore the mass multiplied by the acceleration is equal to the pull in the rod, and the range of pull in pounds may therefore be taken as equal to $2Map^2/g$, where $2a$ is the range of movement of the weight observed in the microscope. If the frequency of the magnet pull differs from the natural frequency by such an amount as considerably to reduce the movement of the weight, the magnetic pull comes nearly into phase with the pull in the bar, the relation of signs being such that if the force frequency is the greater the magnetic pull assists the pull in the rod to accelerate the weight, while if the natural frequency be the greater these pulls are in opposite directions. Thus in the first case the range of pull calculated from the movement of the mass should exceed, and in the second should fall short of, that shown by the extensometer by an amount equal to the range of pull exerted by the magnet.

The following table gives one complete set of observations made with the object of comparing the different methods of getting the stress.

The material used in this trial and in the endurance tests described below was a mild steel containing 0·18 per cent. of carbon, with a breaking stress of about 30 tons per square inch.

Frequency of stress cycle, per sec.	100	100	116	120	134
Range of extension in piece:					
Extensometer *A* (thousandths of an inch)	1·6	2·5	8·6	9·1	0·9
„ *B* („ „)	1·5	2·5	8·2	8·9	1·1
Microscope *A* („ „)	1·6	2·5	8·6	9·1	1·0
Mean	1·6	2·5	8·4	9·0	1·0
Stress calculated from extension (tons per sq. in.) ...	*5·5*	*8·5*	*28·7*	*30·7*	*3·4*
Magnetic Pull:					
Volts on test-coil (100 turns)	47·5	58·5	46	49	48
Maximum flux density (kilo-lines per sq. cm.) ...	4·4	5·4	3·6	3·8	3·3
(1) *Range of magnetic pull* (tons per sq. in.)	*1·5*	*2·3*	*1·1*	*1·1*	*0·9*
Acceleration Pull:					
Movement of mass observed in microscope (thousandths of an inch)	2·4	3·6	12·1	12·7	1·6
(2) *Corresponding range of stress* (tons per sq. in.) ...	*4·0*	*6·0*	*27·3*	*30·6*	*4·8*
Sum or difference of (1) *and* (2)	*5·5*	*8·3*	*(28·4)*	—	*3·9*
Range of stress calculated from frequency (assuming that resonance occurs at frequency 119)	5·1	7·9	(29·0)	—	3·3

The point of resonance in this case was probably very near 119 periods per second. At 100 periods and at 134 periods the magnetic pull would be

very nearly in phase with the pull in the piece. It will be seen that at the first of these frequencies the sum and at the second the difference of the magnetic pull and the acceleration pull agrees closely with the tension in the piece. The frequency of 116 is near to but is certainly below the resonance frequency. There will be a considerable difference of phase in this case, but not so much as 90°. Consequently the stress in the piece should exceed that required to accelerate the weight by something less than the magnetic pull of 1·1 tons per square inch. In fact, it appears to exceed it by 1·4 tons per square inch. At 120 periods we are very close to the resonance point, but probably rather past it. Here the difference of phase will be nearly 90°, and it will be seen that the stress required for acceleration is very near to that calculated from the extension. The last line of the table gives the stress calculated from the magnetic pull P by the formula $P/(1 - n^2)$, where n is the ratio of the forced to the free period. This is, of course, not applicable to frequencies near the resonance point, when the amplitude is controlled largely by the dissipative forces, but it gives results in fair agreement with the observations at lower or higher frequencies.

A great number of comparisons of the stress estimated from the acceleration and that estimated from the extension have been made on different pieces of the same steel, at or near the point of resonance, and with a range of stress of 30 to 32 tons per square inch. These observations, taken altogether, reveal a systematic difference between the two; the stress calculated from the extension being higher by perhaps $\frac{3}{4}$ ton per square inch on the average. It seems probable that this is mainly due to defective elasticity, which becomes apparent in this particular steel when the true stress range exceeds 30 tons per square inch. The endurance tests which follow were made on the same steel.

It may be observed here that the phenomenon of increasing hysteresis under the influence of alternating stress which was observed by Bairstow at low speeds is completely reproduced at the high speed of this machine*. If the true stress, as shown by the movement of the weight, be maintained constant at a fairly high value, the strain shown by the extensometer slowly increases, reaching a steady value after the lapse of some thousands of reversals. At the same time the magnetic pull has to be quite noticeably increased in order to maintain the stress. The piece also gets considerably hotter.

Comparison of Endurance at Different Speeds.

Some systematic endurance tests were then carried out on the same steel with the object of finding whether the high speed had any effect on resistance to fatigue. Dr Glazebrook kindly arranged that a series of trials of the material should be made at the National Physical Laboratory in the

* Bairstow, *Phil. Trans.*, A, vol. 210, p. 42. The increase of hysteresis under alternating stress has also been observed by Turner, who noticed the rise of temperature of a tube subjected to a Wöhler test, *Engineering*, Sept. 4, 1911.

direct stress machine designed by Dr Stanton*, whom I must thank for the great interest he has shown in the tests and the trouble which he has taken in carrying out this part of the work. The steel contains 0·18 per cent. of carbon and 0·7 per cent. manganese and was supplied in the form of bright drawn bars of 1 inch or 1¼ inch diameter. The cold drawing to which these bars are subjected has the effect of raising the elastic limit of the bar as a whole, but that this change is not serious is apparent from the tensile tests given below, and anyhow it is of little consequence in a comparative trial. Three bars of this steel in all were used in the test, two being of 1 inch in diameter, and the other (*D*) 1¼ inch. These bars are referred to as *C*, *D* and *E* respectively. Tensile tests were made on two specimens cut from bar *D* with the following average results:

Elastic limit in tension	19·3 tons per sq. in.
Elastic limit in compression	13·4 ,,
Maximum stress	29·5 ,,
Elongation on 8 in.	16 per cent.
Reduction in area	60 ,,

For the tension test the bar was turned down to a diameter of ¾ inch, and for the compression to 1 inch. The elastic limits correspond to the load at which permanent set first becomes distinctly apparent in a Ewing extensometer; that for compression is not well defined. A series of seven pieces cut from bar *C* and tested in the direct stress machine at the National Physical Laboratory gave the following results, which have been plotted in Fig. 1:

N.P.L. No. of specimen	Mark	Reversals per minute	Range of stress, in tons per sq. in.	Reversals to fracture	Total duration of test (hours)
216	C VI	1351	38·8	6,613	0·08
217	C IV	1351	34·9	7,633	0·09
220	C V	1246	29·65	44,244	0·59
219	C I	1104	26·45	197,761	3·0
218	C III	1063	17·7	1,017,337*	15·9
218	C III	1283	25·7	150,981	1·96
221	C II	1092	22·8	1,113,484*	17·2
221	C II	1125	25·5	1,003,610*	14·9
225	C VII	1076	24·6	326,550	5·0

* Unbroken.

Pieces of another bar (*D*) of the same material were tested in the direct-stress reciprocating machine and also in a high-speed Wöhler machine with the following results:

* Stanton and Bairstow, *Inst. Civ. Eng. Proc.* vol. 166, p. 78. The first machine on these lines was made by Messrs Reynolds and Smith (see *Phil. Trans.*, A, vol. 199, p. 265).

N.P.L. No. of specimen	Mark	Reversals per minute	Range of stress, in tons per sq. in.	Reversals to fracture	Total duration of test (hours)
		Direct Stress Machine			
222	D I	1084	27·7	120,000	1·8
223	D II	1084	27·7	119,200	1·8
224	D J	1084	24·6	326,560	5·0
		Wöhler Machine			
77	D J	2200	27·0	1,637,500	12·5
105	D J	2200	25·5	7,000,000*	53·0

* Unbroken.

The endurance tests on the new high-speed machine were carried out on pieces of the same bars. It may be noted that the diameter of the test-piece—$\frac{1}{4}$ inch—is the same as that of the piece used in the direct stress machine at the National Physical Laboratory, so that exactly the same portion of the bar was tested. The trials were not all continuous, but were split up into periods separated by intervals of rest ranging from a few minutes to several hours. But most of the continuous runs covered at least a million cycles, and if a piece was not broken in one day, the trial was usually continued on the next, and so on, until fracture occurred. In all the trials the piece got perceptibly warm, and if it reached a temperature (judged by the hand) of 60° or 70° C. at the centre, a jacket of blotting paper was applied and kept soaked with water. It does not appear that the rise of temperature materially affected the endurance, for the pieces seemed to break indifferently at the centre or near the ends, where of course the temperature was by conduction kept nearly atmospheric.

Continuous observation was kept of the extensometer reading during a trial. After the first few minutes this settled down to a constant value, and very little adjustment was then required. The "apparent stress" given in the second column of the following tables is that calculated from the extension on the assumption of perfect elasticity.

In the first series of trials, which were made on the "C" bar the strain was kept at rather a high value, and there must have been considerable departure from elasticity. The true stress is, therefore, uncertain except in the one case C_7 where the movement of the mass was measured. Continuous observation was, however, kept of the E.M.F. on the test-coil and as pointed out in the last section the "apparent stress" cannot exceed the true stress by more than twice the range of magnetic pull calculated from these observations. This is the basis of the assertion (in cases other than C_7) that the stress in the following series of trials never fell short of 30 tons per square inch.

No. of bar	Range of stress (tons per sq. in.)		Time (hours)	Millions of reversals to fracture	Remarks
	By extensometer (apparent)	True stress			
C_3	32—34	Exceeded 30	15	6·0	Four periods
C_5	34—35	Exceeded 30	15½	6·2	Three periods, one of 12 hrs. continuous
C_6	33—34	Exceeded 30	29	11·5	Four periods, one of 14 hrs.
C_7	31—33	30—32 (by movement of mass)	7	2·8	Three periods
C_8	32—33	Exceeded 30	8½	3·4	Two periods

In most of these tests the piece heated to such an extent that it was necessary to jacket it with wet blotting paper.

Further trials were made on two pieces cut from another bar of the same material (*D*). Piece D_1 was the same as that used in the comparisons of different methods of getting the stress which have been described above, in the course of which the stress varied, rising sometimes to 31 tons per square inch. On the conclusion of these experiments it was put through a steady endurance test at an "apparent" range of stress of 30·5 tons per square inch, calculated from extensometer. It broke after 10 million cycles taking about five periods of five hours each on consecutive days (total, 25 hours). As the piece kept cool (it was not necessary to jacket it) it may be inferred that the elasticity was nearly perfect to the end of the test, and that the estimate of stress derived from the extensometer was not far wrong. Allowing liberally for all possible errors, it may be asserted that the stress exceeded 29 tons per square inch throughout this test. The treatment of piece D_2 was as follows:

Range of stress (tons per sq. in.)		Total time (hours)	Total reversals (millions)	Remarks
By extensometer	By acceleration			
28·2	26·8	32	12·8	Four periods, about 8 hrs. each
		Four weeks' rest		
29·0	28·8	15	6·0	Three periods, about 5 hrs. each
		Two days' rest		
30·0	28·2	14	5·6	Three periods, one of 12 hrs. continuous
31·0	30·0	15·8	6·3	The piece was perceptibly hotter
32·0	29·0	5·0	2·0	Two periods. Broke

Until the last period of five hours the piece was only slightly warm, and the elasticity must have been nearly perfect. During the last period it

got very hot, and had to be jacketed. It is probable that the stress did not fall short of 29 tons during the last 50 hours and 20 million reversals—certain that it did not fall short of 28 tons.

Finally, a piece E_1, cut from another bar of the same material, was subjected to the following treatment:

Range of stress (tons per sq. in.)		Total time (hours)	Reversals (millions)
By extensometer	By acceleration		
31	Not less than 30	6·7	2·7
32	Not less than 31	2·5	1·0 (broke)

Each of the runs was continuous, and they took place on successive days. In each continuous observation was kept of the movement of the weight, and care was taken that the true stress (calculated from the movement) should never fall short of 30 tons and 31 tons per square inch respectively.

Reference to the curve (Fig. 1) on which the observations with the slow-speed machine (1100 revolutions per minute) are plotted, shows that the probable life of this material under a range of 28 tons per square inch at that speed is about 100,000 reversals and one and a half hours of time; and with a range of 30 tons about half as much. In the Wöhler machine (at 2000 revolutions per minute) the endurance is apparently a little greater, but neither the number of observations nor the amount of the difference is sufficient ground on which to base an inference as to the effect of speed. But at the higher speed of 7000 alternations per minute the evidence seems to be conclusive that the endurance is greater. Not only is the number of reversals of a given range of stress required to break the piece greatly increased, but the actual time during which it must be subjected to alternating stresses is also increased. It is not, however, proved that the limiting range of stress, namely, the maximum range which will not cause breakdown however often it be repeated, is greater at the higher speed. Though the observations suggest that conclusion, it is quite possible that sufficiently prolonged vibration at a range of stress of 27 tons per square inch would ultimately have ruptured the material. Since, however, it takes 10 million reversals and some 25 hours of time to break the piece with a range which certainly exceeds 29 tons, it would appear probable that the number of reversals and the time required with a range of 27 tons would be very great indeed, perhaps so great that for practical purposes the piece might be regarded as unbreakable by this load if it be applied and removed sufficiently rapidly.

Probable Causes of Speed-effect.

In the hysteresis loop (Fig. 5) into which the stress-strain curve of a metal undergoing fatigue apparently always develops, the straight portions *AB* and *CD* correspond to perfect elasticity, but during the curved parts *BC* and *DA* the material is flowing or taking permanent set. There seems to be little doubt that fatigue is the cumulative effect of the internal slips which accompany this flow, and that the rate of fatigue is determined mainly by the amount of the extra-elastic strain occurring in a cycle (*BD* in Fig. 5) which Bairstow has called the "cyclical permanent set." Accepting that view, there are two ways in which speed may be expected to affect endurance. First, if the speed be high enough there may not be time for the full amount of flow corresponding to *BC* and *DA* to take place at each reversal. The amount of flow, and therefore of damage to the material, occurring at each cycle will then be reduced. So far as this factor is concerned, therefore, increased speed will make for greater endurance, provided that the speed is sufficient materially to affect the stress-strain relation. Second, there is the phenomenon of recovery. Any over-strained material tends to recover its elasticity with time. This tendency must be supposed to be continually at work during the progress of a fatigue test, repairing the ravages of the successive over-strains. Obviously, so far as it goes, its operation will be relatively more effective at slow speeds. Hence the effect of this second factor is to associate greater endurance with slower speed, and is opposite to that of the first. The combined effect of the two would be to cause the endurance (always reckoned as the number of stress-cycles to fracture) to diminish at first as the speed is increased, then to reach a minimum value and increase again. But if the influence of recovery on the progress of a continuous fatigue test is small, then the minimum of the curve connecting speed and endurance becomes very flat and we shall get approximately constant endurance until the speed reaches a certain limit, after which the endurance increases.

The conclusion expressed in the last sentence is in accord with the facts and the inference that recovery is not an important factor is at least probable. It may be, however, that the opposed actions of recovery and of time-change in the stress-strain relation happen to cancel each other pretty accurately over the flat part of the curve. Finally, to settle the point it would be necessary to carry out comparative tests in which the speed is the same but the rate of recovery different. The most obvious way would be to try the effect upon endurance of raising the temperature of the piece, which is known to accelerate recovery. Such an experiment would give very useful information. As pointed out above, there is some evidence in the experiments with the high-speed machine that the rate of fatigue, at that speed at any rate, is not much affected by temperature; but the observations were not directed especially to this point, and the evidence is not conclusive*.

* Some experiments on the effect of temperature on fatigue have been made by Unwin, who

The direct determination of the other factor—the effect of speed on extra-elastic stress-strain relations—also requires much more experimental study than it has yet received. It is certain, however, that if the cycle of stress be completed in 1/100 second or less, the effect must be large. The greater endurance at high speeds found in the present research is perhaps sufficient evidence for this assertion, but there is independent confirmation of it in the results of experiments which have been made on the extension of a wire under the action of a blow*. The wire (stretched vertically) was stressed by the blow of a weight falling against a stop at its lower end. Measurement of the momentary extension, and calculation from the mass of the weight and the height of fall, alike showed that the average stress over the top 20 inches of the wire must have reached a maximum value exceeding by 50 per cent. the static elastic limit, and sufficient if long continued to break the wire. Yet it was found that 11 such blows in succession extended the part of the wire under measurement by less than 1/2000 part of its length, and the wire was almost perfectly elastic up to the maximum stress applied by the blow. The stress was outside the elastic limit for about 1/1000 second at each blow, which is of the same order as the time during which the over-stress lasts in each cycle in the high-speed machine.

The arguments here used would lead to the conclusion that though the rate of fatigue under the action of stress which ultimately ruptures the piece will be lower at high speeds of reversal, yet the limiting range of stress which the piece will sustain indefinitely without breaking will be independent of speed. For if a single reversal produces any permanent effect whatever it is to be expected that a sufficient number must ultimately fracture the piece, and it is difficult to believe that time can be a factor determining whether or no such permanent effect shall exist, though it may affect its amount. It is quite possible, however, that as the time occupied by each cycle is diminished the effect produced may diminish in greater proportion. And the ultimate result may be that for practical purposes the limiting range is raised on account of the great length of time required to cause fracture. Further, it is noted that when the rate of fatigue is very slow the property of recovery becomes relatively more important, and it is at least conceivable that it may be more than competent to overcome the fatigue altogether and so really raise the limiting range.

The observations described in this paper were for the most part taken and reduced by my assistant, Mr H. Quinney, whom I must thank for the zeal and ability with which he has carried out the work. I wish also to acknowledge valuable assistance given at various stages by Mr G. Trevor Williams, advanced student in the University of Cambridge, and by my brother, Mr R. C. Hopkinson, Trinity College.

found that the endurance of mild steel was slightly increased at a temperature of about 200° C., *Inst. Civ. Eng. Proc.* vol. 166, p. 117.

* Hopkinson, *Roy. Soc. Proc.* vol. 74, p. 498. Also above, p. 52.

9

THE ELASTIC HYSTERESIS OF STEEL.

[By BERTRAM HOPKINSON, F.R.S., and G. TREVOR WILLIAMS.
"PROCEEDINGS OF THE ROYAL SOCIETY," A, VOL. LXXXVII, 1912.]

IN a recent communication to the Society* one of us described a machine whereby a bar of steel 4 inches long by $\frac{1}{4}$ inch diameter can be submitted to direct alternating stress at the rate of 120 cycles per second or more. The machine is worked by the pull of an electromagnet excited by alternating current, the pull being magnified from 20 to 60 times by resonance between its period and that of a weight attached to one end of the piece, which behaves as a spring. The stress varies between equal limits of tension and compression, and may be of any desired range up to 30 tons per square inch or more. The piece is fitted with an optical extensometer by which the extreme change of length of the piece in a cycle can be observed while the machine is in action and the range of stress calculated. An independent measure of the stress can be obtained by observation with a microscope of the movement of the weight attached to the end of the piece, whose acceleration is the chief element determining the tension or compression. Full details of these measurements are given in the paper referred to, and it will suffice to state here that similar precautions to secure accuracy in the measurements of stress were taken in the course of the work to be described, and that from the agreement between the different methods it may be taken as certain that these measurements are correct to about one half a ton per square inch.

The present paper contains an account of experiments which we have made with the alternating stress machine with the object of measuring the energy dissipated by elastic hysteresis when steel undergoes cyclical variations of stress within the elastic limit. The method used is to measure the fall of temperature between the centre and ends of the test-piece when it is undergoing continuous alternating stress through a constant range. The fall of temperature is proportional to the rate at which the heat is being generated and conducted away, and the absolute rate of dissipation in ergs per cubic centimetre can readily be obtained by passing an electric current along the specimen when at rest, and finding the relation between the temperature and the energy dissipated by resistance.

The steel used was the same material as in the previous experiments on fatigue. It contains about 0·18 per cent. carbon and 0·7 per cent. manganese, and it breaks under a load of about 29·5 tons per square inch (46·5 kgrm.

* *Roy. Soc. Proc.* A, vol. 86, p. 131. Also above, p. 82.

per square millimetre) with an elongation of 16 per cent. (on 8 inches). According to the fatigue test made by Dr Stanton at the National Physical Laboratory, the limiting range of stress is about 25 tons per square inch (say, 40 kgrm. per square millimetre). At the much higher speed of reversal reached in our machine the resistance to alternating stress is considerably greater, and more than one piece has remained unbroken after ten million cycles or more between the limits of $14\frac{1}{2}$ tons tension and $14\frac{1}{2}$ tons compression, giving a total range of 29 tons.

The test-piece is shown in Fig. 1. A detailed drawing of the extenso-

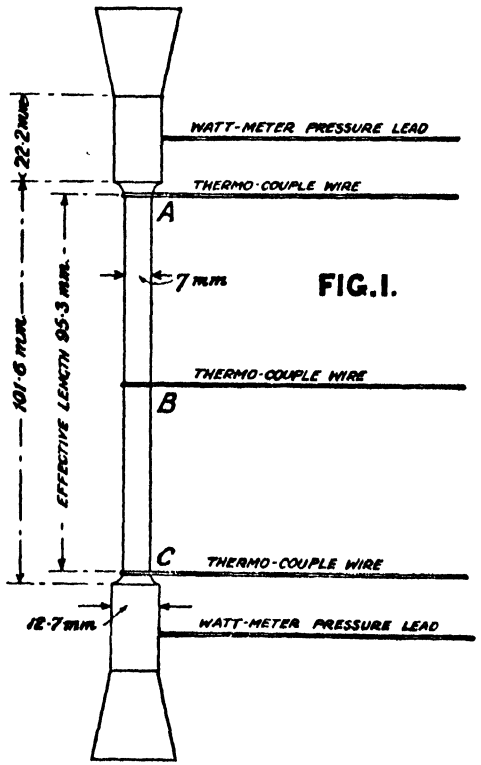


FIG. I.

meter is given in Fig. 3 of the previous paper (p. 86). For measuring the temperature three constantan wires are fixed to the reduced portion, one at the middle and one at each end (points A, B, and C in the figure). The middle wire and one of the end wires are connected to a galvanometer whose deflection is nearly proportional to the difference of the temperature between them. By using three wires and taking the mean of the falls of temperature in the upper and lower halves of the piece any flow of heat from external causes, such as arises for example from the eddy currents induced by the attracting magnet in the weight and in its attachments, is eliminated. The arrangements were such that the temperature difference between the centre and the end could be measured correct to about

1/20 degree C. Under a stress of 28 tons range the fall of temperature is about 10° C.

The determination of the dissipation of energy corresponding to a given fall of temperature is effected by passing alternating current along the piece, which is kept in position in the machine, only the extensometer being removed. The current is taken through the fixed coils of an ordinary suspended coil watt-meter, the leads of whose shunt or pressure coil are attached near the ends of the reduced portion of the test-piece (Fig. 1). The watt-meter measures the energy dissipated between the points at which these leads are attached; that dissipated in the reduced part between the points A and C is obtained by multiplying the measured watts by 0·85*. Simultaneous readings of the watt-meter and of the thermo-couple galvanometer are taken and a direct calibration of the latter is thus obtained in ergs per cubic centimetre. This is all that is required for the purpose of these experiments; the readings of the thermo-couple galvanometer were, however, also calibrated in terms of temperature, and it was found that the dissipation of energy shown on the watt-meter was accurately proportional to the temperature difference. The apparent thermal conductivity of the steel calculated from these observations, on the assumption that the whole of the heat is removed by conduction along the metal, was 0·17. The true conductivity of this material is probably about 0·14, whence it may be inferred that about five-sixths of the heat is in fact removed by conduction, the remainder escaping to the air surrounding the rod.

In making an experiment on elastic hysteresis, the machine was set to work at a certain stress, which was kept constant. The temperature rose for about five minutes and then remained steady. The experiment was usually continued for from half an hour to one hour, continuous observation being kept of the temperature and of the stress. The results are collected in Fig. 2. The actual observations are shown, and it will be seen that fairly consistent values have been obtained. It has been found possible to repeat the measurement of hysteresis loss at the higher stresses within 2 or 3 per cent., though the different measurements were separated by considerable intervals of time, during which the apparatus was taken to pieces or disturbed in various ways.

The energy dissipated in hysteresis increases about as the fourth power of the stress range. It is interesting to note that under a range of stress of, say, 25 tons per square inch (39 kgrm. per square millimetre) the energy dissipated per cycle by elastic hysteresis (25,000 ergs per cubic centimetre per cycle) is of the same order of magnitude as that dissipated by the magnetic hysteresis of similar material in fairly strong magnetic fields.

* This factor was obtained by a special measurement in which watt-meter leads were attached at A and C, and the reading compared with that shown (for the same current) when the leads were attached at the outer points. It agrees with the result of calculation on the assumption that the alternating current is confined mainly to the skin of the metal, so that the effective resistances of different portions are inversely proportional to their diameters.

There seems to be no reason to suppose that in either case is there any cumulative change in the properties of the material associated with the work which is being done upon it. But if the stress range be increased, the point must come at which there is such a cumulative effect, resulting ultimately in the destruction of the material by fatigue. The first sign of such a change would probably be an increase in the energy dissipated by hysteresis. The biggest stress range in these experiments was 28·6 tons per square inch (45 kgrm. per square millimetre) and there was no sign of any increase in hysteresis after half an hour of the application of this range of stress, corresponding to about one quarter of a million reversals. From Dr Stanton's results, however, it would appear most probable that under this range of stress, if applied 20 times per second, the material

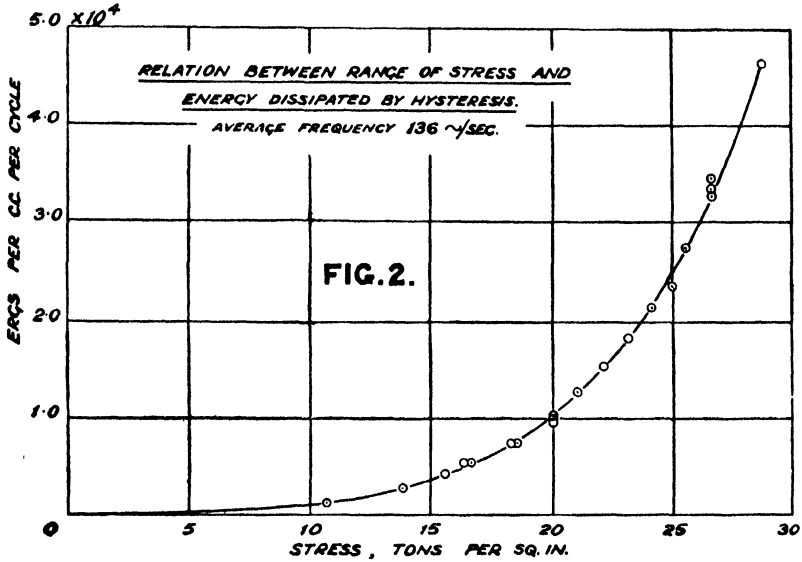


FIG. 2.

would break after less than 100,000 reversals, and that therefore there must be *some* permanent effect, though it is perhaps almost negligibly small at the higher speed of reversal.

The experiments are being continued with the object of finding evidence of this increased hysteresis and generally of discovering how the hysteresis loss is related to the elastic limit.

Static Tests.

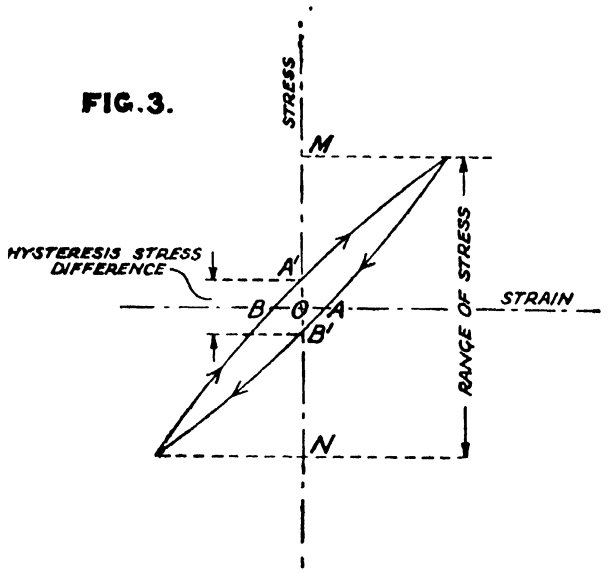
The effect of elastic hysteresis is that the stress-strain diagram corresponding to a cycle ranging between equal tension and compression is a closed loop as shown in Fig. 3, instead of a straight line*. It is the work represented by the area of this loop which is measured in the experiments which have been described. It appeared important to get some approxi-

* The width of the loop is much exaggerated.

mation to the stress-strain curve obtained in a static test, in order to ascertain whether the speed of reversal had any effect on elastic hysteresis.

The only absolute measurements of elastic hysteresis of which we are aware are those described by Ewing, who loaded and unloaded a long wire by means of weights*. Traces of apparent hysteresis have been observed in ordinary measurements of elasticity with an extensometer and testing machine, but there is always a doubt in such cases whether a large part of the difference observed between loading and unloading is not due to friction in the testing machine or in the extensometer. The difference of length to be measured is not more than one hundredth of the total variation of strain, and on a piece 4 inches long amounts to but 1/50,000 inch, so that the measurement is a very delicate one, an error of one-millionth of an inch in absolute length being equivalent to 5 per cent. in the result.

FIG. 3.

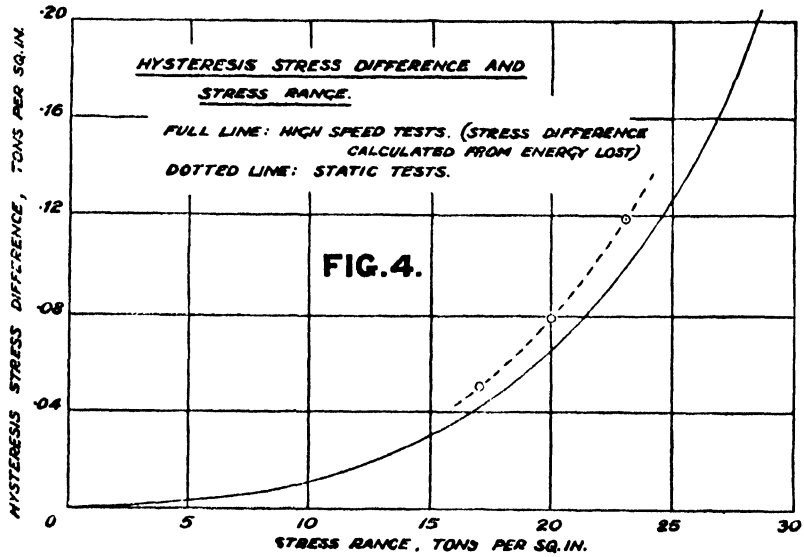


In the present instance no attempt was made to determine a complete stress-strain curve, but the maximum difference of length AB (Fig. 3) was measured. The same piece of steel as that used in the alternating stress machine was first subjected in a testing machine to compressive stress of the amount desired, say, 10 tons per square inch. It was then removed from the compression machine and loaded in tension with weights and a lever to 10 tons per square inch. After this process of alternate compression and tension had been repeated several times in order to get the piece into a cyclical state, measurements of length were taken by means of an extensometer, first, after the piece had been put into the testing-machine, having just previously been loaded to 10 tons per square inch compression, and, second, after a load of 10 tons per square inch tension had been applied

* *Brit. Assoc. Report*, 1889, p. 502.

and removed. The piece was a little longer in the second case than in the first, and the difference in length is the quantity sought. The advantage of this procedure is that there is no measurement of load; when taking both readings the piece was hanging practically free, and there was no possibility of any stress in it. The only errors are those due to strain and backlash in the extensometer, and it was found possible to eliminate these. The resulting values of $A'B'$ (the stress difference corresponding to the change of length AB) are probably correct within 1/100 ton per square inch.

Details of the measurements and of the extensometer are given in an Appendix. The following are the final results. In each case the cycle of



loading was between equal limits of tension and compression, and the "range of stress" given in the first column is twice either of these limits:

Range of stress (tons per sq. in.) ...	17·0	20·0	23·0
Corresponding stress difference ($A'B'$) (tons per sq. in.)	0·05	0·08	0·12
Length difference (AB) (millionths of an inch)	15	24	36

These figures agree as regards order of magnitude with those given by Ewing. The range of stress in one of his experiments was about 15 tons per square inch, the limits being roughly 5 tons and 20 tons per square inch respectively (both in tension). At the intermediate load of 12½ tons the strain in the wire was greater during the unloading part of the cycle than that corresponding to the same stress when loading, by about 1/150 part.

At 17 tons range in the above table the corresponding ratio is 0·05/8·5, or 1/170.

The area of the hysteresis loop can, of course, only be roughly guessed. Assuming, as is probable, that the loop is of the lenticular form shown, its area must lie between $AB.MN$ and half that amount. If the two sides are arcs of circles the area is $\frac{2}{3}\,AB\,.\,MN$. On the assumption that this is, in fact, the area, the value of AB has been calculated from the work done per cycle as measured in the high speed alternating stress machine. The width of the loop on the stress line $A'B'$ is AB multiplied by E, and this has been plotted in Fig. 4. On the same figure is shown the value of $A'B'$ as determined from the statical experiments.

It will be observed that the stress difference calculated from the energy loss bears a substantially constant ratio of 0·8 to that measured statically. This ratio, of course, depends entirely on the factor chosen for calculating the area of the loop from its dimensions. On the extreme suppositions that this factor is 1 and $\frac{1}{2}$ respectively (instead of $\frac{2}{3}$), the ratios of high speed to static hysteresis would become 0·53 and 1·06 respectively. It seems probable, therefore, that the hysteresis in cycles performed 120 times per second is (if anything) less than that found in static tests, but it is unlikely that the difference is more than 30 per cent. Lord Kelvin, as the result of his experiments on the torsional oscillations of wires, was led to no very definite conclusion as to the relation between the dissipation of energy, in a vibration of given amplitude, and the period, but apparently thought that the loss increased slightly with the speed*. Wire, and especially the outer skin of wire (which is alone operative in torsional experiments), is, however, in an abnormal condition, and may give results which are both irregular (as was found by Kelvin) and different from those found in a bar of the same material.

Appendix.

The extensometer is shown diagrammatically in Fig. 5. It is similar in principle to the well-known Ewing instrument, except that a tilting mirror observed from a distance through a telescope is substituted for the microscope. Two rings, A and B, surround the thickened part of the test-piece, and each is fixed to it by a pair of pointed screws so that the rings pivot about the axes C and D respectively, which are fixed in the piece. The rod E is rigidly fixed at its lower end to the ring B, and its rounded upper end engages with a conical recess in the end of a screw fixed in A, being held up by the spring K. The pillar F, which is fixed to the ring A, engages at its lower end with a steel ball at the end of an arm (about 0·08 inch long) attached to the mirror G. This mirror is pivoted in bearings attached to the pillar H (fixed to ring B) so that it can turn about a horizontal axis I. The reflection of a vertical scale in the mirror is observed in a telescope at a distance of about 6 feet. It is possible to read the scale correct to 1/10 mm.,

* *Math. and Phys. Papers*, vol. 3, p. 24.

which corresponds to a change of length of about 1·2 millionths of an inch. A fixed mirror on the piece serves to measure any tilt of the apparatus as a whole. The instrument is calibrated with sufficient accuracy for the purpose by loading the piece with a known tension.

Change of temperature in the piece, due either to conduction of heat to or from the testing machine attachments, or to slow change in the room temperature, causes a gradual change of length. This was allowed for by observing the rate of change before and after an observation.

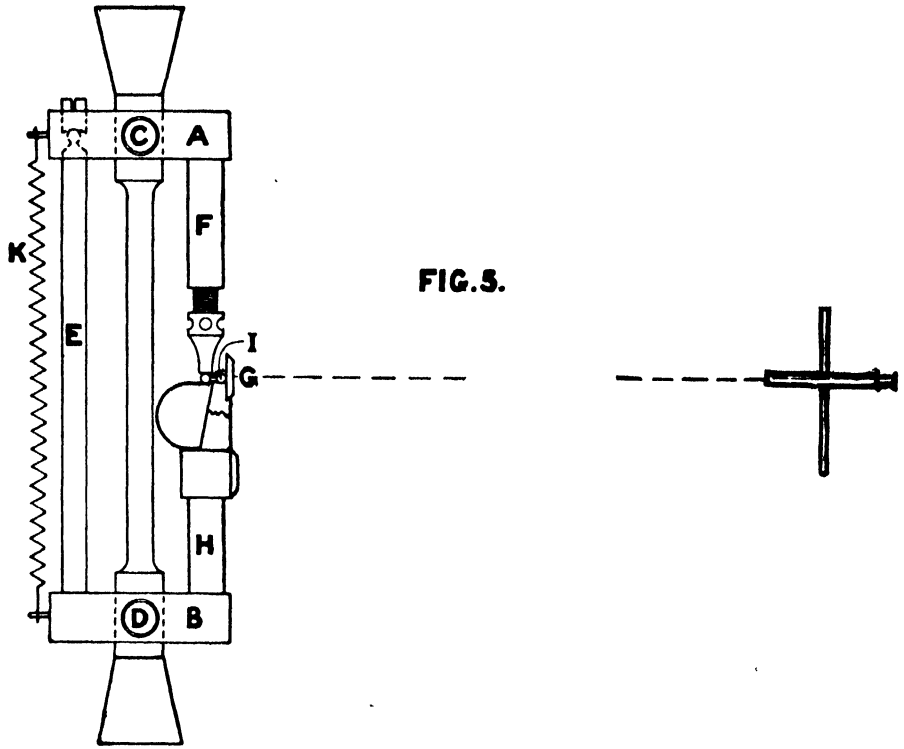


FIG. 3.

It was found that after putting the piece in the testing machine (having previously compressed it in the compression machine) the zero was at first not constant; that is to say, after the application and removal of a load of, say, 2 tons per square inch, whose effects as regards elastic hysteresis must be quite negligible, the reading did not return to the same value. After a few applications and removals of this small load, however, the instrument seemed to settle down and the full tension load was then put on and removed, the corresponding change of length being noted.

In order to make quite certain that the change of length observed as the result of application and removal of tension in a piece which had previously been compressed was really due to the elastic properties of the material, and not in any way to the extensometer, a control experiment

was performed. The piece was put in the tension machine, and a load equivalent to 10 tons per square inch was applied and removed a number of times. The piece was then removed from the machine, and handled as nearly as possible in the same way as after its removal from the compression machine in the experiments which have been described. It was then replaced in the tension machine and treated again in the same way, that is, a load of about 2 tons per square inch was applied and removed about a dozen times, so that the zero became perfectly constant. The full tension load of 10 tons per square inch was then applied and removed, and the consequent change of length was noted. It will be seen that in this control experiment every circumstance is exactly the same, except that, prior to its commencement, the piece was loaded in tension instead of in compression, and was brought into the cyclical state corresponding to the application and removal of 10 tons per square inch tension. It was found that the apparent change of length in the control was usually unmeasurable and was never more than about 10 per cent. of the change occurring in the full cycle from compression to tension. It may be inferred from this, and from the general agreement between the experiments, that the width of the hysteresis loop over a range of 20 tons per square inch has certainly been measured correct to within 10 per cent., or, say, to within 1/100 of a ton per square inch.

It should be added that it was found necessary, for the prevention of buckling, to enclose the test-piece when undergoing compression in a closely fitting jacket. This jacket was formed by casting type-metal round the piece, the casting being split so that it could be removed. The jacket was, of course, taken off when the piece was removed from the compression machine and put into the tension machine. The amount of compressive stress applied was measured with sufficient accuracy for the purpose by means of the extensometer. In this way the effect of friction between the jacket and the piece was eliminated from the measurement of stress.

10

A NEW TORSION-METER.

[By B. Hopkinson and L. G. P. Thring. "Engineering," June 14th, 1907.]

With the increasing use of turbines for marine propulsion the problem of directly measuring the power delivered to a shaft by the engine has become of pressing importance. Several instruments for this purpose have been designed and have proved more or less satisfactory, the best known being that of Messrs Denny and Johnson, which is now considerably used. All these instruments measure the twist in a length of the shaft, whence, knowing the rigidity of the shaft and the speed of revolution, the horse-power transmitted can be at once deduced. In Messrs Denny and Johnson's torsion-meter, and in the others with which the authors are acquainted, it is necessary, in order to secure reasonably accurate results, that a considerable length of shafting be taken. In Messrs Denny and Johnson's apparatus two wheels are clamped to the shaft as far apart as is conveniently possible, and the relative displacement of these two wheels produced by twisting of the shaft is measured by an electrical device. According to figures given by Mr Archibald Denny at the meeting of Institute of Naval Architects in March, 1907, the displacement at the periphery of the wheels can be determined correct to 1/100 in.; a remarkably good result, considering the nature of the means employed. If we assume that the wheels are three times the diameter of the shaft, and that the shear in the latter at its periphery, when running fully loaded, is 1/2000, the corresponding displacement at the surface of the wheels if placed 50 ft. apart will be $3 \times 50/2000 = \cdot 075$ ft., or 9/10 in. Thus, under these circumstances, an error of 1/100 in. in measuring the relative displacement of the two wheels amounts to about 1 per cent. on the full-load twist of the shaft. If a length of only 10 ft. were available, the corresponding possible error would be 5 per cent. It may be assumed, therefore, that a length of the order of 10 ft. is the minimum which will give good results with this form of apparatus; and Mr Denny, at the meeting referred to, stated that they rarely made use of less than 15 ft. or 20 ft. of shafting. This requirement, while not militating greatly against the use of the apparatus in big ships where there is plenty of room round the shafting, and considerable lengths are easily accessible, seriously restricts its application in small vessels. It should be observed that with a given ratio of wheel diameter to shaft diameter, the length of shafting required to secure a given percentage of accuracy is independent of the diameter of the shaft; for all shafts when fully loaded are sheared by

approximately the same amount at the periphery. Thus two points a given distance apart on the surface of any shaft will be relatively displaced by a constant amount, independent of the diameter of the shaft, which may be taken as about one two-thousandth part of their distance apart. It is this relative displacement, magnified in the ratio of the wheel diameter to the shaft diameter, which is really measured by these torsion-meters.

In the authors' apparatus, described in this paper, the length of shaft in which the twist is measured may be reduced to about 12 in., the twist being measured correct to 1 per cent. of the maximum which the shaft can transmit in normal working. Figs. 1 and 2 show one of the earliest forms that were tried; it was subsequently altered considerably in detail, and is only given here because it is easy to follow the principle in it. The sleeve A is clamped to the shaft by screws in the plane 1, 1. The collar B is similarly clamped in the plane 2, 2. On the collar is mounted a mirror M which is pivoted in a frame carried on the collar, so that it can turn about an axis at right angles to that of the shaft. The axis of the mirror carries a short arm which engages with a flat-plate C carried on the free end of the sleeve. Twist of the shaft between the planes 1, 1 and 2, 2 causes the plate C to move relative to the supports of the mirror, thus tilting the latter about its axis. When the shaft is in the position shown, the mirror, which is concave, forms (on a ground-glass screen) at D an image of a straight filament lamp placed at E. When the shaft twists so that the mirror turns, this image is displaced to D' through a distance which is proportional to the relative motion of sleeve and collar, or to the twist in the shaft between the planes 1, 1 and 2, 2. If the shaft is revolving, the image is still formed momentarily once in each revolution, and its position can easily be seen. Assuming that the distance between the planes 1, 1 and 2, 2 is 12·5 in., and that the shear at the surface of the shaft is 1/2000 (which about corresponds to normal full-load running), the relative

Fig. 1.

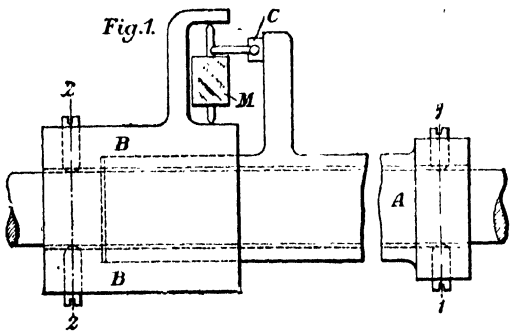
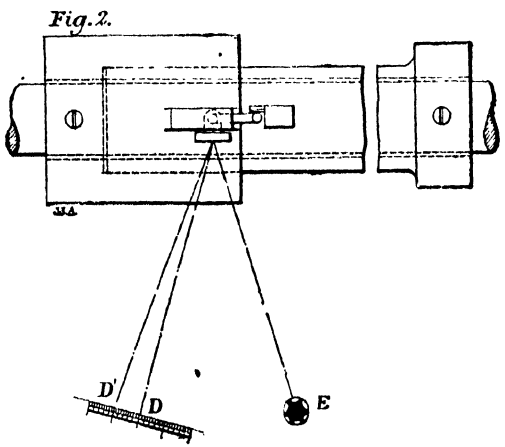


Fig. 2.



displacement of the points of attachment of the sleeve and collar will be 0·00625 in., and this, as already explained, will be the same whatever the diameter of the shaft. In the apparatus constructed by the authors the distance of the actuating plate C on the collar from the centre of the shaft is 2·9 times the shaft radius. The movement of the plate will then be 2·9 times that of the surface of the shaft, or 1·8 hundredths of an inch. The corresponding angular displacement of the mirror will depend upon the radius of the arm; in the apparatus, as constructed, this is 0·3 in., and the mirror is tilted through an angle of $\frac{0{\cdot}018}{0{\cdot}3} = 0{\cdot}06$ of a radian. The angle through which the reflected beam is displaced will be double this, or 0·12 of a radian. Thus the linear displacement of the image corresponding to full-load running will be 0·12 of the distance from the screen to the mirror. Good definition can be obtained if the latter is 60 in., giving a displacement of 7·2 in. With an ordinary spectacle lens, silvered to form a mirror, it is quite easy to read the position of the image correct to 1/20 in., which corresponds to less than 1 per cent. of the maximum working torque for which the shaft is designed.

It is evident, therefore, that so far as magnification is concerned, the twist in a short length of shaft can be read quite sufficiently accurately by this method. It was found, however, that in the form of apparatus described above, when used on small shafts, the bearing between sleeve and collar was apt to be strained in putting the instrument on or by bending of the shaft. Friction was thus set up, which caused error in reading, partly from the resulting deformation in the instrument, and partly because the shaft was relieved of part of the torque which was transferred to the instrument. The attempt at a complete constraint between the two members was therefore abandoned, and a partial constraint was substituted which permitted motion in certain ways, and so practically eliminated friction. At the same time the mirror and actuating-plate were so disposed that relative motion of sleeve and collar, other than pure twist, produced no motion of the mirror. Such motions necessarily occur in ordinary working, in consequence of bending of the shaft, and as the motion of the actuating-plate is only about 1/50 in., they may cause serious errors unless eliminated. The manner in which this result has been secured will be apparent from the detailed description of the apparatus as used, which follows.

In the engravings, Figs. 3 to 5, A is the free end of the sleeve, and B the collar, each being made with deep flanges for stiffness. They are split into halves, so that the apparatus can be fitted over the shaft when in place. Three screws with rounded ends are used for clamping: one set at the end of the sleeve, and another set passing through the central plane of the collar. An extension of the sleeve passes through the collar and engages with the round ends of the screws G, G carried on the collar, being held

up into engagement with them by means of a spring H. The end of the sleeve is otherwise quite free. The elasticity of the sleeve and the slight "give" in its clamping-screws are sufficient to allow of its coming to a bearing on the screws G, G without much pressure on the spring H, even when the shaft is slightly bent. The mirror is carried in a frame fixed to the flange of the collar in such a manner that the mirror axis passes through the shaft axis. The mirror axis carries a small steel ball at its outer end, which engages with the plate C. This latter is fixed to an arm carried on the sleeve flange and passing through a hole in the collar flange, and its face is parallel to the shaft axis. The ball on the end of the mirror is approximately (in theory it should be exactly) in the plane of the screws G. With this arrangement it is obvious that any relative movement of plate and mirror, except that due to twist, will either be prevented by the bearing against the screws G, or will be innocuous, owing to its being in the plane of the plate C. Thus bending of the shaft in the plane of the mirror axis will merely cause the plate to slide over the ball at the end of the mirror arm; shortening of the shaft due to thrust has a similar effect; bending in a plane at right angles to the mirror axis is also without effect, because the ball at the end of the mirror is in the plane of the two screws G, G. When the two halves of the sleeve and collar are taken apart they are kept in their relative position by the rods D, D, which, while stiff enough for this purpose, are sufficiently elastic not to affect the reading when the apparatus is on the shaft.

Fig 3.

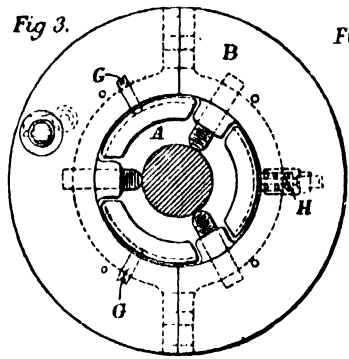


Fig 4.

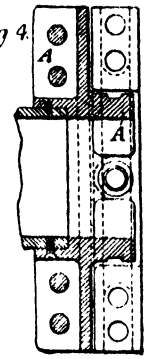
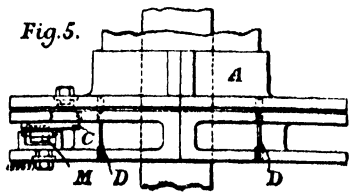


Fig.5.



The apparatus was first tested statically on a shaft of the same diameter, and probably of the same material, as the turbine shaft upon which it was ultimately to be used. This shaft was $3\frac{3}{8}$ in. in diameter, and was mounted in the works of Messrs J. I. Thornycroft and Co., Limited, with arrangements for applying a torsional load equal to about half that which it was to transmit when at work. It was found that the dynamometer reading was proportional to the load within 1 per cent. of the maximum applied throughout the whole range. The absolute twist in the shaft was also measured direct by pointers carried at the two ends. The modulus of rigidity for this shaft was thus found to be 11,800,000 lbs. per square inch. There were slight signs of hysteresis, but these were not of any practical importance. The instrument was then fixed on the port wing shaft of the

torpedo-boat *Greenfly*, which is driven by the low-pressure turbine. The readings were taken during the Admiralty trials of the boat on March 25, 1907. On that day the boat did circling trials, and stopping and starting trials, so that the changes in power on the shaft were frequent and severe. With full steam in the turbine the shaft ran at 1230 revolutions per minute, and the reading on the ground-glass screen, which was placed 57 in. from the mirror, was then 8·75 in., corresponding to 1100 horse-power. The deflection could be read easily correct to 1/20 in., or less than 1 per cent. The zero was determined by allowing the propeller to drag the turbine round with a good vacuum in the casing. Two readings were taken, first with the boat going ahead, and, secondly, with the boat going astern, with moderate speed in each case. The mean of these readings, which differed by 4/10 in., were taken to be the zero. In the first half of the day's run the zero was found to have changed by 0·35 in., corresponding to an error of 4 per cent. in the torque at maximum power. It is probable that this change was due to a slight shift of the scale, which was rigged up on packing-cases in a very temporary fashion. Just before running home after the trials, the zero was again carefully determined, and the boat then ran at full power for half an hour, care being taken to fix the positions of the lamp and scale. It was found that during this run the zero had not shifted by so much as 1/20 in. Altogether the apparatus was running round for about eight hours, and was not touched in any way during that time, except once when the clamping screws were slightly tightened. It was brought down on the morning of the trial and fitted on to the shaft in about half an hour. The maximum length of shaft exposed in this boat is about 15 in., and the clearance between the shaft and the vessel's plates is there about 8 in.; this is just sufficient to get the apparatus in. There is, of course, no independent means of checking the accuracy of the results obtained on a trial of this kind. But the facts that, when tested statically, the deflections were proportional to the twist of the shaft, and that when running the zero did not change, leave no doubt that the power readings when running must also have been correct to within 2 per cent., and probably within 1 per cent. of the maximum power transmitted.

The apparatus can be fitted on to a shaft of any size smaller than the bore, within reasonable limits; that designed for a 3½ in. shaft works quite satisfactorily on a shaft of 2 in. diameter, and would, no doubt, work down to 1½ in. The over-all diameter is about 2½ times the diameter of the largest shaft it will take. As regards length, the greater the length, of course, the greater the accuracy; but a length of 1 ft. or 2 ft. is ample. This length will suffice, whatever the diameter of the shaft, but the larger the shaft the easier is the accurate measurement of the power; because, while the linear displacements to be measured are the same, the disturbances due to bending and vibration become less.

The instrument, of course, measures the instantaneous value of the

torque in the shaft at the moment when the reflected beam from the mirror strikes the screen. If the torque varies in the course of a revolution, as it will do with a reciprocating engine, it is necessary to measure it at several points and to take the mean. This can be done by using several mirrors placed at intervals round the apparatus; or by the use of more than one screen. In a turbine-driven shaft the torque will be fairly uniform, though it will doubtless vary slightly with the position of the propeller blades in relation to the hull and the water surface.

Since carrying out the above-described tests we have learned that Mr Herman Frahm, of the firm of Messrs Blohm and Voss, Hamburg, has been working at the same problem as that with which we have dealt, and has arrived, quite independently, though a little later in point of date, at a very similar solution. We understand that apparatus of the kind designed by Mr Frahm has lately been tried on a German steamer, and has proved satisfactory. Mr Frahm's apparatus is apparently the same as ours in principle, but differs from it in detail to a certain extent.

The apparatus as we have designed it can be easily applied to factory shafting, and will, we hope, prove useful for this purpose as well as in marine work, especially to those who have to put in electric motors for driving such shafting, and so require to know the exact power taken by a particular machine or set of machines.

11

NOTES ON THE MEASUREMENT OF SHAFT HORSE-POWER.

["Transactions of Institution of Naval Architects," Vol. LII. 1910, p. 184.]

Relation between Twist and Torque.

The measurement of shaft horse-power in a turbine-driven vessel is based on the reading of a torsion-meter which measures the angular twist of some portion of the shaft. The twist is proportional to the torque in foot-lbs. transmitted by the shaft, and, in order to determine this torque, it is necessary to know the constant ratio which it bears to the angular twist. This connecting factor is usually measured directly by mounting the shaft on bearings before it is placed in the vessel, applying a twisting couple by means of levers and weights and measuring the corresponding twist by means of the torsion-meter. This plan has obvious advantages, and will probably be adopted wherever it is possible. In some cases, however, it may be impossible, or at least very inconvenient, to carry out a test of this kind, and it will then be necessary to calculate the constant for the shaft from its dimensions and the modulus of rigidity of the material of which it is made. The author has made, or has seen the results of, a considerable number of static tests of shafting which go to show that, if the modulus of rigidity be assumed to be 12,000,000 lbs. per square inch, the stiffness of the shaft so calculated is nearly always correct within about 4 per cent. The following figures, which have been furnished to the author by Messrs Siemens Bros., illustrate the variations in rigidity which may occur.

No. of shaft	Modulus of rigidity $\times 10^{-6}$		No. of shaft	Modulus of rigidity $\times 10^{-6}$	
	By pointers	By torsion-meter		By pointers	By torsion-meter
1	12·10	12·40	9	12·10	11·88
2	11·88	12·44	10	12·01	12·35
3	12·07	12·13	11	12·05	11·94
4	11·90	12·65	12	12·00	12·23
5	12·23	12·33	13	12·22	12·09
6	11·88	12·07	14	11·94	12·08
7	12·26	12·24	15	11·93	12·03
8	12·09	11·96	16	11·91	12·03

Of the sixteen shafts referred to, the first twelve were 9⅞ in. diameter, and the last four 10¾ in. diameter. Each shaft was mounted in the builders'

yard and loaded by levers and weights. The twist was observed by means of long pointers fixed to the shaft at distances ranging in different cases from 7 ft. 6 in. to 14 ft., and also by a Hopkinson-Thring torsion-meter which measures the twist over a length of about 3 ft. The modulus of rigidity was calculated in the usual way from the twist and torque so measured, and is of course affected by any errors in measuring the dimensions of the shaft. With reasonably careful work the outside diameter should be correct within 1 part in 1000, corresponding to 1 in 250 in the modulus. The bore will not always be equally accurate, but has less effect on the result.

It will be seen that in all but one case out of the sixteen the modulus, by whichever method it be measured, is within 4 per cent. of 12,000,000. It may also be observed that the twist as measured by the torsion-meter frequently differs considerably from that registered by the pointers under the same conditions of stress, the difference reaching 6 per cent. in one case.

The author has little doubt that the twist recorded by the torsion-meter under these conditions of static loading was correct within 1 per cent., and, although he was not present at these trials, he thinks it probable that a similar degree of accuracy was reached with the pointers. If this be so, the results indicate that the twist per foot on the 3 ft. length covered by the torsion-meter may sometimes differ by as much as 4 per cent. from the mean twist over several times that length, which is registered by the pointers. In other words, the variation of rigidity from point to point of the same shaft is of the same order of magnitude as the variation from one shaft to another. Hence the increase of accuracy obtained by calibrating the shaft before it goes into the vessel, instead of proceeding by calculation with an assumed modulus, will be largely illusory, unless the calibration be performed with the torsion-meter fixed at that point on the shaft where it is going to be used.

The circumstances of calibration of the shaft differ from those obtaining in the vessel in that there is no end thrust. In the ship, when the shaft is working at full power, the end thrust may reach 1000 lbs. per square inch. It is a question of some importance whether the presence of this thrust affects the relation between torque and twist. Such an effect might conceivably arise in either of two ways. There might be a direct effect of longitudinal stress upon the modulus of rigidity; a shaft under compression might twist either more or less for a given increment of torque than when free. Such a change of modulus implies a deviation from Hooke's Law, but it is not impossible that it exists, though it does not appear to have been observed except to a very minute amount. On the other hand, longitudinal thrust or tension may produce twist in the shaft in the absence of torque, and this twist due to the thrust will be added to the twist produced by torque, the resultant twist being the sum of these two elements. To put the matter in symbols: the twist, instead of being represented by aT, where T is the torque in foot-lbs., and a a constant dependent on the

modulus of rigidity and the dimensions, must be represented by $aT + bR$, where R is the longitudinal stress. The kind of result which would be obtained in the calibration of a shaft such as this is shown in Fig. 5, Pl. I, in which OB represents the calibration line when the shaft is twisted in the ordinary way without end thrust, while CD represents a similar line when a thrust is applied. The thrust is supposed to be applied when there is no torque in the shaft giving rise to a small twist OC, and the line CD, which is parallel to OB, shows the increment of twist consequent on the application of torque, the thrust being maintained constant. The dotted line OD would then give approximately the calibration of the shaft when an increasing torque is applied concurrently with a proportionately increasing thrust. The result, for the practical purpose of measuring shaft horse-power, is much the same as though the modulus had been increased, though physically it cannot be so regarded.

The production of twist by means of a longitudinal pull or push, in the absence of any torque, implies a peculiar structure in the shaft, which may perhaps be best described as a helical arrangement of the fibres. A rope when pulled tends to untwist, and circumstances of manufacture or treatment may sometimes produce an analogous structure in a metal rod or shaft. It seems improbable that the ordinary processes of manufacture of a propeller shaft could have any such result, but it cannot be regarded as impossible without more experimental evidence than we at present possess. Overstraining by the application of a torque exceeding the elastic limit would certainly produce a structure of the kind required; and, though it is most unlikely that such overstraining would occur after the shaft is finished, there is at least a possibility that local treatment in forging may have a similar effect. The question requires further experimental investigation on full-sized shafts*. The experiment would consist simply in applying a pure end thrust or tension to a shaft and ascertaining whether or no any measurable twist is produced thereby.

NOTE (added March 31, 1910). Since the above was written, Mr Thring has made some experiments upon the effect of longitudinal stress on torsional rigidity. These experiments are not yet complete, but the results have an important bearing upon the subject of this paper, and it has therefore been thought well to incorporate a short account of them so far as they have gone.

The experiments consist in suspending a rod vertically, the lower end being quite free and the upper end gripped so as to prevent twisting, and applying tension by means of weights hung on the lower end together with a twisting couple. The couple is applied by means of a wheel fixed near the lower end, at opposite diameters of which are attached strings leading over pulleys to weights, the strings lapping round the circumference of the wheel and leading off in opposite directions. The tensions in the strings are the same, and it will be seen

* Mr Hamilton Gibson has recorded some observations on a large shaft which show an effect which may have been of this kind; though the observations are apparently also consistent with a change of modulus. *Trans. I.N.A.*, March, 1907.

that by this means torque is applied without the possibility of any friction. The twist in the rod is measured by two mirrors fixed to it at points about 20 in. apart. Each mirror reflects its own image of a straight filament lamp on to a graduated transparent screen placed a few feet from the rod, and the twist of the length between the mirrors is proportional to the relative movement of the two images.

Two mild steel rods (Union Brand) have been used, each about $\frac{3}{8}$ in. diameter. It was found that hanging a weight of 200 lbs. on the lower end giving a tensile stress of about 1800 lbs. per square inch did not in either case cause any appreciable twist. It is quite certain that the shearing strain at the surface, if any, produced by this longitudinal tension, does not exceed 1/200,000 or 1 per cent. of the surface strain ordinarily found in a propeller shaft at full load. When torque was applied sufficient to give a surface shearing strain of rather over 1/2000 (equivalent to full working torque in a shaft) the application and removal of the load of 200 lbs. again produced no appreciable effect. There was a slight apparent change in the twist, but it amounted to less than 1 per cent., and it is probable that even this small change, which seemed to be somewhat uncertain in amount, was due to slight bending effects or other accidental errors of measurement which could not be entirely eliminated. It is quite certain that in the case of these two rods longitudinal stress has no practical effect upon the relation between twist and torque.

Mr Thring is continuing his experiments with a view to discovering whether, by any treatment such as overstrain, it is possible to put the rod into such a condition that pure tension will produce twist. The results so far indicate that in order to obtain any such result it will be necessary for the amount of overstrain to be excessive. A permanent twist of a quarter of a revolution in a 3 ft. length had no perceptible effect. It may be noted that in the article on "Elasticity" in the *Encyclopaedia Britannica* Lord Kelvin gives the results of experiments, similar to those of Mr Thring, carried out upon wire, from which it appeared that, if the wire were stretched nearly to its elastic limit, its modulus of rigidity was altered by about $\frac{1}{2}$ per cent. In view of this experiment and of the results obtained by Mr Thring, it may perhaps be considered as proved that longitudinal stresses of the order of 2000 lbs. per square inch affect the rigidity modulus of a rod or wire by less than 1 per cent., and probably by a great deal less. The question whether and under what circumstances tension or compression may produce twist must perhaps be left open for the present, and it is hoped that Mr Thring's further experiments may throw some light upon it. It may be pointed out, however, that if there ever be any such effect it must be of a purely accidental character, and will vary in direction and amount from one shaft to another.

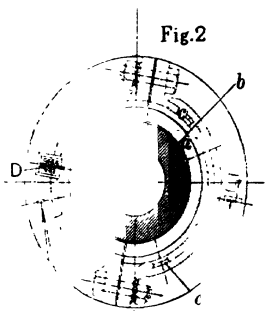
Variation of Torque in a Revolution.

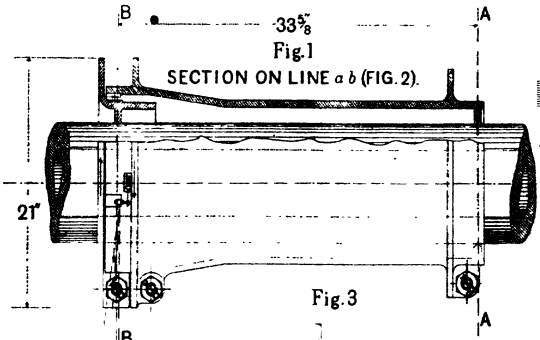
Most of the torsion-meters which have come into practical use measure the twist which instantaneously exists in the shaft at one or more definite points in a revolution. For example, the Denny-Johnson instrument measures the twist at the moment when the small magnet carried on the wheel passes the coils connected to the telephone, and in the instrument designed by the author and Mr Thring the measurement is made at the moment when the flash is on the screen. In the simplest form of such instruments the measurement is made at one point only, and it has then

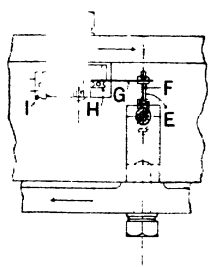
to be assumed for the purpose of calculating the horse-power that the instantaneous torque at this single point fairly represents the average during the revolution. Such an assumption, though obviously erroneous where reciprocating engines are used, has much probability of truth when the motive power is a steam turbine, because there is then no variation in turning moment due to the engine. Even here, however, it is possible that the varying resistance experienced by the propeller due to the changing positions of its blades in relation to the surface of the water or the hull of the vessel may cause appreciable changes of torque. The author has not seen any published results of observations bearing on the question whether such changes are of practical importance, though probably such observations have been made. He thinks it therefore desirable to place on record some results of this character, partly obtained by himself and partly by others under conditions of which he has an intimate knowledge. It may be said at once that the general effect of these observations is to support the conclusion that a single point measurement of the torque in a turbine-driven propeller shaft sufficiently nearly represents the average over a revolution, unless in quite exceptional circumstances.

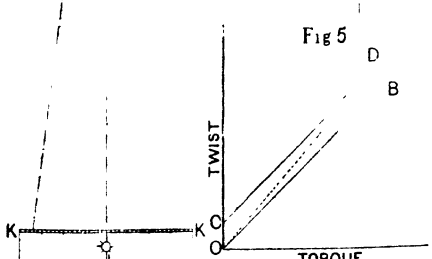
The general principle of the Hopkinson-Thring torsion-meter with which these observations were made is probably familiar to most of those who are interested in the measurement of shaft horse-power. A short description of the latest pattern of this instrument as manufactured by Messrs Siemens Brothers may, however, be interesting.

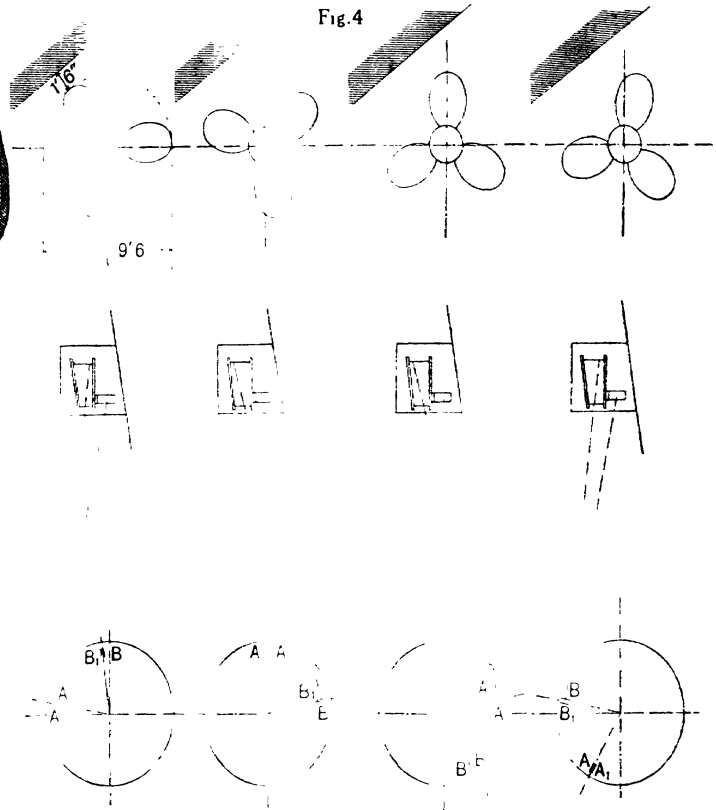
Figs. 1 and 2 show in plan, part section and end elevation, the size of meter used for shafts from 8 to 12 in. in diameter. Referring to Fig. 1, the sleeve is clamped on to the shaft in the plane AA and the collar in the plane BB. Two bearing plates cc (Fig. 2) are carried on the inside of the sleeve and are kept in engagement with the corresponding plates on the collar by means of the spring D, which is fixed to the sleeve and pressed against the collar. The sleeve and collar are thus kept concentric, and the twist of the shaft between the planes AA and BB appears as relative motion of the opposed flanges of the sleeve and collar as shown by the arrows (Fig. 1). In order to render this relative motion visible, a mirror is used, which is shown in detail in Fig. 3. A pair of mirrors facing opposite ways are carried in a metal frame which can turn on pivots about an axis E, which is fixed relative to the collar. This frame carries an arm F, which is connected by means of a flexible spring G to a block H fixed to the flange of the sleeve. When the shaft twists the relative motion of the sleeve and collar flanges causes the mirror to turn about the axis E as shown by the arrow. The spring strut G, while retaining its length practically invariable, and so making the motion of the end of the arm the same as the relative motion of the sleeve and collar, is able to bend slightly, and so to take up the angular movement of the arm. A screw nut I serves to give slight movement to the block H relative to the flange, and thus to adjust the


Fig.2
b
D
c


B
33⅝"
A
Fig.1
SECTION ON LINE a b (FIG. 2).
21"
B
A


Fig.3
I
H
G
F
E


Fig 5
D
B
TWIST
K
K
C
O
TORQUE


Fig.4
1'6"
9'6"
B1
B
A
A
B1
E
A
B
B1
B
A
A1

zero reading of the mirror to any desired point. By means of the same screw, the pitch of which is accurately known, it is also possible to determine, after the instrument is fixed in place, the deflection on the scale corresponding to a given relative movement of the two flanges. A sheet metal cover, not shown in the drawing, is fixed to one flange on the edge and is carried right round, completely enclosing the space between the flanges except for the small openings necessary for showing the mirrors. The mirrors are thus protected from accidental damage and from oil and steam, etc. The movement of the mirror is observed by means of a straight filament lamp J, an image of which is formed by the mirror on a ground glass screen K placed parallel to the axis of the shaft. It is obvious that the movement of this image across the scale will be proportional to the twist in the shaft, and, further, that for a single mirror this image will be thrown upon the screen at one point only in the revolution. In the standard fitting, as now supplied, however, there are, as already stated, two mirrors placed back to back in the same frame, and there will therefore be two reflections, one from each mirror, corresponding to two points in a revolution which are nearly, but not quite, 180° apart (see Fig. 2). These reflections, though occurring intermittently and at different times, will, if the speed of revolution exceeds a moderate amount, appear as continuous lines on the screen. They are, however, easily identified by an arrangement of the screen with clear glass slots, of which it is unnecessary to give particulars. Identification is rendered easier by the fact that with the same direction of twist the two reflections move opposite ways.

It is obvious that by putting on more mirrors at different points round the circumference the number of observations in the course of a revolution may be increased, and in the set of experiments made by the author with the special object of measuring the change of torque in a revolution due to propeller action, a second pair of mirrors was added, so as to secure four observations in a revolution. These measurements were made on a wing shaft which it was thought would be most likely to show changes of torque of the amount contemplated, because the propeller blades come rather close to the hull of the vessel, and, it was supposed, might then experience a considerably augmented resistance. The disposition of mirrors is shown in Fig. 4, which gives the position of the propeller blades at the instant when each of the four reflections passes the screen. It will be seen that the points of observation are fairly evenly distributed over the cycle of relative position of blade and hull, which, of course, occupies one-third of a revolution. Readings were taken at several different speeds and powers, but no difference could be certainly detected between the four mirrors. The actual figures for nearly full power and for about one-third of full power were as follows:

Mirror	A	A_1	B	B_1	Mean
Full-power readings ...	89	90	89	90	89½
One-third of full power	42	43	41	42	42

The zeros were obtained by trailing. It will be seen that, assuming the mean of the four mirrors to be correct, the error in the torque obtained from any single one is not more than about 1 per cent. of the full-power torque.

It is, of course, possible that the actual changes in resistance experienced by the propeller are greater than these figures would indicate, but that these changes are met by the flywheel action of the propeller. The relation between the actual change in the instantaneous couple exerted by the water on the propeller and the corresponding change recorded by the torsion-meter will depend upon the relation between the period of variation of the couple, which with a three-blade propeller will be one-third of that of the revolution of the shaft, and the natural period of torsional oscillations in the shaft, being given (in theory) by the formula

$$\frac{n^2}{n^2-1},$$

where n is the ratio of these two periods. In the case of the vessel in which these experiments were made, the author had not complete data for calculating the natural period, but it was probably about $\frac{1}{10}$ second. At full speed the period of change of couple would be $\frac{1}{15}$ second. From the fact that these periods are not very greatly different, and that, so far as the author was able to observe, there was no marked change in torque at a speed of about 200 revolutions per minute (at which n would be equal to 1, and resonance would occur), the author is inclined to conclude that there was really very little difference in the actual resistance experienced by the propeller at different points, and that the absence of indications of such difference in the torsion-meter readings was not merely due to the flywheel effect of the propeller. It is, of course, possible that with a four-blade propeller the changes might be more marked.

The general conclusion that the torque does not vary perceptibly in a revolution in ordinary cases is supported by a number of other observations. It has already been said that the standard form of the Hopkinson-Thring torsion-meter always gives at least two readings in a revolution. Observations have now been taken with this instrument on a considerable number of shafts in different vessels, and, so far as the author is aware, no case has yet been recorded in which the readings of either mirror at full power differ by more than 2 per cent. from the mean of the two, and the difference rarely exceeds 1 per cent. In these trials the instruments have been fixed on at random without any definite relation to the position of the propeller blades, and the number of observations which has been made is such that in all probability any cause of variation of torque which is of at all frequent occurrence would before now have been detected. There is, of course, still the possibility of occasional disturbance in consequence of resonance, but the cause of this would probably be promptly recognised, because it would occur only at one particular speed.

12

THE MAGNETIC PROPERTIES OF IRON AND ITS ALLOYS IN INTENSE FIELDS.

[By Sir ROBERT A. HADFIELD, F.R.S., AND B. HOPKINSON, F.R.S
"PROC. INSTITUTE OF ELECTRICAL ENGINEERS," 1911.]

THE magnetic properties of materials in fields of very high density are of great scientific interest because the relation between B and H which, in the moderate fields employed in practice, or in the ordinary methods of testing, is so complicated and dependent upon so many variables as to defy analysis, then assumes a very simple form. According to the molecular theory of magnetism, as developed by Ewing, the force which opposes the tendency of the magnetic molecules to set themselves in the direction of an externally applied magnetic force is that due to their mutual magnetic actions upon one another. The system of forces caused by these interactions plays a large part in determining the molecular configuration assumed under moderate magnetic forces, and this system is dependent upon the previous history of the material, on stress, temperature, and on other physical conditions. The molecular theory, while it can give a good account of the general behaviour of the material under moderate forces, clearly forbids us to expect that the magnetic properties under such conditions can be quantitatively expressed in simple terms. On the other hand, it predicts equally clearly that if the externally applied force be so great as completely to swamp the effects of the mutual interactions, the magnetic properties will become very simple. Practically the whole of the magnetic molecules then set themselves in the direction of the magnetising force, without regard to their mutual actions. The relation between B and H becomes

$$B = H + 4\pi I;$$

where I, in the terms of the theory, is the sum of the magnetic moments of the molecular magnets contained in unit of volume.

If it can be assumed that the moment of a molecular magnet is unaffected by the magnetic forces to which it is subjected, the quantity I, in the above equation, is a constant so far as variations of H are concerned, though it might still be affected by temperature and any other physical conditions which can affect the properties of a separate molecule. The experiments of Ewing and Low* and of Du Bois† show that in iron, nickel and cobalt and some alloys, B does in fact exceed H by a nearly constant

* *Philosophical Transactions*, A, vol. 180 (1889), p. 221.
† *Philosophical Magazine*, vol. 29 (1890), p. 293.

amount when H lies between about 2000 and 25,000 C.G.S. units. These experiments, while they do not negative the possibility of some dependence of the molecular magnetic moment on the external force, give strong grounds for supposing that, at any rate in the materials referred to, the mutual attractions between the molecules are almost completely overpowered by a force of 2000 C.G.S. units, and that if the force is not more than 25,000 the magnetic moments of the molecules are unaltered by it. The saturation value of $4\pi I$, the constant quantity by which B exceeds H within these limits of the latter quantity is then as definite a physical constant for the material as is the density. Like the density, it may possibly vary with temperature and other physical conditions, but it will probably be unaffected by any change which merely alters the relation of the molecules to one another, and does not alter the internal arrangements of separate molecules. The saturation value of I, expressing as it does the quantity of magnetisable matter in the material, must be regarded as a fundamental magnetisation constant. We propose to call this constant the "magnetism" of the material; or where any confusion can arise with other uses of the word, the "specific magnetism." It appeared that an examination of a series of alloys of iron with carbon and with other metals in fields of very high intensity might lead to results from which some general deductions could be drawn, not only as to magnetic properties, but also as to the constitution of these bodies. The great advantage of the method for this purpose lies in the fact that the saturation value of $4\pi I$ in a material consisting of a mixture of substances, is dependent only upon the relative proportions and on the magnetic properties of the several constituents separately, and not on their arrangement in the mass.

Most steels consist of such a mixture of constituents, the arrangement of which is an important factor in the results of magnetic testing of the ordinary kind. The arrangement of the constituents as revealed by the microstructure is dependent upon a great variety of circumstances, and this makes it hopeless to look for any simple quantitative relation between the magnetic properties of an alloy as ordinarily tested and its composition. When the alloy is saturated, however, this complication disappears. Let it be supposed that the magnetic moments of the molecules of the several constituents present are respectively μ_1, μ_2, μ_3, ... and that the numbers of these molecules per unit volume of the material are respectively n_1, n_2, n_3, ... Then, if H be above the saturation value, but not so large as to affect μ_1, μ_2, ... we have $I = n_1 . \mu_1 + n_2 . \mu_2 + \dots$.

The same result may be expressed without using the language of the molecular theory if it be supposed that the magnetisms of the several constituents are I_1, I_2, ... per unit of mass, and that m_1, m_2, ... are the masses of the several constituents present per unit mass of the mixture. In that case

$$I = m_1 I_1 + m_2 I_2 + \dots . \qquad \dots\dots(1)$$

This relation is not dependent upon any molecular theory of magnetisation, but only on the assumptions that there are definite constituents which are mechanically mixed, and that for each there is a definite saturation value of I.

The material for the research was at hand in the form of a series of alloys prepared at the Hecla works. The alloys have already been the subject of extensive investigation, and many of their physical properties are known. In particular, magnetic tests by the magnetometer method have been carried out on many of them, the force used ranging up to 40 C.G.S.* In composition the alloys cover a wide range, including iron-carbon steels, nickel steels, and manganese steels, with varying proportions of the added elements.

Summary of Results.

The following are the most important results obtained from the work so far as it has progressed at present:

1. Every alloy examined, without exception, has a definite saturation intensity of magnetisation. In most cases this is reached in a field of 5000 units, but in a few there is a small increase, as between 5000 and 25,000. The forms of the curves of magnetisation connecting I and H in these exceptional materials, however, leave no doubt of the existence in them also of a saturation value of I. Every one of the materials tested, without exception, behaves as though it consisted of a mixture of magnetic substances with non-magnetic substances having a permeability not differing materially from unity. This holds good also for the non-magnetic or nearly non-magnetic nickel or manganese alloys, and, in this respect, our results differ from those obtained by Ewing and Low—according to whose experiments a nearly non-magnetic manganese steel had a constant permeability of about 1·4. Among all the alloys which we have tested there is none having a constant permeability differing from unity by more than 2 per cent.

2. There is in the series no alloy having a higher specific magnetism than pure iron.

3. The saturation value of I in absolute units for pure iron of density 7·80 is 1680 within 1 per cent. This is slightly lower than the values obtained by Ewing and Low and other experimenters.

4. In an annealed iron carbon steel, in which other elements are present in small proportions, the specific magnetism is less than that of pure iron by a percentage equal to six times the percentage of carbon. This result constitutes a verification, in the case of annealed iron-carbon alloys, of the linear relation (1) above connecting the magnetism of a mixture with the magnetisms of its constituents. In this case there are two constituents,

* Barrett, Brown, and Hadfield, *Proceedings of the Institution of Electrical Engineers*, vol. 31 (1902), p. 674.

mechanically mixed, viz., pure iron and iron-carbide, the percentage of iron-carbide being 15·5 times that of the carbon in the steel. It is readily deduced from equation (1) that the magnetism of carbide of iron is about two-thirds of that of pure iron.

5. Quenching an iron-carbon alloy from a high temperature reduces the specific magnetism by a large but somewhat uncertain amount.

6. The addition of silicon or aluminium to iron results in a reduction in specific magnetism which is roughly in proportion to the amount added as though the addition behaved as an inert diluent. If carbon be present, however, silicon seems to neutralise its action to some extent. For instance, an alloy containing 2·28 Si and 0·67 C is 3·6 per cent. less magnetic than pure iron, whereas if the carbon had its full effect as in iron-carbon alloys, and the silicon were simply an inert diluent the reduction would be 6·3 per cent.

7. The observations on the alloys of iron with nickel and with manganese, or with both these elements, have failed to reveal any simple relation between their magnetism and their composition. It is probable that this is due to the peculiar effects of heat treatment on these substances. When these effects are sufficiently known it may be possible to apply to the manganese and nickel alloys the same kind of magnetic analysis as we have applied to the carbon steels.

The Electro-magnets.

The general dimensions of the magnet on which most of the tests were made are shown in Fig. 1. The vertical limbs are of rectangular section, 10 cm. by 20 cm. The pole-pieces are cylindrical and are made a good fit in holes at the top of the vertical limbs, so that they can slide axially, and be clamped in any position by means of set-screws. The yoke is wound with about 1600 turns and each pole-piece with about 400 turns of No. 14 s.w.g. wire.

For producing the highest fields the magnet was fitted with the pair of pole-pieces shown in place in Fig. 1. With the tips of these $\frac{1}{4}$ in. apart, and with a current of 30 amperes passing in the coils of the magnet, the two parts of which were placed in parallel, a field of about 20,000 c.g.s. could be obtained in the space between the flat ends of the pole-pieces. The distribution of the field was determined by means of concentric annular test coils and was found to be constant within 1 per cent. over a circle of 2·5 mm. radius. By increasing the current to 60 amperes, which, however, could only be kept on for a short time, the intensity of the field could be increased to 25,000 c.g.s.

Lower fields ranging up to 10,000 c.g.s. were obtained by replacing the conical pole-pieces, shown in Fig. 1, by a pair of flat pole-pieces having flat faces 1 in. square. With these placed $\frac{1}{4}$ in. apart with their faces parallel

a uniform field of about 7000 C.G.S. was produced over an area 20 mm. square with a current of 5 amperes in the magnet coils.

The material was for the most part in the form of rods about 5 mm. diameter. In most cases it was rolled into this form, but a few pieces were forged or cast. The magnetic test-pieces were turned down from these rods into little cylinders $\frac{1}{8}$ in. diameter. They were, in most cases, $\frac{1}{4}$ in. long. The same testing coil was used for all the pieces. It consisted of sixteen turns of No. 38 wire (diameter over insulation 0·057 mm.) wound on a brass bobbin which just fitted over the test-piece. A second coil also of sixteen turns was wound outside the first with a layer of paper between the surfaces, to measure the magnetising force in the neighbourhood of the specimen. As it was not possible to make sufficiently accurate measurements of the dimensions of such small coils, the effective areas

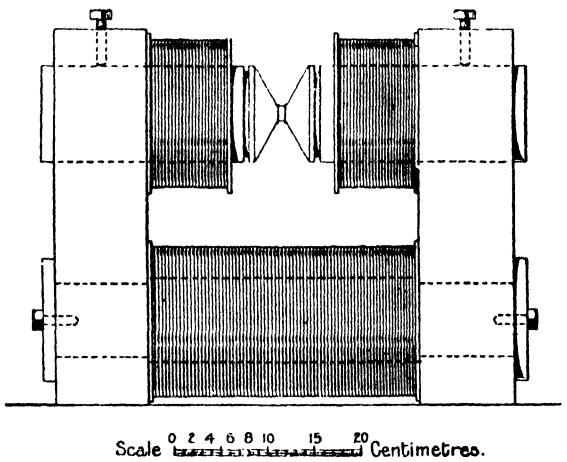


Fig. 1.

were determined by magnetic measurements. The flat pole-pieces were fixed in position about $\frac{1}{4}$ in. apart, and a coil of twelve turns wound on a brass former about 15 mm. diameter was inserted between them. The fling obtained when this coil connected to the ballistic galvanometer and a current of about 5 amperes reversed in the magnet coils, was observed. The test coil was then inserted and connected to the galvanometer and the fling again taken with exactly the same exciting current. The area of the larger coil could be accurately calculated from its dimensions, and that of the smaller deduced from the ratio of the flings after reducing them to the same resistance in the galvanometer circuit. An enlarged section of the specimen, bobbin, and coils when in place is shown in Fig. 2.

The galvanometer used in most of the tests was of the moving coil type made by Nalder Bros., and had a period of about 16 seconds. A full account of its calibration is given in Appendix I.

In order that the ballistic galvanometer may correctly record the flux

change in a coil connected to it, it is necessary that that change should be substantially complete within a period which is small compared with the natural period of oscillation of the galvanometer. In our experiments the flux change was caused by the reversal of the current in the coils of the magnet, and was considerably retarded by the effects of eddy currents and of self-induction. With the flat pole-pieces and a current of 5 amperes, 5 per cent. of the flux change had still to be completed at the end of 2 seconds. The effect of this was fully investigated, and it was found that while measurements of the absolute value of the flux density based on the moving-coil galvanometer might be subject to an error of several per cent., this error did not materially affect the determination of I. This matter is discussed in detail in Appendix II; it will suffice to say here that the results obtained with the moving-coil galvanometer were checked by comparison with similar measurements on a suspended magnet instrument (Broca type, made by Cambridge Scientific Instrument Company) of much longer period (about 30 seconds). Measurements of I for the same piece of iron obtained with this galvanometer agreed within $\frac{1}{2}$ per cent. with those given by the shorter period instrument*.

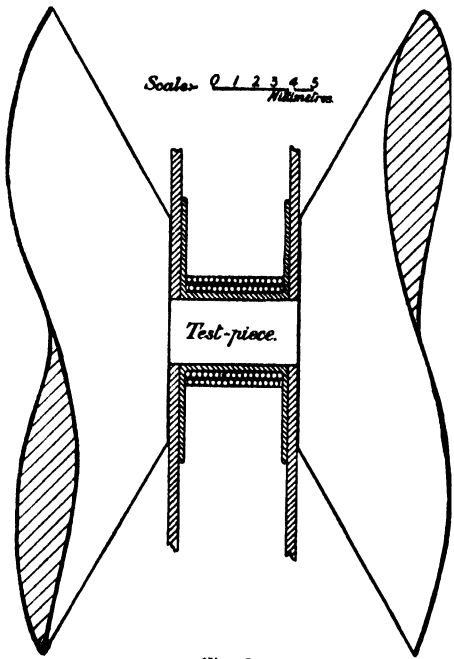


Fig. 2.

In making a test the pole-pieces were gently butted up to the ends of the test-piece and fixed in position. The testing coil was first inserted with a brass dummy in place of the test-piece, the current in the magnet coils was reversed, and the fling observed on the ballistic galvanometer which was connected to the inner coil. The brass piece was then replaced by the iron test-piece, the current again reversed—care being taken that it should have exactly the same strength as before—and the fling again observed.

Let

S' be the area of the testing coil, and S the area of the test-piece, each multiplied by the number of turns in the coil—in this case 16.

* Added February 10, 1911. Since writing the paper this has been further confirmed by comparison of the effect of reversal with that of withdrawing the piece from the field. The two methods agree within $\frac{1}{2}$ per cent.

I the intensity of magnetisation in the test-piece.

H the value of the magnetising force before the test-piece is inserted.

H' the value of the magnetising force after the test-piece is inserted.

Then the flux-change corresponding to the fling obtained with the brass dummy is HS', and that corresponding to the fling obtained with the test-piece is $H'S' + 4\pi IS$. The difference D of the two flings will therefore, on the same scale, represent

$$4\pi IS - (H - H')S'.$$

Since the current and the distance between the pole-pieces is exactly the same whether the dummy or the test-piece be in position, the magnetising force will be the same except in so far as it is disturbed by the introduction of the test-piece. The disturbance so produced is nearly proportional to $4\pi IS$. Ultimately, therefore, the difference of the two flings is proportional to $4\pi IS$ and the values of I for two different specimens are in proportion to these differences. This comparison is, to a high degree of accuracy, independent of any knowledge of the correction $H - H'$, and the comparison of I for two specimens based upon it does not involve any accurate knowledge of the area S' of the testing coil, but only a knowledge of the area of the test-piece. The latter can be accurately measured, and it is probable that the ratio of the magnetisms of two steels can be measured by this method with an accuracy of one part in two hundred.

For determining the absolute value of I, however, it is necessary to obtain the absolute value of $H - H'$, the reduction of the magnetising force caused by the introduction of the piece. This was found to be comparatively small in the low fields obtained with flat pole-pieces, but is of serious amount when the conical pole-pieces are used, and a good deal of time was devoted to its determination. The effect of the introduction of the test-piece upon the distribution of magnetic matter which determines the magnetising force is confined to the immediate neighbourhood of the ends of the test-piece, and consists in the addition of a volume and surface distribution of magnetisation in that neighbourhood. The additional distribution consists of three parts, that is:

1. Over the circular patch covered by the end of the test-piece the surface magnetisation disappears; which may be represented by supposing that there is an additional distribution of density $+ I$ over this patch which annuls the density $\mp I$ present before the test-piece was inserted.

2. There will be a volume density of magnetisation just under each end of the test-piece, and within the conical pole-pieces due to the spreading of the lines of induction in the latter after they emerge from the test-piece. The general effect of this will be opposite to that of No. 1 and is probably nearly equivalent to that of a surface distribution of magnetism of uniform density over the circular patch.

3. The fringing of the lines of induction at the end of the test-piece gives rise to a surface distribution on the cylindrical surface of the latter near to the end, and associated with this there is a diminution in the surface density in an annulus on the flat face of the pole-piece near to the specimen, where the fringing lines re-enter the iron.

Of these three additions, Nos. 1 and 2 are probably the most important, and the general effect of all may be represented with a considerable degree of accuracy as being the same as that of two circular patches of magnetism of uniform density on the parts of the pole-faces covered by the test-piece. It is easy to calculate the magnetic effect of two such patches in any coil of given dimensions*, and by measuring the absolute amount of the diminution of flux in the annular space between two coils surrounding the iron specimen produced by the introduction of the specimen, the appropriate density of magnetisation can be determined. For this purpose two testing coils were used; one (A) has already been described and is figured in Fig. 2. In the other coil (B) as in coil A there were 16 turns on the inner winding and 16 turns on the other, so that by placing these windings in opposition the flux in the annular space between them could be determined. The inner winding B was wound on a former of the same size as the specimen, and the outer on an ebonite bobbin which was just large enough to be slipped over the inner coil on its former. The former was then withdrawn so that the iron test-piece could be inserted. It will be seen that in coil B the inner winding is very much closer to the specimen, and that a larger average effect will be produced in the annulus than in the case of coil A. It was found that the reduction in magnetising force in the annulus of either coil was very closely the same as that which would be caused by a patch of magnetism of density $\pm 0{\cdot}8I$ on each end of the test-piece. The experiment consisted simply in keeping the position of the magnet-poles fixed, exciting the coils with a current of about 30 amperes, and observing the fling produced by reversal first with a brass dummy, and secondly with a piece of Low Moor iron in position. The experiment was repeated a great number of times, brass and iron being inserted alternately in order to eliminate accidental differences between the conditions with and without the iron test-piece. The following table shows the average results which are certainly correct within half a division, together with the differences calculated on the above supposition that they are due to two patches of magnetism of density $\pm 0{\cdot}8I$.

	Reduction of fling in annulus coil		Correction on D	
	A	B	A	B
Observed ...	9½	15½	—	—
Calculated ...	9½	14½	20½	15½

In the same table are shown the corrections which must be applied in the case of each coil to D which is the increase in the fling due to the inner

* See Appendix III.

coil when iron is introduced in place of the dummy. The corrections are equivalent to the quantity $(H - H')S'$, and when added to the observed value of D give the fling corresponding to $4\pi IS$ for the specimen. In the case of the piece used in this experiment, the value of D with coil A was 121 divisions, and with coil B 124 divisions, so that the corrected values of the fling would be $141\frac{1}{2}$ and $139\frac{1}{2}$ respectively. These, of course, ought to be the same, and for finally calculating the correction it has been assumed that the true value of $4\pi IS$ is $140\frac{1}{2}$ for this piece. The correction to be applied with coil A is then $19\frac{1}{2}$ divisions or 13·9 per cent. of $4\pi IS$. In other words, the observed fling must be multiplied by 1·16 in order to arrive at the fling corrected for end effect.

The above calculation of the correction for end effect is based on the assumption that it may be regarded as due to a uniform distribution over the two circular ends of the specimen. This, of course, is not quite accurate, but that it is not far from the truth is shown by the fact that the surface density which must be assumed in order to account for the observed changes in H is four-fifths of I, from which it appears that the most important term in the added distribution is that due to the blotting out of the magnetism on the parts of the surface covered by the test-piece, and for this part, of course, the assumption of uniform distribution is very close. That the manner of distribution of the magnetism does not make a great difference in the result, however, can be shown by taking an alternative, but probably much less accurate, assumption, namely, that the whole of the added magnetism may be supposed to be concentrated at the centre of the circular patch. In that case the differences to be expected in the annulus coils A and B and the corresponding corrections to be added to D as determined from these coils are as follows:

	Reduction of fling in annulus coil		Correction on D	
	A	B	A	B
Calculated ...	$9\frac{1}{4}$	$13\frac{1}{2}$	$23\frac{3}{4}$	$18\frac{1}{2}$
Observed ...	$9\frac{1}{4}$	$15\frac{1}{2}$	—	—

The values of $4\pi IS$ obtained by applying this correction would be $144\frac{3}{4}$ and $142\frac{1}{2}$ respectively, the mean being $143\frac{1}{4}$ as against $140\frac{1}{2}$ when the correction is calculated on the assumption of uniform distribution. It seems probable that the latter assumption leads to a value of $4\pi IS$, which is within 1 per cent. of the truth. The fact that the correction is four-fifths of that produced by the patches of magnetism on the ends of the test-pieces, shows that it must be closely proportional to the values of $4\pi IS$, and a correcting factor 1·16 has been applied for all the specimens.

In the case of the lower fields obtained with the flat pole-pieces the correction for the ends can be determined by direct experiment. In this case it may be regarded as equivalent to an addition to the length of the test-piece due to the fact that the pole-pieces, which before the introduction

of the test-piece are but slightly magnetised (the maximum flux density under these conditions is 10,000 C.G.S.) become fully saturated over a short distance within the metal just under the parts covered by the ends of the test-piece. The joints between the test-piece and the pole-pieces slightly increase this effect. In order to determine the amount of the correction in this case the following experiment was made. Two pieces of Low Moor iron were turned up, each $1\frac{1}{2}$ in. long and $\frac{1}{8}$ in. in diameter. The value of D was determined for each of these pieces, the pole-pieces being separated by a distance of $1\frac{1}{2}$ in. to admit it and the current in the magnet coils being increased to about 30 amperes, which gave a field of about 5000 C.G.S. when the piece was not there. The middle, $\frac{5}{8}$ in. of the piece, was then cut out and the value of D again determined for the shortened piece. Finally the length of this piece was reduced to $\frac{1}{4}$ in., being the central quarter of the original $1\frac{1}{2}$ in., and the value of D determined for this short length. The magnet current was adjusted in each case to give H about 5000 C.G.S. The results are shown in the following table:

	Length of piece:	$1\frac{1}{2}$ in.	$\frac{5}{8}$ in.	$\frac{1}{4}$ in.
Fling D	1st piece ...	$230\frac{3}{4}$	229	227
	2nd piece ...	232	$231\frac{1}{4}$	$229\frac{1}{2}$

from which it appears that the value of D for a piece $1\frac{1}{2}$ in. long is about $1\frac{1}{2}$ per cent. greater than for a piece $\frac{1}{4}$ in. long. For an infinitely long piece, in which the end effects would be absent, the difference would be rather under 2 per cent. This is the correction which has been applied in all cases to tests in the low fields.

In the first series of systematic tests of the alloys the value of H', the magnetising force within the piece, was determined by measuring the flux density in the annular space between the two layers of the coil. This was the method adopted by Ewing and Low, who measured the flux density in an annular space surrounding the neck of the piece and assumed that it was the same as that in the piece. In their case, at any rate when using highly magnetic materials, the assumption was probably nearly correct, for the pole-pieces were so shaped as to give a very uniform field with the iron specimen in place. We, at first, made a similar assumption, but the investigation which is outlined above shows that it is not in accord with the facts when applied to the intense fields produced by our conical pole-pieces. The field in the annulus is diminished by the insertion of the test-piece owing to the influence of the circular patches of magnetism, but it is not diminished so much as is the magnetising force in the centre of the iron. Calculated on the above assumption that the end effect may be represented as due to two circular patches of magnetisation on the ends of the piece of density $\pm 0{\cdot}8I$, the diminution in the average magnetising force in the annulus amounts to $0{\cdot}047 \times 4\pi I$. If, therefore, H' (the average value of the axial component of magnetising force over the area of the inner coil)

be assumed to be reduced in the same proportion as the magnetic force in the annulus, the correction $(H - H')S'$ will amount to

$$0{\cdot}047 \times 4\pi IS \times \frac{S'}{S} = 0{\cdot}079 \times 4\pi IS,$$

whereas it should be $0{\cdot}139 \times 4\pi IS$. Thus the value of $4\pi IS$ will be underestimated by 6 per cent. if H' be taken as given by the field in the annulus.

When the flat pole-pieces and more moderate fields are used, there is also a slight reduction in the annulus field when the piece is inserted. This was found by experiment to amount to about 1/200 of $4\pi IS$ with a piece $\frac{1}{4}$ in. long and a field of about 7000 C.G.S. Here again the value of H' is rather less in the iron than in the annulus; a correction of $1\frac{1}{2}$ per cent. has still to be added to the estimate of $4\pi IS$ based on the assumption that the magnetising force is the same in the annulus as in the test-piece.

At a later stage in the research another magnet was brought into use. The limbs and yoke of this instrument were formed of transformer plates, insulated by sheets of paper, the section of the iron being 32 sq. cm. They were wound with two coils of 2200 turns each No. 18 S.W.G. copper wire. The pole-pieces were solid and of Low Moor iron, and had flat opposed faces 3·8 cm. in diameter. Two testing coils were used, each consisting of 16 turns of No. 30 S.W.G. copper wire wound on a thin brass tube. The two coils were connected together with their axes parallel. The areas of these coils were adjusted to exact equality, which could be tested by connecting them in series and in opposition between the parallel faces of the magnet poles and reversing the current. They were used differentially, the piece to be tested being inserted in one of the coils. The fling in the galvanometer is then proportional to $4\pi IS$ for the piece, subject, however, to the end correction. The special advantage of this method is that the magnetising force H is automatically deducted from the observed B at the moment of measurement, instead of being determined in a separate experiment. This is of especial importance where it is desired to test an alloy which is nearly non-magnetic. It was easy in this way to detect magnetism amounting to less than 1/200 part of that of pure iron and to measure the magnetism of a nearly non-magnetic alloy to within that amount. In some cases where it is desired to test an alloy which is highly magnetic, a standard piece of pure iron can be put in the other coil; the difference between the magnetism of the alloy and that of pure iron is then measured directly by the galvanometer fling. The galvanometer can be calibrated at any moment by taking a fling with the pure iron standard against air, and the magnetisms of the other pieces then reduced to percentages of that of pure iron.

In what follows this magnet will be called the "small" magnet to distinguish it from the "large" magnet described above, with which most of the experiments were made.

The Effect of Field Strength on the Value of I.

The value of I was determined with the large magnet for every one of the alloys in a field of about 8000 C.G.S., and also in a field of over 20,000 C.G.S. Particulars of these measurements are given later, but it will be convenient here to give details of some trials which were made with the special object of finding how nearly I could be regarded as constant. These trials may be regarded as typical of all the tests, and the figures (which are given in full) will convey some idea of the order of accuracy attained and of the method of reduction.

It should be explained that when the highest current (60 amperes) was used, the ordinary method of complete reversal was not available because the magnet coils heated too much while the galvanometer needle was being steadied preparatory to reversal. The reversal was therefore

Trial of Low Moor IV. by B.H., June 27, 1907.

Nalder galvanometer; period, 15·35 seconds. Constant, $k = 3{\cdot}74 \times 10^{-7}$. Additional resistance, 4 ohms. Total resistance of galvanometer circuit, 8·24 ohms. Galvanometer constant, 198*. Conical pole-pieces, $\frac{1}{4}$ in. apart; current, 60 amperes.

Test-piece	Fling $C'b'$ or Cb	Fling $A'b'$ or Ab	Total	D
Brass ...	$139\frac{1}{2}$	143	$282\frac{1}{2}$	$113\frac{3}{4}$
Iron ...	$140\frac{3}{4}$	$255\frac{1}{2}$	$396\frac{1}{4}$	
Brass ...	$138\frac{3}{4}$	$142\frac{1}{2}$	$281\frac{1}{4}$	115
Iron ...	$140\frac{1}{4}$	256	$396\frac{1}{4}$	
	Mean value of D			114·6

Value of D corrected for end effects $114{\cdot}6 \times 1{\cdot}16 = 133$.

performed in two parts. Fig. 3 shows diagrammatically the cyclical changes of H, and of the flux in the testing coil ($HS' + 4\pi IS$), when it encloses an iron specimen and the current is varied between $\mp$ 60 amperes. After two or three preliminary reversals, the current was broken, leaving a residual magnetising force represented by OA. The galvanometer coil was then brought to rest with the circuit closed and the reversing switch thrown over. The current was then made, bringing H to the point B, and the corresponding fling observed. This corresponds to flux change $A'b'$ in the piece. The galvanometer circuit was opened, and the magnet current then broken. This left H at the point C and the test-piece at point C'. The galvanometer was again steadied and the current made again in the same direction (viz. without throwing over the reverser), thus bringing

* Viz., the number which, multiplied into the fling gives the total flux embraced by the coil.

H back to the point B. The fling $b'd'$ corresponding to total reversal is equal to the sum of the separate flings $A'b'$ and $C'b'$. The difference of the flings is the flux induced in the piece by the residual magnetisation of the magnet, the end connections cancelling out. When the brass dummy is substituted for the iron piece, the changes of flux in the coil (neglecting end corrections) are represented by the curve $DABC$, and the difference fling D is equal to $b'd' - bd$.

The observations given on page 132 were taken in fairly quick succession, iron and brass being inserted in the test coil alternately and the pole-pieces not moved.

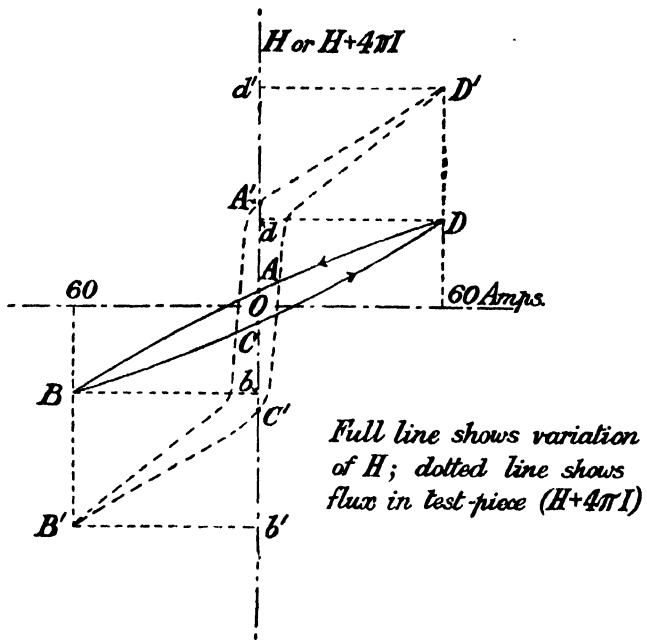


Fig. 3.

The flat pole-pieces were now substituted for the conical pole-pieces, and the value of D again determined when a current of 4·9 amperes was reversed. The procedure was the same as before, brass and iron being inserted alternately and the current kept constant within 1 per cent. It is unnecessary to give all the figures. The means were as follows:

Iron gave a fling of	...	...	$212\frac{1}{2}$ divisions
Brass dummy ...	...	...	84 ,,
		D =	$128\frac{1}{2}$ divisions

Value of D corrected for end effects $128\frac{1}{2} \times 1{\cdot}02 = 131$.

The difference between the two values of D is $1\frac{1}{2}$ per cent., and is not more than can be accounted for by errors of observation.

Taking the mean of the two values of D, we have

$$4\pi IS = \frac{132 \times 396}{2} = 26{,}100 \text{ lines.}$$

The diameter of the piece is 0·316 cm. and the sectional area is 0·0784 sq. cm. Hence

$$S = 16 \times 0{\cdot}0784 = 1{\cdot}255,$$

and

$$4\pi I = 20{,}800,$$

$$I = 1658.$$

The area of the coil (S') is 2·13 sq. cm. The value of the magnetising force with the brass dummy in position with the conical pole-pieces and a current of 60 amperes is

$$H = \frac{282 \times 396}{2 \times 2{\cdot}13} = 26{,}200.$$

The effect of inserting the iron is, as already explained, to reduce this by about $0{\cdot}09 \times 4\pi I$, or, say, 1900 C.G.S. The actual magnetising force acting on the iron is, therefore 24,300 C.G.S., and the flux density B is about 45,000. The force produced by the residual magnetism of the magnet when the current is cut off is

$$\frac{3\frac{1}{2}}{282} \times 26{,}200 = 325 \text{ C.G.S.},$$

and the value of $4\pi IS$ in this residual field is found by taking the difference of the two flings $A'b'$ and $C'b'$ and subtracting $3\frac{1}{2}$, the corresponding difference with the brass piece. In this case the difference of the flings is $115\frac{1}{2}$, leaving 112 as the fling corresponding to $4\pi IS$ in a field of 325. In this field, therefore, the value of I for this Low Moor iron has reached $\frac{112}{132} = 85$ per cent. of its saturation value. The value of B when $H = 325$ is 18,000, which agrees closely with that found by J. Hopkinson for wrought iron*.

With the flat pole-pieces, H worked out in the same way is about 7800 C.G.S. This estimate, however, is subject to a correction for the time of growth of flux. More probably H was about 5 per cent. greater than this, or, say, 8200.

The use of so large a current as 60 amperes is attended with considerable difficulties, and the necessity of making the measurement in two stages greatly increases the probable error. It was therefore given up after all the pieces had been tested by its means, and a current of 30 amperes was used for all further comparisons. With this current it was possible to use complete reversal and the magnetising force H was only about 12 per cent. less than that reached with 60 amperes. It is unnecessary to give the figures of trials made in this way, as they differed in no respect except as regards the pole-pieces and current used from those made with the flat

* *Philosophical Transactions*, vol. 176 (1885), p. 455.

pole-pieces. A very large number of trials were made with different materials, and with one or two exceptions they agreed in showing that the value of D was sensibly the same whether the test were made in a field of 22,000 or one of 5000 to 8000 lines. As showing the order of accuracy of these conclusions, reference may be made to a series of determinations made by Mr Quinney on a piece of nearly pure iron. These measurements were carried out at different times and were all independent, the pole-pieces being shifted occasionally between the tests. Seven determinations of D in a field of about 8000 C.G.S. (flat pole-pieces 5 amperes) gave a mean of 138·0 scale divisions, the maximum being $138\frac{3}{4}$ and the minimum $137\frac{3}{4}$. Adding 2 per cent. for the end correction, the corrected value of D is 140·9. In seven similar measurements in a field of about 21,000 C.G.S. (conical pole-pieces 30 amperes) the mean value of D was 120·7, the maximum being $121\frac{1}{2}$ and the minimum $119\frac{1}{2}$. The corrected value of d given by these tests is $120{\cdot}7 \times 1{\cdot}16 = 140$, which differs by 0·7 per cent. from the other.

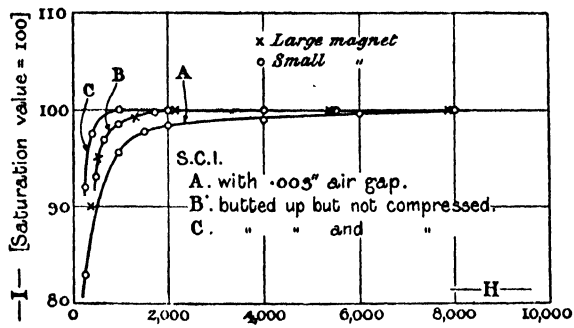


Fig. 4.

Taking due account of the possibility of an error of 1 per cent., arising from the end correction in the high field, we feel justified in concluding that the value of I in nearly pure iron is the same, certainly within 2 per cent. and most probably within 1 per cent., when $H = 21{,}000$ as it is when $H = 8000$.

This result receives additional confirmation from a study of the way in which the state of saturation is approached. This is shown in Fig. 4, which refers to pure iron (S.C.I.). The piece was tested between the flat pole-pieces of the larger magnet and with varying magnetising currents and also in the small magnet. The results agree very well, but are affected by the joints at the ends when H falls below 2000. It will be seen that the change in I when the effect of the joints has been reduced by compression is hardly perceptible until H is less than 1000. Similar curves, to which reference will be made later, were obtained from a number of other materials. With two or three exceptions, the state of saturation was reached within 1 per cent. at $H = 5000$.

The Saturation Value of I for Pure Iron.

Among the materials available for examination was a sample of Swedish iron (maker's mark, S.C.I.) containing less than 0·2 per cent. of impurities. A special study was made of this specimen with the object of determining the value of I in absolute measure for pure iron. Most of the tests were made on a cylinder of the following dimensions:

Length, 6·26 mm.
Mean diameter at middle, 3·18 mm.
Weight, 0·385 gramme.
*Density (calculated from weight and dimensions), 7·74.

A large number of independent measurements of the value of $4\pi I$ were made for this piece in a field of about 8000 C.G.S. in the manner already described. On each occasion the constant of the galvanometer was determined by means of a standard inductance or a standard condenser. The following table summarizes the results of measurements made by three different observers and with the two galvanometers†:

Observer	Galvanometer	$4\pi IS$
J. H.	Nalder	26,800
B. H.	Broca	26,800‡
H. Q.	Broca	26,800
H. Q.	Nalder	26,600

In the above the end correction of 2 per cent. has been applied to readings of D in all cases. In one test made with the conical pole-pieces in a field of about 21,000 C.G.S., the value found was 26,600. This experiment was made with the Nalder galvanometer, and is in satisfactory agreement with those recorded above for the lower field.

The mean sectional area of this piece is 0·0794 sq. cm. correct within 1 per cent., and S is 16 times the sectional area or 1·270 sq. cm. The mean value of I obtained with the Broca galvanometer is 1680, and with the Nalder galvanometer 1675.

* According to Brown (*Transactions of the Royal Dublin Society*, vol. 9 (1907), p. 59) the density of S.C.I. is 7·877. Our measurements of density depend on estimating the average sectional area of a piece only 3 mm. diameter, which is not easy to measure and may be in error by 1 per cent The error will generally be in excess because there is a tendency to measure outside diameters and no account is taken of flats or slight hollows. This, however, is not enough to account for the wide difference, amounting to 2 per cent., between the density here found and the value given by Brown. It is possible that there are local differences of density which may amount to 1 per cent. and which become apparent in these small test-pieces.

† The Nalder galvanometer was of suspended coil type, with a period of about 15 seconds the Broca was suspended magnet and its period was about 30 seconds.

‡ Mean of four independent measurements, ranging from 26,100 to 26,500.

A test was also made on another piece of the same iron $\frac{5}{8}$ in. long. Flat pole-pieces were used with a current of $10\frac{1}{2}$ amperes, which gave a field of about 7000 C.G.S. In this case the correction for the ends would be 2/5 of that with the $\frac{1}{4}$ in. piece, or rather less than 1 per cent. The value of $4\pi IS$ determined with the Broca (long period) galvanometer was 27,000. The dimensions of the piece were:

Length, 15·92 mm.
Mean diameter, 3·19 mm.
Weight, 0·99 gramme
Density (calculated), 7·78

whence the value of I is 1680.

Finally reference may be made to some experiments made towards the end of the present research by Mr E. F. Clark, Advanced Student in the Engineering Laboratory, Cambridge. He used a new magnet with limbs built up of transformer plates and solid pole-pieces having parallel opposed faces 2 in. in diameter. The test-pieces were $1\frac{1}{2}$ in. long by $\frac{1}{8}$ in. diameter. The measurements were made differentially, in the same manner as those in the "small" magnet. He used the Nalder galvanometer, and in the circuit was included the secondary coil of a standard inductance, so that a direct calibration independent of resistance measurements, damping, etc., could be performed at any moment by reversing the current in the primary. Using the same Swedish iron in a field of 5000 C.G.S. he found for four different pieces values ranging from 1680 to 1690, the mean being 1685. The length of the specimen precludes all end effects, the lamination of the magnet eliminates any uncertainty due to eddy currents, and finally the differential method of working and the direct calibration greatly reduce casual errors of observation. On every ground these measurements are to be regarded with considerable confidence, and it is very unlikely that the mean value found by Mr Clark is in error by so much as 1 per cent.

A number of trials were also made on pieces of Low Moor iron, some in absolute measure and some in comparison with the pieces of pure Swedish iron. In most cases the value of I was perceptibly less than for the purer iron, and in none was it perceptibly greater. The difference did not in any case exceed 2 per cent. Mr E. F. Clark tested five different pieces, and found values ranging from 1659 to 1677, the mean being 1667, or about 1 per cent. less than the value which he found for S.C.I. The following are some values of this constant found by other observers:

Ewing and Low (Low Moor iron, isthmus method), 1630–1740 (mean value 1700)*.

Du Bois (optical method $H = 2500$), 1700–1750†.

Gumlich (isthmus method) (electrolytic iron $H = 6000$), 1725‡.

* *Philosophical Transactions*, A, vol. 180 (1889), p. 242.
† *Philosophical Magazine*, vol. 29 (March, 1890), p. 263.
‡ *Elektrotechnische Zeitschrift*, vol. 30 (1909), p. 1065.

The last-mentioned determination is the most recent, and it will be seen that it exceeds our value for S.C.I. by 2½ per cent. In part, this may be due to difference in the material. It is quite possible that the density of the electrolytic iron may have been rather greater than that of the S.C.I. which we used. Having regard, however, to the close agreement which we have found between different varieties of nearly pure iron, we think it probable that there is a small systematic error in one or both sets of measurements. Such an error is most likely to occur in the estimation of H. In Gumlich's experiments, as in those of Ewing and Low, the value of H was measured by determining the flux density in an annular air space immediately surrounding the test-piece or isthmus. If the pole-pieces are truncated cones having the same vertex and an angle of about 80°, and if the cylindrical isthmus fills up the whole space between the flat ends, then the magnetising force due to magnetism on the conical surfaces is approximately uniform within the isthmus and for a little distance outside it. There will be some magnetism on the cylindrical surface of the isthmus near the ends which, though of small amount, may on account of its close proximity considerably affect the value of H within the piece. Subject to this correction, the amount of which is difficult to estimate, the value of H within the piece will be equal to that measured just outside. Ewing and Low used pole-pieces of the shape described, and it seems probable that in their measurements the field was in fact nearly uniform. The arrangement of Gumlich's pole-pieces and test coil is shown in Fig. 5. The conical poles, though they are of about the correct angle to give a uniform field, have not a common vertex, and the field might be far from uniform. The field was explored with test coils when the piece was removed and found to be substantially uniform under these circumstances. But, as we have found in our experiments, exploration of the field when the iron isthmus is removed throws but little light on the distribution when it is there. It is quite possible that the value of H within the piece in these experiments was greater than in the annular space. If this were the case, it would lead to an overestimate of I.

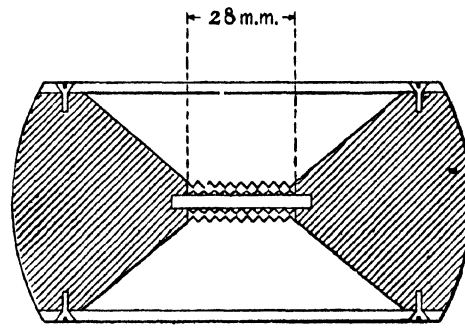


Diagram of test-piece, 3 mm

Fig. 5.

Alloys of Iron: General.

The following was the course of the systematic testing of the whole series of iron alloys. A test-piece was prepared of each sample about ⅛ in. diameter and ¼ in. long. The value of D for every one of these pieces

was measured by Mr J. Hirst, first with flat pole-pieces and a current of about 5 amperes and then with conical pole-pieces and a current of 60 amperes. The value of H in the first case was about 8000 C.G.S., and in the second from 24,000 C.G.S. for pure iron to 26,000 C.G.S. for non-magnetic alloys. In the experiments with the conical pole-pieces and a current of 60 amperes the procedure was as described above, the flux-change corresponding to reversal being determined in two parts, by observing the fling on making the current first after reversal and then after breaking without reversal.

In this series of tests the value of H was measured by determining the flux change in the annular space between the concentric coils of bobbin A, which were connected to the galvanometer in opposition. It was at first assumed that the magnetising force in the annulus might be taken as equal to that in the test-piece, but it was found that on this supposition the value of I as obtained by Mr Hirst with the intense field given by the conical pole-pieces was uniformly less than that found with the flat pole-pieces and a more moderate field. This led to a further investigation of the effect of the ends, the principal results of which are given above. It appeared that the result of taking the magnetising force inside the test-piece as equal to that in the annulus was to cause the value of I to be under-estimated by about 5 per cent. in the tests with conical pole-pieces.

The whole of Mr Hirst's results were then re-calculated, with proper corrections for the ends. As a result of this re-calculation a close agreement was in most cases established between the saturation value of I obtained in the high and low fields. The following table gives a statistical summary of the results*. The first column shows the number of pieces in which the difference between the high and low field values of I lies within the limits shown by the second column. The difference is expressed as a percentage of the saturation value of I for pure iron, 1 per cent. representing about 16 absolute units:

Number of cases	Difference between high and low fields†
6	exceeding 3 (max. difference 5·4)
7	+ 2 to + 3
9	+ 1 to + 2
5	− 1 to + 1
9	− 2 to − 1
1	less than − 2 (max. difference − 2·3)

The galvanometer fling could be read correct within one division, which corresponded to about 12 absolute units in I or 0·75 per cent. of the saturation value for pure iron. Differences between high and low fields amounting to $1\frac{1}{2}$ per cent. were therefore to be expected occasionally from ordinary errors of observation. Allowing for this it will be seen that there

* Non-magnetic or nearly non-magnetic alloys are not included.

† Difference reckoned positive when high field exceeds low.

is distinct evidence in a few cases that saturation is not quite reached in the lower field. It is improbable, however, that the difference in any case exceeds 3 per cent. or 50 absolute units.

About 40 of the pieces were then tested by one of us with conical pole-pieces and a current of 30 amperes. With this current complete reversal is possible and the measurements are accordingly rather more accurate than those made by Mr Hirst with 60 amperes. The results in nearly all cases agreed closely with those obtained by Mr Hirst; where there was any difference it was usually in the direction of a close approximation to the value found by him in the lower field.

The number of pieces examined by Mr Hirst was about 100 and covered the whole range of alloys. A considerable number of other pieces were tested subsequently by the other observers. These were mainly iron-carbon and iron-silicon steels which had been subjected to different heat treatments. Finally a large number of pieces were tested differentially in the small magnet, the more magnetic specimens being tested against the pure iron (S.C.I.)*. These tests were made in a field of about 13,000 C.G.S. units. Out of over one hundred such tests only four differed by more than two (in the magnetism relative to pure iron taken as 100) from the corresponding results obtained with the large magnet (low field). For the nearly non-magnetic pieces and for those which have nearly the same magnetism as pure iron, these differential results are probably more accurate than the others. A number of magnetisation curves, showing the value of I for different values of H down to about 1000 were also obtained with this instrument. With regard to these curves it is to be observed that the joints at the ends of the test-piece introduce an error, which may be considerable when H is less than 2000, the tendency being to make the value of I too low, so that the piece when compared with a non-magnetic material appears to approach the state of saturation more slowly than it does in fact. The amount of this effect in the case of S.C.I. is shown in Fig. 4. So far as possible the piece was of the same length as that with which it was compared, so that there would be no air-gap in either case; but no compression was used. For this reason the curves are not very accurate for lower values of H than 2000 except in the case of nearly non-magnetic materials, for which the error is inappreciable.

In reducing the results for tabulation the most convenient course is to express the magnetism of each steel in terms of that of an equal weight of pure iron. The difference fling D (corresponding to $4\pi IS$) is determined for the piece, and is compared with the corresponding fling D_0 taken under identical circumstances for a certain standard piece of pure iron. The piece is weighed and its length measured. If W_0 be the weight of the standard

* Another piece was used for these tests. The density was slightly greater (7·84) than that of the first piece, but the magnetism (when referred to unit of weight so as to correct for the difference of density) was the same within ½ per cent.

and L_0 its length, W and L the corresponding quantities for the test-piece, the relative magnetism of the latter is taken to be $\frac{D}{D_0} \cdot \frac{W_0}{W} \cdot \frac{L}{L_0}$. The lengths of the different pieces are all nearly the same, never differing by more than about 2 per cent. from the mean value. For pieces of equal density $\frac{W}{L}$ is a measure of the mean sectional area; where the densities are different it is a measure of mean area multiplied by density. I, the saturation intensity, is generally referred to unit of volume; hence, $\frac{DL}{W}$ is proportional to $\frac{I}{\text{density}}$. It appears to us more logical to express the magnetisation constant in terms of unit mass rather than in terms of unit volume, and it has the practical advantage that the effect of small gas cavities, and to a great extent that of traces of uncombined non-magnetic material such as slag, is automatically discounted. The same procedure was followed by Du Bois in expressing the results obtained by his optical method.

The whole of the results expressed in this way are collected together for convenience in Appendix IV. The analysis and density of each specimen are also shown. The densities were calculated from the weights and dimensions and are expressed in terms of the density of a piece of S.C.I. whose absolute density was 7·84*. The saturation value of I which is given is in each case the most probable value, having regard to all the observations, and is probably in nearly all cases correct within 0·5. Where the high field exceeded the low by more than 2 a note is made of the fact, and the high field result is taken as the true saturation value.

In the same table the results obtained by Barrett, Brown, and Hadfield are also shown. These are put into the form of a statement of the ratio of the permeability of the material to that of pure iron in a field of 45 c.g.s. units, and a statement of the coercive force.

General Conclusions.

Attention may be drawn to the following points of a general character:

1. There is no alloy having a higher magnetism than has pure iron.

2. Among the alloys there are one or two whose magnetism is greater than the sum of that of their constituents taken separately. Note particularly No. 59, containing 11·4 per cent. Ni.

3. The magnetic properties of every alloy, even those which are practically non-magnetic, are very closely expressed by the relation $B = H + 4\pi I$, where I is the constant which we have called the "magnetism," provided that H exceeds 7000 c.g.s. The coefficient of H in this equation certainly does not in any case differ from unity by more than

* The smallness of the diameters precludes great accuracy in the measurement of density the figures given are probably correct within about 1 part in 200.

3 per cent. It has sometimes been said that the non-magnetic manganese steels, or some of them, behave as though they had a constant permeability of the order of 1·2 or 1·4. Our experiments are not in accord with this. A steel having a permeability of 1·2 would give the following results:

$$\text{at } H = 7000, \quad 4\pi I = 1400, \quad I = 112;$$
$$\text{at } H = 2500; \quad 4\pi I = 5000, \quad I = 400;$$

showing a difference of 288 between the values of I in the high and low fields. According to our experiments on the non-magnetic steels, the difference in the values of I does not in any case exceed 30 units.

It appears that the less-magnetic alloys behave as though they consisted of some substance having a permeability which does not differ from unity by more than 2 or 3 per cent. mixed with varying quantities of magnetic matter which is practically saturated with magnetism in a force of 8000 C.G.S. Such a mixture would behave in fields of low density as though it had a constant permeability which might greatly exceed unity. But in fields of high density it would become saturated with magnetism, as the non-magnetic steels are found to do in fact. The curves of magnetisation of steels which are but slightly magnetic confirm this supposition as to the constitution of these bodies. In every instance examined when the magnetism is perceptible at all, it increases much less rapidly than H over the range 2000 to 12,000. This will be apparent from inspection of the curves Figs. 11 to 16. These curves were all obtained by the differential method and may be relied on as accurate to within one unit (S.C.I. being 100) and within one-half a unit for the nearly non-magnetic materials. For instance, in No. 98 (Fig. 15), whose magnetism relative to iron is about 3 per cent., the value of I in a field of 12,000 is only twice as great as in a field of 2000.

4. Comparison with the result obtained by Barrett in fields of 40 units or less shows that in some cases the magnetism of an alloy may be considerably greater relative to that of iron in low than in high fields. Barrett found that several of the silicon steels and one of the aluminium steels were actually more magnetic than pure iron for small values of H; but all these materials are less magnetic than pure iron when saturated. In these cases the coercive force is small. On the other hand, in some alloys with a very high coercive force (*e.g.* some of the manganese steels) the magnetism relative to pure iron is much greater when saturated than in a field of 40, in some cases more than twice as great.

The Iron-carbon Steels.

Fig. 6 shows the amount of the reduction in the magnetism of iron produced by the addition of varying amounts of carbon. The alloys shown in this diagram are those containing less than $\frac{1}{2}$ per cent. of substances

other than iron and carbon, but the impurities will to some extent mask the effect of the carbon. It is, however, clear that the points are grouped about a straight line. The effect of adding carbon within the limits here shown appears to be to reduce the magnetism by an amount proportional to the amount of carbon, the reduction of magnetism being about six times

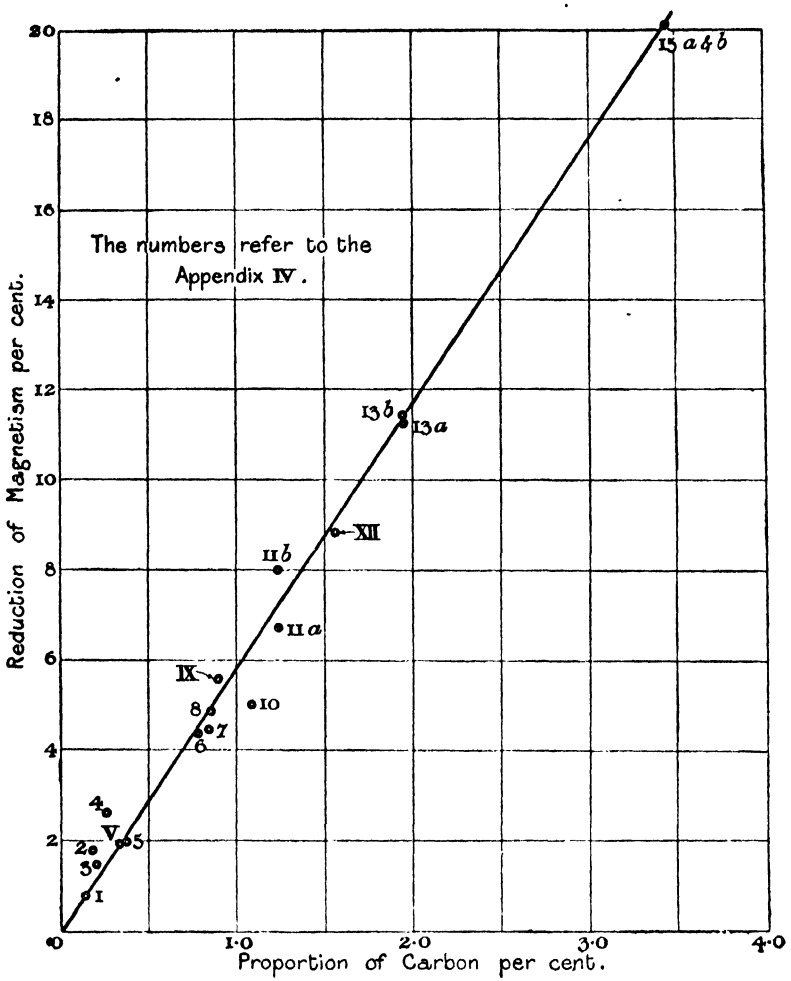


Fig. 6.

the amount of carbon. The relation is sufficiently exact to suggest that measurements of magnetism might be used as a means of determining the amount of combined carbon in a steel.

If these steels, all of which were cooled rather slowly, be regarded as mixtures of the carbide of iron Fe_3C and of pure iron, the proportion of carbide of iron will be 15·5 times the proportion of carbon. Let c be the proportion of carbon in a steel, and let I_0 be the magnetism of pure iron,

I_c that of Fe_3C. Then equation (1) connecting the magnetism (I) of the steel with that of its constituents takes the form:

$$I = 15{\cdot}5cI_c + (1 - 15{\cdot}5c)\, I_0;$$

and by experiment

$$I = I_0\,(1 - 6c);$$

whence it follows that

$$I_c = \frac{9{\cdot}5}{15{\cdot}5} I_0 = 0{\cdot}62\, I_0;$$

or iron-carbide Fe_3C is about two-thirds as magnetic as pure iron.

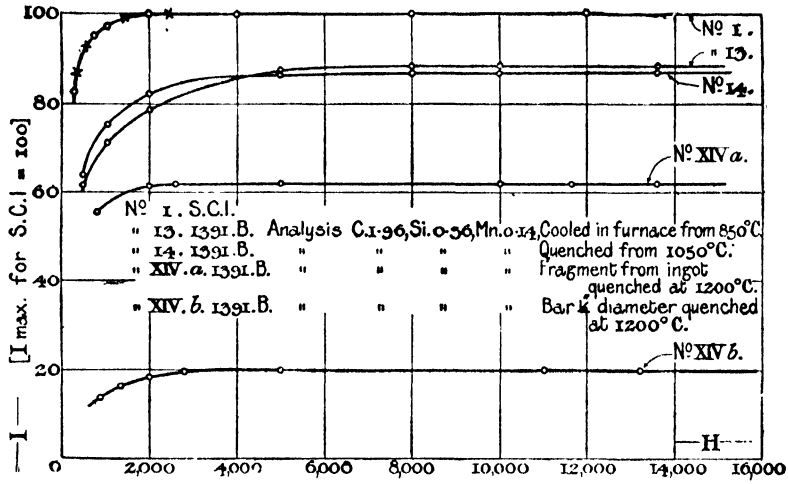


Fig. 7.

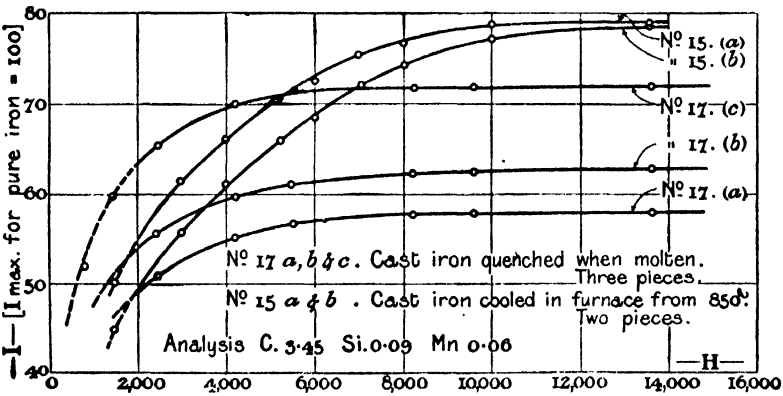


Fig. 8.

The steels containing 2 per cent. of carbon or less are practically saturated at $H = 7000$, though No. 13 (1391 B, $c = 1{\cdot}96$) approaches this state slower than does pure iron. The annealed cast iron No. 15, with 3·45 per cent. carbon, is distinctly short of saturation when $H = 7000$, the magnetism being then 75·8, whereas the saturation value of I is 79·7. It is remarkable that if the alloy be quenched in water from the molten state (No. 17) the re-

sulting product approaches the state of saturation more rapidly and is saturated at $H = 7000$, though the value of I obtained is not so high. The same is true of Nos. 13 and 14 (1391 *B*).

These peculiarities are exhibited in the magnetisation curves Figs. 7 and 8.

Effect of Rapid Cooling.

Most of the specimens were allowed to cool slowly in air after rolling or forging. In a few cases special specimens were prepared which had been carefully annealed and allowed to cool very slowly in the furnace from about 850° C. In the carbon and silicon steels little, if any, difference in the magnetism could be detected as the result of this treatment. It may therefore be assumed that the cooling of the original rods was in such cases sufficiently slow to insure the attainment of an equilibrium state.

The effect of quenching was also tried on several pieces. The results appear in the following table:

No.	Mark	Percentage of C.	Treatment	I per cent. of pure iron
8 *a*	1392 *A*	0·85	As forged	94·8
8 *b*	,,	,,	Annealed at 850° C.	95·0
9 *a*	,,	,,	Quenched in water from 775° C.	89·4
9 *b*	,,	,,	Quenched in water from 775° C.	88·8
9 *c*	,,	,,	Quenched in water from 1050° C.	88·9
11 *a*	1392 *G*	1·23	As forged	93·3
11 *b*	,,	,,	Annealed at 850° C.	91·9
12	,,	,,	Quenched from 1050° C.	82·2
13	1391 *B*	1·96	Cooled in furnace from 850° C.	88·6 (mean of 2)
14	,,	,,	Quenched from 1050° C.	87·8 (mean of 3)
XIV *a*	,,	,,	Quenched from 1200° C.	62·0
XIV *b*	,,	,,	Quenched from 1200° C.	20·0
15	White Swedish iron	3·45	Cooled in furnace from 850° C.	79·7
16 *a*	,,	,,	Quenched just after solidifying	76·5
16 *b*	,,	,,	Quenched just after solidifying (second piece)	75·7
17 *a*	,,	,,	Quenched molten	58·5
17 *b*	,,	,,	Quenched molten (second piece)	64·0
17 *c*	,,	,,	Quenched molten (third piece)	72·0

Quenching is, of course, a process of uncertain character, the outer portions of a piece cooling much more rapidly than the inner. This is exemplified in the large differences between the three samples of the cast iron which were quenched when molten. These samples were cut from

different parts of the same lump. There are corresponding differences in the micro-structure*.

Similar results were obtained with 1391 *B* (Nos. XIV *a* and XIV *b*). Specimens quenched from 1050° C. had practically as much magnetism as the annealed piece (88 per cent. of pure iron). In XIV *a*, which was cut from an irregular lump about $\frac{3}{4}$ in. diameter quenched from 1200° C., the magnetism was reduced to 60 per cent., while a bar of $\frac{1}{4}$ in. diameter quenched from 1200° C. (XIV *b*) had only 20 per cent. of the magnetism of pure iron. Photomicrographs of these three pieces are given for comparison (Fig. 17)*. The full discussion of these results hardly falls within the scope of this paper, but it may be noted that in this steel, according to the usual theory based on the cooling curves, the formation of cementite would occur in the neighbourhood of 1100° C. It is possible that the effect of quenching may depend on whether or no free cementite is present, and this might account for the marked difference between the pieces quenched at 1200° C. and at 1050° C. respectively. The difference between the two pieces quenched at 1200° C. is probably due to difference in the speed of cooling. The second piece (XIV *b*) was the smaller and would cool more rapidly.

Silicon-iron and Aluminium-iron Alloys.

Silicon seems (at high inductions) to act mainly as an inactive diluent, the reduction in magnetism being but little greater than the percentage of this element which is present, after allowance is made for the effect of the carbon. The following figures illustrate this point:

No.	Si	C	Reduction of magnetism	Reduction due to carbon alone
			per cent.	per cent.
26 (803) ...	2·28	0·67	3·6	4·0
29 (898 *L*) ...	2·77	0·34	3·2	2·0
28 (898 *E*) ...	2·67	0·20	3·9	1·2
32 (898 *M*) ...	3·03	0·07	4·1	0·4
†34 *a* (1,894 *A*) ...	3·26	0·15	5·0	0·9
†34 *b* (do. annealed) ...	3·26	0·15	4·1	0·9
31 (898 *K*) ...	3·89	0·11	6·2	0·7
33 *a* (898 *H*) ...	5·53	0·26	7·4	1·5

In the first two instances in the above table, it would appear that the addition of the silicon had neutralized the effect of the carbon to some extent, the combined effect of the two elements being only slightly more than if both were inert diluents.

Some of these silicon-iron alloys are very soft (magnetically) in fields of low density. Thus No. 28, containing 2·67 Si and 0·20 C., has 98 per cent.

* The Photomicrographs, Figs. 9 and 17, are omitted in this reprint.

† Contains 0·96 per cent. Al.

of the magnetism of pure iron when $H = 45$ (Barrett), the proportion diminishing to 96 per cent. when saturation is reached. The curve of magnetisation determined by Barrett from this material (Fig. 10) is of such a form relative to that of pure iron as to suggest that the relative magnetism will diminish with increasing force. The coercive force is only about half of that of pure iron. These facts suggest that the silicon has the effect of reducing the interaction of the molecules of iron—perhaps by mechanical

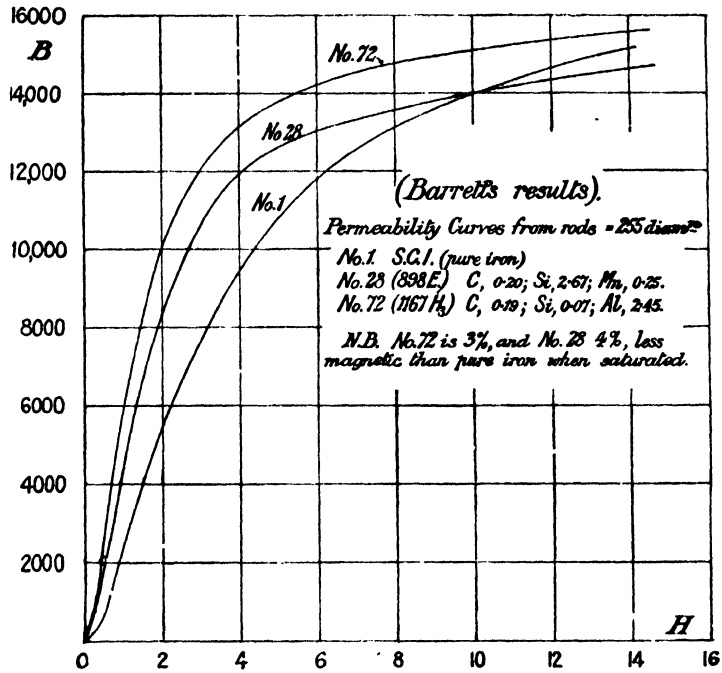


Fig. 10.

separation—without otherwise affecting them. Quenching reduces the magnetism of the silicon-iron alloys, but this may be due to the presence of carbon. No. 33, containing 5·5 Si and 0·26 C, is hardly altered by quenching, whereas No. 29, containing 2·77 Si and 0·34 C, loses about 3·5 per cent. of its magnetism when quenched.

The effect of aluminium is similar to that of silicon but more marked. Thus No. 72, containing 0·19 per cent. of carbon and 2·45 per cent. of aluminium, has, when saturated, 3 per cent. less magnetism than iron. According to Barrett, this alloy is more magnetic than iron in a field of 45 and has a coercive force of 1·0. The permeability curve (see Fig. 10) evidently crosses that of pure iron at some value H greater than 45.

Iron-manganese Alloys.

We have been unable to discover any simple relation between the proportion of manganese and the corresponding reduction in magnetism. It is well known that the influence of the manganese is greatly affected by comparatively small amounts of carbon. The results which we have found, however, present anomalies which can hardly be explained by the varying proportions of this element. They suggest that there is some other variable, possibly the temperatures to which the alloys happen to have been subjected in rolling or forging, and the precise mechanical character of that process. Thus we have:

No.	C	Mn	Si	Reduction in magnetism per cent.
37 (1379 B_2)	0·08	3·50	0·130	9·1
38 (1323 C)	0·15	5·40	0·037	6·4
39 (1379 D)	0·16	10·08	0·630	55·5
40 (1379 D_2)	0·15	15·27	—	95·5

In No. 38 the manganese, 5·40 per cent., behaves like an inactive diluent, whereas in 37, containing less carbon, the percentage reduction of magnetism is nearly three times the percentage of manganese. On the other hand, the addition of another 5 per cent. of manganese to No. 38 reduces the magnetism by 50 per cent. Larger proportions of carbon, however, appear to have an opposite effect. Thus No. 42 is only about half as magnetic as pure iron, yet it contains but 3·81 per cent. of manganese. The presence of 0·78 of carbon may be the cause of this. Again, No. 43, though containing more manganese than No. 42, has 91 per cent. of the magnetism of pure iron. In this case the carbon is 0·36. The same thing appears in a comparison of Nos. 47 and 48:

No.	C	Mn	Specific magnetism
47 (1010 *W.T.*)	1·23	12·64	0
48 (1338 B_2)	0·26	13·00	15

the higher carbon being associated with the lower magnetism.

A good deal of attention was devoted to No. 47 (1010 *W.T.*). This is the original Hadfield's non-magnetic steel, containing about 12 per cent. Mn and 1 per cent. C, and its magnetic properties have been examined by a number of observers, including J. Hopkinson, Ewing, Barrett, and Du Bois. Hopkinson, working in fields of moderate intensity (up to about 200 C.G.S.), found it to possess a constant permeability of about 1·3. Ewing

tested it by the isthmus method, and found the permeability to be roughly constant and equal to 1·4 from $H = 1900$ to $H = 10{,}000$, the value of I in the latter field amounting to about 25 per cent. of the saturation value of pure iron. As Ewing remarks, this is a "respectably high" intensity of magnetisation. Du Bois, using the rotation of the plane of the polarisation of light reflected from the metal as a measure of the intensity of magnetisation, obtained no definite results, the rotation varying with the precise point of the metal surface from which reflection took place. This Du Bois ascribed to heterogeneity of structure.

We have tested in all five pieces of this material, all cut from the same bar, and in none was the magnetism so much as 1/100 part of that of pure iron. The permeability for $H = 10{,}000$ did not exceed 1·02. One of the pieces of this manganese steel (No. 47) was kept for 70 hours at a temperature of 800° C. and then slowly cooled, without, however, making it magnetic. The piece when taken out of the furnace was attracted by a magnet, but after the outer layer (which was probably decarbonized) had been ground off the attraction practically ceased, and the piece then had no magnetic quality perceptible by our method of testing. It is, however, certain that by some sort of heat treatment this steel can be rendered fairly magnetic. A bar is in existence, one end of which has over 27 per cent. of the magnetism of pure iron, while the other end (which had been forged down) is absolutely non-magnetic. The exact treatment to which this bar was subjected is not known, but its composition (C 1·76, Mn 11·6) is nearly that of No. 47 (1010 *W.T.*). It will be observed that No. 45 having 1·66 per cent. of C and 11·53 of Mn, had 42 per cent. magnetism, as originally forged. By heating to redness and cooling in air (No. 46), however, the magnetism was completely destroyed. The precise character of the treatment by which it can be restored is now undergoing investigation. It should be observed that both No. 47 in its magnetic state and No. 45 (see Fig. 11) showed steady approach to saturation which was practically reached in a field of about 10,000 C.G.S. like the other materials examined by us. There was nothing approaching the constant permeability found in Ewing's experiments*. The magnetisation curves of several different iron-manganese alloys, obtained differentially, are shown in Fig. 11. Our observation that the non-magnetic manganese steel has no perceptible magnetism in fields of high density is, however, consistent with the existence of a nearly constant permeability of 1·3 or 1·4 in fields up to 200 C.G.S., as found by J. Hopkinson. A little pure iron, amounting to, say, 1 per

* It may be observed that Ewing's method, though well adapted for examining materials which are nearly as magnetic as iron, is not so suitable for testing non-magnetic alloys. The measurement of I depends on the assumption that the value of H in the neck of the test-piece is equal to that in the outer coil. This assumption of uniformity of field is undoubtedly nearly correct with an iron test-piece. But it is probable that when the test-piece is non-magnetic the force is considerably greater on the axis, than at points at a distance, because of the distribution of magnetism on the end of the conical pole-pieces. This would make the piece, if in the non-magnetic state, appear to have a nearly constant permeability greater than unity.

cent. of the whole, if distributed through the alloy in continuous threads, would account for its behaviour both in weak and in strong fields.

The peculiar effects of heat treatment on iron-manganese alloys which appear in our experiments on Nos. 45 and 46 are illustrated by the following results given by J. Hopkinson:

Steel containing 1·3 *per cent.* C *and* 8·7 *per cent.* Mn.

As forged	...	B = 747	in field of 240 units.
Annealed	...	B = 1985	„ 240 „
Oil-hardened	...	B = 733	„ 240 „

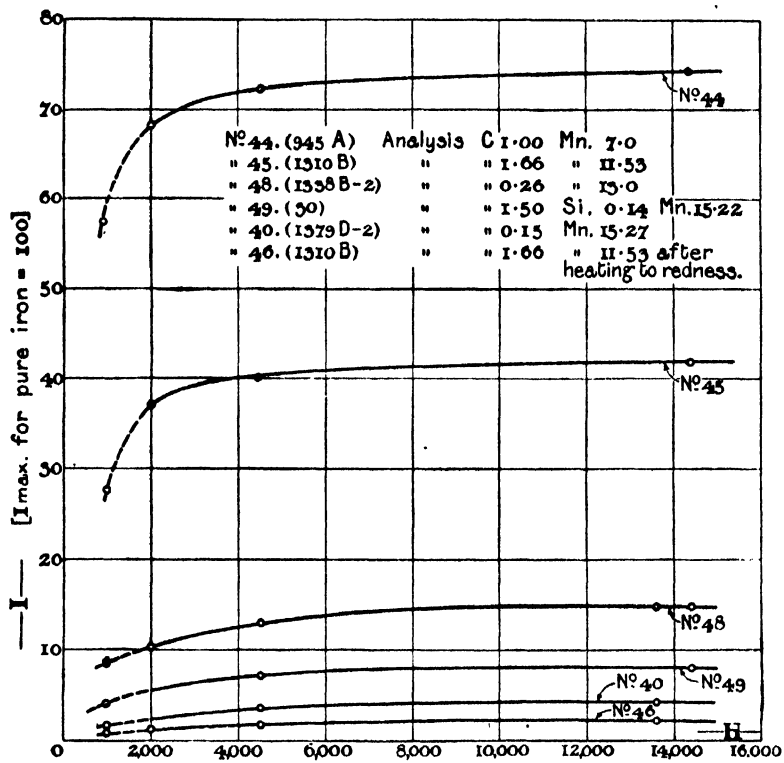


Fig. 11.

It will be noticed that the cooling in air which presumably followed forging was in this case sufficiently rapid to produce all the effects of quenching in oil. It seems possible that most of the anomalous results obtained from these alloys in this research may be explained by the temperatures to which they have been subjected in rolling or forging. The cooling in air to which all were subjected, and which would be without much effect in an iron-carbon alloy, may have been quick enough in some of these iron-manganese alloys to give the effects of quenching.

In the more magnetic iron-manganese alloys the approach to the state of saturation is very slow in the earlier stages. This is apparent from the magnetisation curves (Fig. 11), and may also be seen by comparing the limiting value of I found in our experiments with Barrett's results for $H = 45$. Thus No. 43 (4·68 per cent. of manganese, 0·36 per cent. of carbon), is 91 per cent. as magnetic as pure iron in very intense fields, but in a field of 45 units, I for this material is only 49·5 per cent. of its value in pure iron in the same field. Barrett found the coercive force of this alloy to be 19·6 or 10 times that of pure iron, which would lead one to expect excessive magnetic hardness under small forces, though it is consistent with a large saturation intensity. No. 44 has similar properties. In this respect the effect of manganese is opposite to that of silicon. On the other hand, in No. 38 the intensity of magnetisation in a field of 45 units, is 87½ per cent. relative to pure iron, rising to 93½ per cent. under large forces. In this case, however, the coercive force is much less—only 6·0.

One result of the complex effects of temperature or mechanical work on the iron-manganese alloys is that different parts of the same bar frequently exhibit different magnetic qualities. Any statement of the magnetism of these substances ought evidently to be accompanied by a precise specification of the heat treatment. Probably the exact temperature at which rolling or forging took place is an important factor. Pending further investigation, therefore, in which these details will be noted, the figures given in Appendix IV for these bodies must be regarded as provisional only.

Iron-nickel Alloys.

The magnetism of the iron-nickel alloys, like that of the iron-manganese alloys, cannot be expressed in simple terms, and is now undergoing further investigation, in which attention is being paid to various details of heat treatment, etc. Meanwhile, we may draw attention to one or two points which appear in the results already obtained. Comparing the results obtained by us for specimens Nos. 57 to 64 with those found by Barrett for the same materials in fields of low density, it will be seen that in four cases (59 to 62) there is a large increase in relative magnetism when the force is increased from 45 to 5000. Each of these steels has a high coercive force. The most striking instance is, perhaps, No. 59, containing 11·4 per cent. of nickel, whose magnetism when saturated is 96 per cent. of that of pure iron in spite of the large nickel content, though in a field of 45 it is less than half as magnetic as pure iron. This steel is remarkable in that its magnetism is greater than that of the elements taken separately*. With the possible exceptions of Nos. 57 and 58 no other alloy in our series possesses this peculiarity. According to Barrett, this material is

* There is 87 per cent. of iron and 11·4 per cent. of nickel. The nickel is magnetically equivalent to about 3·5 per cent. of iron. Total, 90·5 per cent.

only half as magnetic as pure iron in a field of 45 units. It will be seen from this that experiments on fields of the order of 100 units may completely fail to reveal the ultimate magnetic constitution of a steel with high coercive force.

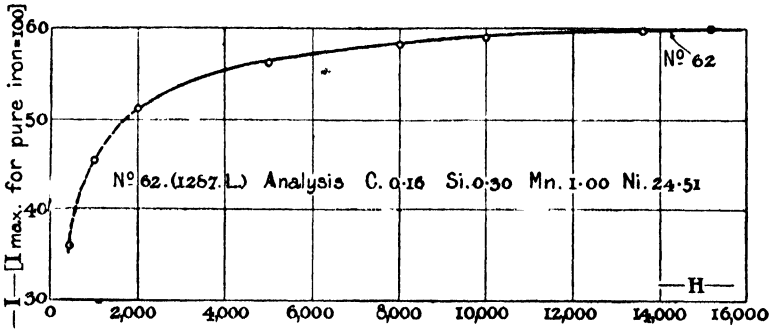
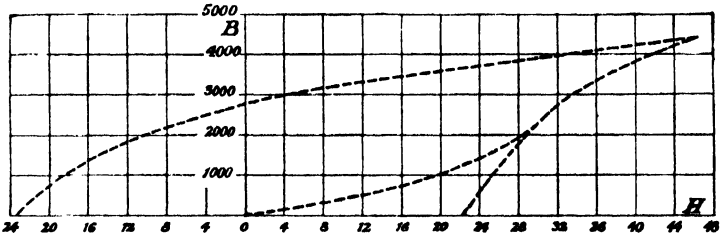


Fig. 12.



(Barrett)

No. 62. (1287 L) C, 0·16; Ni, 24·5; Mn, 1·00,

Fig. 13.

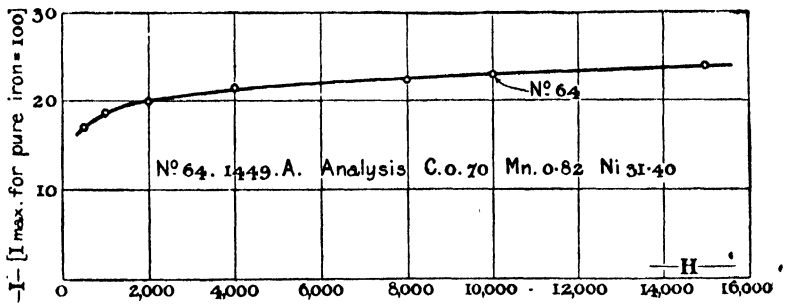


Fig. 14.

On the other hand, the more magnetic alloys of the group in which the coercive force is low—viz., Nos. 57 and 58—are, relatively to pure iron, practically as magnetic when H = 45 as they are when H = 4000. The relative permeability of No. 64 (Ni = 31·5 per cent.), which also has a low coercive force, seems to be considerably less in the low field, but there may have been some difference of heat treatment which would account for this.

Fig. 12 is the magnetisation curve of No. 62 (1287 *L*), and Fig. 13 is the curve given by Barrett for the same alloy. Fig. 14 is No. 64 (1449 *A*). Figs. 15 and 16 show the manner in which nearly non-magnetic alloys approach the state of saturation.

Some of the iron-nickel alloys exhibit the same sort of indefiniteness as regards magnetism as do the alloys of iron and manganese, different parts of the same bar having different magnetic qualities. It is hoped that by further investigation in which careful observation is kept of the heat treatment this indefiniteness may be removed.

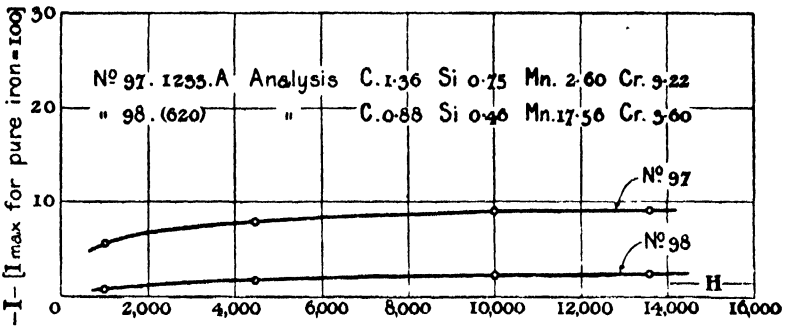


Fig. 15

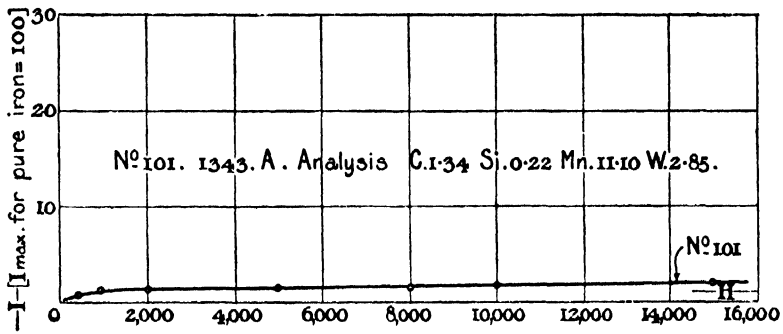


Fig. 16.

We have to acknowledge with thanks the great assistance which has been rendered in this research by our assistants and by advanced students in the Engineering Laboratory, Cambridge, including particularly Mr J. Hirst and Mr H. Quinney, and Mr Milne and Mr Main, of Hecla Works. The greater part of the laborious work of testing and of reducing the results has been carried out by these gentlemen.

APPENDIX I

The Ballistic Galvanometer.

The galvanometer used in most of the experiments was of the moving-coil type. This form—though less sensitive than the suspended magnet type—was chosen in order to avoid stray field effects, which are rather large in the neighbourhood of so big a magnet. The galvanometer was made by Nalder Bros., some years ago, and is provided with permanent magnets. It was found necessary to increase the natural period of oscillation considerably, on account of the slowness of the change of flux following a reversal of the magnet current, and, for this purpose, the moving system was loaded with small pieces of lead carried on a mica arm. The period of oscillation was about 15½ seconds, and the resistance of the coil was about 3·3 ohms. The sensitiveness of the galvanometer was such that one division ($\frac{1}{40}$ in.) on a scale distant about 90 cm. from the instrument, corresponded to about $3{\cdot}7 \times 10^{-7}$ coulombs.

In order to obtain flings sufficiently large for accurate reading with the small flux changes available, it was necessary to keep the resistance of the galvanometer circuit low. In most of the tests the external resistance (viz. resistance additional to the galvanometer and testing coil) was 4 ohms, and the total circuit resistance was 8·24 ohms. With this amount of added resistance the ordinary changes of room temperature did not alter the total resistance by more than about ½ per cent. above or below the mean value, and at the same time the sensibility was such that the value of $4\pi IS$ for a piece of Low Moor iron ⅛ in. diameter corresponded to a fling of about 130 divisions. The damping with so low a resistance was, however, very large, the ratio of successive swings on the same side being 0·331. In the theory of the ballistic galvanometer it can usually be assumed that the damping is so small that its effect on the period of oscillation may be neglected, and in that case the expression for the flux-change F in terms of the fling is $F = kL\phi R \times 10^8$, where k is the equivalent in coulombs of 1 division of undamped fling, R the total resistance of the circuit, and L the fourth root of the ratio of two successive swings on the same side when the galvanometer is freely swinging with its circuit closed. In the present case the damping is so large that the more accurate formula

$$F = kL'\phi R \times 10^8$$

must be used. k and R have the same meanings as before, and

$$L' = Le^{-\epsilon \tan \epsilon},$$

where L is, as before, the fourth root of the ratio of two successive swings on the same side, and

$$\tan \epsilon = \frac{2}{\pi} \log_e L.$$

The following table gives the constant $kL'R \times 10^8$ (which gives flux-change in the terms of fling) for different values of R, together with the values of L and ϵ on which it is based. It is assumed that $k = 3{\cdot}72 \times 10^{-7}$ coulombs per division, which was the value of this constant for many of the tests.

Additional resistance (ohms)	R	L	ϵ	L'	$RL'k$
0	4·19	1·757	0·344	1·553	242
4	8·24	1·320	0·176	1·284	394
10	14·22	1·178	0·105	1·168	618
20	24·20	1·108	0·065	1·103	994

The flings produced by the same flux change with different external resistances were carefully compared on a number of occasions, and it was found that their relative amounts were in inverse proportion to RL', certainly within 1 part in 200.

Three methods were used for the determination of the constant k: (1) The discharge of a condenser. A standard condenser with a capacity of 1·015 microfarad was available for this*. (2) By the use of a standard inductance. Two standard fields were used, each consisting of a long solenoid wound on a brass cylinder with a secondary coil of a few turns at the middle of its length. The flux embraced by the secondary coil per ampere of current in the primary was obtained by calculation from the dimensions. (3) By measuring the current α, corresponding to one division of steady deflection on the galvanometer, and calculating the constant from the formula $k = \frac{\tau\alpha}{2\pi}$, where τ is the period of oscillation of that circuit. On one occasion, when all three methods were used, the following values were obtained for k:

1. Condenser:

$3{\cdot}67 \times 10^{-7}$ coulombs per division.

2. Standard fields:

(a) $3{\cdot}735 \times 10^{-7}$ coulombs per division.
(b) $3{\cdot}745 \times 10^{-7}$ coulombs per division.

3. Steady deflection:

$3{\cdot}76 \times 10^{-7}$ coulombs per division.

* "The instrument was marked '1 microfarad,' and the maker's test gave it as 1·0015 microfarad. I am indebted to Mr Albert Campbell for suggesting that B.A. microfarads were intended and not true microfarads, and thereby clearing up an apparent discrepancy in calibration which had caused much trouble. As the condenser was bought as late as 1906, such a possibility never occurred to me."—B.H.

The period of the galvanometer on this occasion was 15·40 seconds. A day or two later the condenser and steady deflection calibrations were again compared, with the following result:

Condenser	...	$k = 3{\cdot}715 \times 10^{-7}$;
Steady deflection	...	$k = 3{\cdot}715 \times 10^{-7}$.

The period on this day was 15·27 seconds.

The steady deflection method is subject to a little uncertainty on account of the slow change of zero which goes on when the suspension is held twisted*. The most accurate value of k is probably that given by the standard fields, and it has been assumed for the purpose of these experiments that that given by the condenser is for one reason or another ½ per cent. too low. This corresponds to taking $k = 3{\cdot}74 \times 10^{-7}$ coulombs per division in the above group of three tests. It is practically certain that this is within 1 per cent. of the truth, and probable that it is within ½ per cent. The galvanometer constant varied slightly with time, and whenever absolute measurements were desired it was re-determined. The most convenient way of doing this happened to be by using the condenser, and in all cases ½ per cent. was added to the constant so determined in order to get the correct value. The condenser was always charged to the same potential—about 100 volts—and the potential was measured with the same instrument.

For some tests a galvanometer of the suspended magnet type was used. This was of Broca's form, and was made by the Cambridge Scientific Instrument Company. It had a quartz fibre suspension, and a period of about 30 seconds. The resistance of the coils was about 11·5 ohms. Its sensitiveness was such that it could only be used in a room at some distance from the large electromagnet, and arrangements had to be made for signalling to the operator who reversed the current. On this account the measurements consumed much time, and were only made for the purpose of checking the readings obtained with the other galvanometer. The Broca galvanometer was calibrated by the use of the standard condenser and by the steady deflection method. The quartz fibre suspension made the latter quite reliable in this case. The two methods gave values of k differing by ⅔ per cent., the condenser being the higher.

The ultimate electrical standards used were some Clark and Weston cells which were in close agreement, and a resistance box—by the Cambridge Scientific Instrument Company, which was checked at the National Physical Laboratory. A Siemens millivoltmeter with shunt and series resistances was used for most of the actual measurements of current and potential; its readings were checked and corrected by comparison with the ultimate standards.

* The effect of this was eliminated to a great extent by observing the total fling (sum of right and left) which followed breaking the current in the galvanometer when the latter was deflected. The steady deflection was taken to be half this total fling when corrected for damping.

APPENDIX II.

Slow Growth of Flux in the Large Magnet.

Since the magnet limbs and pole-pieces are solid and of considerable size, the reversal of the current in the coils is followed by the generation of large eddy currents which take some time to die out and delay the change of flux. In order to get a rough estimate of the magnitude of this effect the experiment was tried of reversing the magnet current with the galvanometer circuit open and closing the circuit, by depressing a key, 2 seconds after reversal. The fling of the ballistic galvanometer then corresponds to that fraction of the total flux change which remains to be completed after 2 seconds. The time was roughly estimated by watching a seconds pendulum. It was found that using a current of 5 amperes, obtained from a source of 50 volts, with the flat pole-pieces $\frac{1}{4}$ in. apart, the fling obtained after 2 seconds was about 5 per cent. of the total observed when the galvanometer circuit was kept closed during the whole period of flux change. Under the same circumstances, but using a source of 100 volts with external resistances, the percentage left after 2 seconds was rather smaller, a result due to the diminished effects of self-induction. These experiments were made with the usual test coil, and with a brass dummy in place of the test-piece.

In order that the ballistic galvanometer may correctly record the flux in a coil connected to it, that change must be completed within a period which is small compared with the period of oscillation of the galvanometer. The latter period in the experiments here described was about 15 seconds, which is longer than usual, but not sufficiently long as compared with the period of the flux-change to preclude the necessity of a careful investigation of the effect of the time of growth of the flux.

A determination of the actual flux in the air-gap was therefore made by withdrawing a coil from the field. The coil used for this purpose was wound in one layer on a circular former about $\frac{1}{2}$ in. diameter. A large coil was used in order to reduce the effect of the field embraced by the leads. The current (4·9 amperes) was put on with the coil in place between the flat pole-pieces (which were $\frac{1}{4}$ in. apart), and the coil was then rapidly taken out by hand, removed to a distance of some inches, and placed with its plane parallel to the axis of the magnet poles. The whole operation occupied less than 1 second. The flings observed were 125 divisions with the current in one direction and 127 divisions with the current in the other direction. The flux change produced by reversal, therefore, corresponds to a fling of 252 divisions. The fling actually obtained by reversal of the same current with the coil fixed in place was 235 divisions. Under these circumstances, therefore, the galvanometer underestimates the flux change in the air-gap by 17 divisions, or about 7 per cent. In this test the source

of current was a battery of 50 volts E.M.F.; by using 100 volts the error was reduced to about 5 per cent.

With the conical pole-pieces and a large magnet current giving a field of over 20,000 lines, the difference between the values of flux density obtained by withdrawing a coil from the field and by reversing the current with the coil in place, was less than 2 per cent. The smaller difference in this case is doubtless due to the fact that the tips of these pole-pieces very soon become saturated, so that the greater part of the magnetic force in the air-gap develops in a very short time, and only a comparatively small proportion is added after the galvanometer coil has begun to move.

The experiments hitherto described in this section refer to the measurement of H or the field in the air-space when no test-piece is there. They show that the errors in the determination of this quantity caused by the slowness of flux change are by no means inappreciable. It happens, however, that these errors do not appreciably affect the determination of I. The reason of this is that the magnetisation of the iron or steel test-piece is almost completely reversed in a very short time. Within less than a second after the reversal of the current the magnetising force has attained a value sufficient almost completely to saturate the test-piece in the reverse direction; and the change in the magnetisation of the test-piece corresponding to the latter parts of the change in H is a very small proportion of the whole. The fling produced by reversing the current with the test-piece in position is made up of two parts, the effects of which can be superposed by simple addition. The first corresponds to HS', the second to $4\pi IS$. The first part is (apart from end effects) the same both as regards total amount and as regards distribution in time as the flux change when the test-piece is replaced by a brass dummy. It is underestimated to some extent owing to the slow growth of flux, but to the same extent whether the test-piece or dummy be used. The second part, $4\pi IS$, is the additional flux due to the magnetisation of the steel-piece, and this change is completed in a time so short that the ballistic galvanometer records it correctly. In taking the difference of the flings with and without the test-piece the error in measuring HS' cuts out, and the difference (after allowing for the end correction) gives a correct measure of $4\pi IS$.

As stated in the text of the paper, the value of I was determined for a number of pieces with another galvanometer having a period of 30 seconds. It was found that the value so obtained agreed within $\frac{1}{2}$ per cent. with that obtained with the shorter-period instrument. The fact that the result is independent of the period of the galvanometer shows that the retardation of the flux is without effect on the measurement of I.

NOTE (added February, 1911). In view of the discrepancy between the value of the magnetism of pure iron obtained in this research and the values of the same constant found by some other observers, it was thought desirable to make a fresh measurement in which any possible effect of reversing the magnet

current should be avoided. For this purpose arrangements were made whereby the test-piece could be withdrawn from the test coil by sliding it in the direction of its length through a hole bored in the pole-piece. A piece of Low Moor iron was used, $1\frac{1}{2}$ in. long by $\frac{1}{8}$ in. diameter, and it was placed between the flat parallel pole-faces of the large magnet used by Mr E. F. Clark, which is referred to on page 137. It was inserted into a brass bobbin wound with a coil of about 70 turns. The coil was connected to a standard inductance and to a ballistic galvanometer. The withdrawal of the test-piece, being equivalent to the substitution of air for the iron within the test-coil, gave a fling which was a direct measure of $4\pi IS$. The exciting current of the magnet was, of course, kept constant during the operation, and the piece was withdrawn just so far that the end was flush with the face of the pole-piece through which it had been drawn.

The value of I found in this way for Low Moor iron was 1665 in a field of 3000 C.G.S. units. The same piece tested differentially by the reversal of the exciting current of the magnet gave I equal to 1656. The difference between the two methods, amounting to about $\frac{1}{2}$ per cent., undoubtedly exists, and is to be ascribed to time lag, but is of no importance for the present research. In a field of 5000 units the difference would be considerably less.

APPENDIX III.

Calculation of Effect of Ends of Specimens.

It is desired to find the average force parallel to the axis due to distribution of magnetism of uniform density $\pm I$ respectively on the circular ends of the piece.

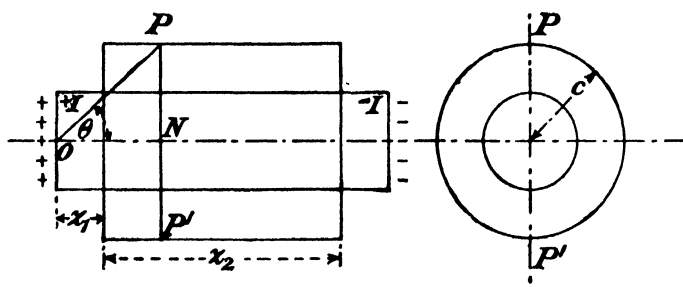


Fig. 18.

The potential of one of the circular patches of magnetism at a point P (Fig. 18) is

$$V = 2\pi\left[\frac{1}{2}\frac{a^2}{r} - \frac{1.1}{2.4}\frac{a^4}{r^3}P_2 + \frac{1.1.3}{2.4.6}\frac{a^6}{r^5}P_4 - \dots\right],$$

where a is the radius of the patch, $OP = r$, and $P_2, P_4 \dots$ are zonal harmonics. The total flux from the patch through the circle PP' (radius c) is

$$F = \int_1^z 2\pi r^2 \frac{dV}{dr}\, d\mu, \quad \text{where } \mu = \cos\theta,\ ON = z$$

$$= 4\pi^2 a^2\left[\frac{1}{2}\left(1 - \frac{z}{r}\right) + \frac{1.1}{2.4}\frac{3a^2}{r^2}\left(\frac{z^3}{2r^3} - \frac{z}{2r}\right) - \frac{1.1.3}{2.4.6}\cdot\frac{5a^4}{r^4}\left(\frac{7}{8}\frac{z^5}{r^5} - \frac{5}{4}\frac{z^3}{r^3} + \frac{3z}{8r}\right) + \dots\right].$$

The total flux through a helix of radius c, extending axially from z_1 to z_2 and having n turns per unit of length, is

$$n\int_{z_1}^{z_2} F\,.\,dz = 4\pi^2a^2n\Big[\frac{z-r}{2} + \frac{1\,.\,1\,.\,3}{2\,.\,4}\frac{a^2c^2}{6r^3} + \frac{1\,.\,1\,.\,3\,.\,5}{2\,.\,4\,.\,6}a^4\Big(\frac{c^4}{8r^7} - \frac{c^2}{10r^5}\Big) + \cdots\Big]_{z_1}^{z_2};$$

or, sufficiently nearly, the average flux per turn of the helix is

$$\frac{4\pi^2a^2}{z_2-z_1}\Big[\frac{z-r}{2} + \frac{1}{16}\frac{a^2c^2}{r^3}\Big]_{z_1}^{z_2}.$$

For coil A (inner coil), we have

$$z_1 = 0{\cdot}7a, \qquad r_1 = 1{\cdot}48a, \qquad c = 1{\cdot}3a,$$
$$z_2 = 3{\cdot}3a, \qquad r_2 = 3{\cdot}55a,$$

from which it follows that the average flux per turn is $0{\cdot}91 \times 4\pi^2a^2$. With two end patches of magnetism of density $\pm\, 0{\cdot}8I$, the average flux per turn will be $0{\cdot}182 \times 0{\cdot}8 \times 4\pi IS = 0{\cdot}146 \times 4\pi IS$, S being the area of each patch (πa^2). The difference fling D in this case is therefore reduced by 14·6 per cent. of $4\pi IS$ below what it would be if H were unaffected by the end patches, and the correcting factor is $\frac{1}{0{\cdot}854} = 1{\cdot}17$. For the outer coil of A the data are

$$z_1 = 0{\cdot}7a, \qquad r_1 = 1{\cdot}885a, \qquad c = 1{\cdot}75a,$$
$$z_2 = 3{\cdot}3a, \qquad r_2 = 3{\cdot}740a,$$

whence it follows that the average flux per turn in the outer coil given by patches of density $\pm\, 0{\cdot}8I$ is 0·214 of $4\pi IS$. In the annulus between the coils it is $0{\cdot}214 - 0{\cdot}146 = 0{\cdot}068$ of $4\pi IS$. This is the reduction in the fling due to the annulus, produced by introducing the iron piece. In the experiment described on page 128 the fling corresponding to $4\pi IS$ was 140 divisions, and $0{\cdot}068 \times 140 = 9{\cdot}5$, which is the figure given in the table as the calculated reduction.

For the other coil B the data are

Inner coil ... $c = 1{\cdot}11a$, $r_1 = 1{\cdot}31a$, $r_2 = 3{\cdot}48a$,
Outer coil ... $c = 1{\cdot}75a$, $r_1 = 1{\cdot}48a$, $r_2 = 3{\cdot}55a$.

And for both coils $z_1 = 0{\cdot}7a$, $z_2 = 3{\cdot}3a$.

The flux in the inner coil due to uniform distributions $\pm\, 0{\cdot}8I$ is 11 per cent. of $4\pi IS$, and in the annulus 10½ per cent.

APPENDIX IV

The relative density is stated in terms of a piece of S.C.I. whose absolute density calculated from the weight and dimensions was 7·84. The densities given by Brown are in absolute measure, that of S.C.I. being given as 7·877.

The relative magnetism for $H = 45$ is the ratio of the permeability of the material to that of pure iron in that field, and is calculated from the figures given by Barrett, Brown, and Hadfield.

No.	Maker's mark	Analysis					Relative density	Relative magnetism (saturation) pure iron = 100	Relative magnetism $H = 45$ (Barrett) pure iron = 100	Coercive force (Barrett)	
		C	Si	Mn	Al	Other elements					
	Iron										
I	S.C.I.	0·04	0·07	Trace	—	0·005 S, 0·004 P	1·0000	100·0	—	1·10	
II	L.S.S.	„	0·02	0·180	—	0·011 S, 0·013 P	0·9860	100·0	—	1·66	
III	B	0·03	0·14	0·036	—	0·016 S, 0·065 P	0·9960	98·6	—	1·66	
IV	Low Moor	—	—	—	—	—	0·9686	98·1	—	—	
IV *a*	Low Moor	—	—	—	—	—	0·9840	98·5	—	—	
	Iron Carbon										
1	1166 A_4	0·14	0·08	0·07	0·175	—	0·990	99·2	—	—	
2	1754	0·19	0·28	0·52	—	—	0·986	98·2	—	—	
3	48	0·20	0·02	0·50	—	—	0·988	99·5	95·0	3·2	
4	C	0·26	—	1·21	—	—	0·992	97·4	—	—	
5	1397 A	0·37	0·380	0·200	—	—	0·983	98·0	—	—	
V	... ...	0·33	0·037	0·031	—	—	—	97·9	—	—	Annealed at 750° C.
6	1392 H	0·78	0·100	0·100	0·013	—	0·980	95·6	—	—	
7	1392 I	0·83	0·060	0·250	—	—	0·991	95·5	—	—	
8 *a*	1392 A (as forged)	0·85	0·170	0·320	0·020	—	0·984	94·8	—	—	
8 *b*	1392 A (annealed at 850° C.) ...	„	„	„	„	—	0·999	95·0	—	—	
8 *c*	1392 B	0·84	0·200	0·180	0·030	—	0·978	95·1	—	—	

APPENDIX IV—(*continued*)

No.	Maker's mark	Analysis					Relative density	Relative magnetism (saturation) pure iron = 100	Relative magnetism $H = 45$ (Barrett) pure iron = 100	Coercive force (Barrett)	
		C	Si	Mn	Al	Other elements					
	IRON CARBON—*continued*										
9 *a*	1392 A (quenched at 775° C.) ...	0·85	0·170	0·320	0·020	—	0·984	89·4	—	—	
9 *b*	1392 B (quenched at 775° C.) ...	,,	,,	,,	,,	—	0·983	88·8	—	—	
9 *c*	1392 A (quenched at 1050° C.)...	,,	,,	,,	,,	—	,,	88·9	—	—	
IX	... ...	0·91	0·046	0·200	—	—	—	94·4	—	—	Annealed at 750° C.
10	1392 L	1·09	0·170	0·320	—	—	0·985	95·0	—	—	
11 *a*	1392 G (as forged)	1·23	0·120	0·140	0·025	—	,,	93·3	—	—	
11 *b*	1392 G (annealed at 850° C.) ...	,,	,,	,,	,,	—	1·000	91·9	—	—	
12	1392 G (quenched from 1050° C.)	,,	,,	,,	,,	—	0·985	82·2	—	—	
XII	... ...	1·54	0·056	0·057	—	—	—	91·2	—	—	Annealed at 750° C.
13 *a*	1391 B (cooled in furnace from 850° C.)	1·96	0·360	0·140	—	—	0·988	88·7	—	—	
13 *b*	1391 B (cool in furnace from 850° C.)	,,	,,	,,	—	—	0·990	88·4	—	—	
14	1391 B (quenched at 1050° C.)	,,	,,	,,	—	—	0·984	87·7	—	—	Mean of three pieces, 87·1 to 88·5
XIV *a*	1391 B (quenched at 1200° C.)	,,	,,	,,	—	—	0·987	62·0	—	—	Fragment from ingot.
XIV *b*	1391 B (quenched at 1200° C.)	,,	,,	,,	—	—	0·998	20·0	—	—	¼ in. bar
15 *a*	White Swedish iron (annealed)	3·45	0·09	0·060	—	—	0·975	79·7	—	—	Magnetism, 75·8 when $H = 8000$
15 *b*	Do., second piece	,,	,,	,,	—	—	0·980	,,	—	—	
16 *a*	White Swedish iron (quenched just after solidifying)	,,	,,	,,	—	—	0·971	76·5	—	—	
16 *b*	Do., second piece	,,	,,	,,	—	—	0·969	75·7	—	—	

17 *a*	White Swedish iron (quenched when molten) ...	„	„	„	—	—	0·975	58·2	—	—
17 *b*	Do., second piece	„	„	„	—	—	0·980	64·0	—	—
17 *c*	Do., third piece	„	„	„	—	—	0·978	72·0	—	—
18 *a*	Cast iron (quenched when molten)	4·06	0·04	0·090	—	—	0·955	69·2	—	—
18 *b*	Do., second piece	„	„	„	—	—	0·894	75·0	—	—
19	4147/104	0·24	0·59	1·040	—	—	0·979	96·2	92·5	3·40
20 *a*	6	0·50	0·65	1·000	—	—	„	97·2	—	—
20 *b*	6 (second piece)	„	„	„	—	—	0·996	97·0	—	—
21	611	0·58	0·49	0·580	—	—	0·974	96·6	93·0	2·56
22	1420 A	0·75	—	1·000	—	—	0·967	94·2	—	—
23	613	1·05	—	0·580	—	—	0·981	94·1	83·5	6·43
24	614	1·20	0·46	0·620	—	—	0·980	93·8	„	„
25	1392 E	1·68	0·13	1·110	0·045	—	0·972	90·5	—	—
	IRON SILICON									
26	803	0·67	2·281	0·500	—	—	0·958	96·4	—	—
27 *a*	803 (quenched at 1050° C.) ...	„	„	„	—	—	0·954	89·0	—	—
27 *b*	Do., second piece	„	„	„	—	—	0·958	90·2	—	—
28	898 E	0·20	2·670	0·250	—	—	0·979	96·1	98·0	0·90
29 *a*	898 L	0·34	2·770	—	—	—	0·975	96·6	—	—
29 *b*	898 L (annealed at 800° C.) ...	„	„	—	—	—	0·974	96·8	—	—
29 *c*	Do., second piece	„	„	—	—	—	0·981	96·9	—	—
30	898 L (quenched at 1050° C.) ...	„	„	—	—	—	0·968	93·0	—	—
31	898 K	0·11	3·890	0·020	—	—	0·963	93·8	—	—
32	898 M	0·07	3·030	0·064	—	—	0·977	95·9	—	—
33 *a*	898 H (as rolled)	0·26	5·530	0·290	—	—	0·965	92·6	95·0	0·85
33 *b*	898 H (quenched from 1050° C.)	„	„	„	—	—	0·960	92·0	—	—
34 *a*	1894 A (as rolled)	0·15	3·260	—	0·96	—	0·977	95·0	—	—
34 *b*	1894 A (annealed)	„	„	—	„	—	0·979	95·9	—	—
35	5 per cent. Si iron (cooled in furnace from 850° C.)	—	5·080	—	—	—	0·960	80·0	—	—
36 *a*	5 per cent. Si iron (quenched just after solidifying)	—	„	—	—	—	0·892	79·0	—	—
36 *b*	Do., second piece	—	„	—	—	—	0·877	76·2	—	—

APPENDIX IV—*(continued)*

No.	Maker's mark	Analysis					Relative density	Relative magnetism (saturation) pure iron = 100	Relative magnetism $H = 45$ (Barrett) pure iron = 100	Coercive force (Barrett)	
		C	Si	Mn	Al	Other elements					
	Iron Manganese										
37	1379 B_2	0·08	0·130	3·50	—	—	0·9890	90·9	—	—	
38	1323 C	0·15	0·370	5·40	—	—	0·9960	93·6	—	—	
39	1379 D	0·16	0·630	10·08	—	—	0·9950	44·5	—	—	
40	1379 D_2	0·15	—	15·27	—	—	1·0020	4·5	—	—	
41	53	0·41	0·070	2·23	—	—	0·9850	95·0	87·5	6·0	
42	1381	0·78	—	3·81	—	—	0·9640	54·3	—	—	
43	34	0·36	0·100	4·68	—	—	0·9720	91·6	49·5	19·60	
44	945 A	1·00	—	7·00	—	—	0·9850	74·0	23·0	20·00	
45 *a*	1310 B	1·66	—	11·53	—	—	—	42·0	—	—	
45 *b*	Do., second piece	,,	—	,,	—	—	—	41·5	—	—	
46	1310 B (heated to redness and cooled in air) ...	,,	—	,,	—	—	—	2·0	—	—	
47	1010 W.T.	1·23	—	12·64	—	—	1·00	—	—	—	Five pieces tested; all less than $\frac{1}{200}$ of S.C.I.
48	1338 B_2	0·26	—	13·00	—	—	0·9980	15·0	0·7	—	
49	30	1·50	0·14	15·22	—	—	,,	8·5	0·8	—	
50	598	1·54	—	18·50	—	—	0·9814	0·0	0·5	—	
	Iron Chromium										
51	993	0·88	0·19	0·28	—	1·75 Cr	0·980	85·0	—	—	
52 *a*	1177 I	0·43	0·32	0·25	—	3·28 Cr	0·982	93·0	—	—	
52 *b*	1177 I (second piece)	,,	,,	,,	—	,,	0·980	,,	—	—	
53	1177 N	1·09	0·45	0·10	—	9·55 Cr	0·993	82·5	—	—	

	Iron Nickel									
54	1397 B	0·26	0·33	0·18	—	0·58 Ni	0·990	98·5	—	—
55	1420 B	0·79	—	—	—	0·76 Ni	0·987	93·6	—	—
56	1392 D	0·83	0·18	0·39	—	0·78 Ni	0·980	95·5	—	—
57	1287 D	0·14	0·21	0·72	—	1·92 Ni	0·986	98·0	95·5	2·67
58	1287 E	0·19	0·20	0·65	—	3·82 Ni	0·987	98·5	96·5	2·76
59	1287 I	0·18	0·22	0·93	—	11·39 Ni	0·991	96·0	48·5	17·33
60	1447 B	0·97	0·56	0·61	—	12·08 Ni	0·977	84·3	24·9	22·40
61	1287 K	0·19	0·27	0·93	—	19·64 Ni	0·989	87·8	46·0	20·00
62	1287 L	0·16	0·30	1·00	—	24·51 Ni	0·999	61·0	25·0	22·50
63	1287 L (heated to 550° C. and cooled in air)	,,	,,	,,	—	,,	1·015	,,	—	—
64	1449 A	0·70	—	0·82	—	31·40 Ni	1·001	24·0	17·0	0·50
65	1798 H_2 (forged and cooled in air from 550° C.) ...	0·48	—	1·34	—	19·98 Ni	1·036	0·0	—	—
	Iron Tungsten									
66	1294 F_1	0·16	0·05	0·11	—	1·10 W	1·006	98·5	95·5	3·25
67	1294 H	0·28	0·06	0·28	—	3·40 W	1·009	95·5	93·5	5·73
68	1294 I_2	0·38	0·11	0·20	—	7·47 W	1·027	92·5	91·0	9·02
	Iron Copper									
69	1264 A	0·68	0·04	0·36	—	1·59 Cu	0·9788	95·3	87·0	5·0
70	1264 B	0·59	0·07	0·32	—	2·49 Cu	0·9935	94·2	85·0	5·4
	Iron Manganese Copper									
71	1263 C	0·17	0·15	1·04	0·997	2·87 Cu	0·9978	95·1	—	—
	Iron Aluminium									
72 *a*	1167 H_3	0·19	0·07	—	2·45	—	0·958	97·2	100·5	1·00
72 *b*	1167 H_3	,,	,,	—	,,	—	—	96·5	—	—
73	1167 D	0·17	0·10	0·18	0·85	—	0·979	97·6	95·5	1·80
74 *a*	1167 K	0·10	0·11	0·11	2·90	—	0·9430	97·2	—	—
74 *b*	1167 K	,,	,,	,,	,,	—	—	96·5	—	—

APPENDIX IV—(*continued*)

No.	Maker's mark	Analysis					Relative density	Relative magnetism (saturation) pure iron = 100	Relative magnetism $H = 45$ (Barrett) pure iron = 100	Coercive force (Barrett)
		C	Si	Mn	Al	Other elements				
	Iron Nickel Copper									
75	1252 B	0·18	0·33	1·10	—	5·81 Ni, 2·87 Cu	0·9915	82·2	—	—
	Iron Ni-Cr									
76	1663	0·60	—	—	—	1·96 Ni, 2·00 Cr	0·992	82·5	—	—
77	1286 A	0·25	0·26	0·40	—	2·67 Ni, 0·64 Cr	0·981	98·2	90·0	3·0
78	1480	0·89	0·20	0·14	—	1·92 Ni, 2·00 Cr	0·988	89·5	—	—
79	1286 C	0·31	0·31	0·39	—	2·60 Ni, 1·80 Cr	0·990	95·7	90·0	7·9
80	1775	0·17	—	0·17	—	3·02 Ni, 1·55 Cr	0·997	95·0	—	—
81	1327 C	0·86	0·22	0·60	—	3·22 Ni, 1·79 Cr	0·993	87·8	—	—
82	1734	0·44	—	0·32	—	3·50 Ni, 1·71 Cr	0·995	94·2	—	—
83	1210 D	0·41	0·24	Trace	—	2·60 Ni, 4·41 Cr	0·997	92·2	81·0	13·1
84	1450	0·64	—	0·54	—	12·24 Ni, 2·01 Cr	1·004	36·5	—	—
	Iron Ni-Si									
85	1103 A	0·38	2·07	0·54	—	3·30 Ni	0·972	95·3	94	2·0
86	1103 C	0·22	3·22	0·80	—	3·53 Ni	0·974	94·3	92·5	1·9

		Iron Ni-Mn										
87	1254 C	... ...	0·57	0·31	3·75	—	3·92 Ni	0·988	79·2	38·0	19·6	
88	1287 A	... ...	0·19	„	0·79	—	0·27 Ni	0·995	98·8	—	—	
89	1267 B	... ...	0·78	0·60	1·06	—	4·87 Ni	0·994	90·2	—	—	
90 *a*	1339	... ...	1·21	—	8·00	—	2·57 Ni	1·015	1·5	—	—	
90 *b*	1339	... ...	„	—	„	—	„	0·980	1·2	—	—	
91	1313 C	... ...	1·40	0·70	13·40	—	9·25 Ni	0·997	0·5	0·5	—	
92	1109 D	... ...	0·60	0·84	5·04	—	14·55 Ni	0·994	„	—	—	
93 *a*	1414 A	... ...	1·00	—	6·05	—	17·91 Ni	1·017	1·0	—	—	
93 *b*	1414 A	... ...	„	—	„	—	„	1·007	1·5	—	—	
94 *a*	1414 B	... ...	1·18	—	„	—	24·30 Ni	1·004	4·5	—	—	
94 *b*	1414 B	... ...	„	—	„	—	„	1·012	1·5	—	—	
		Iron Mn-Cr										
95	1274 A	... ...	1·14	0·74	4·39	—	4·95 Cr	0·997	2·8	—	—	
96	1430	... ...	1·30	—	3·09	—	8·92 Cr	0·971	73·5	—	—	
97	1233 A	... ...	1·13	0·75	2·60	—	9·22 Cr	0·987	8·7	—	—	
98 *a*	620	... ...	0·88	0·46	17·56	—	3·60 Cr	0·974	3·0	0·4	—	
98 *b*	620	... ...	„	„	„	—	„	0·995	3·2	—	—	
		Iron Mn-W										
99	687	... ...	0·40	0·79	2·28	—	3·20 W	1·008	92·2	91·0	6·1	
100	1234 B	... ...	1·28	1·05	1·0	—	7·90 Cr	—	2·0	—	—	
101 *a*	1343 A	... ...	1·34	0·22	11·10	—	2·85 W	0·998	3·0	—	—	
101 *b*	1343 A	... ...	„	„	„	—	„	1·020	3·2	—	—	
102 *a*	1343 B	... ...	1·08	0·51	10·20	—	2·11 W	1·013	0·2	—	—	
102 *b*	1343 B	... ...	„	0·50	„	—	„	1·003	„	—	—	
		Iron Mn-Si										
103	601	... ...	0·40	4·27	1·90	—	—	0·954	91·8	—	—	
		Iron Mn-Cu										
104	1240 I	... ...	0·25	0·31	2·01	—	{1·39– 1·45 Cu}	0·996	94·7	—	—	
105	1260 A	... ...	0·64	0·39	8·06	—	2·65 Cu	1·004	48·9	—	—	

APPENDIX IV—(*continued*)

No.	Maker's mark	Analysis					Relative density	Relative magnetism (saturation) pure iron = 100	Relative magnetism $H = 45$ (Barrett) pure iron = 100	Coercive force (Barrett)
		C	Si	Mn	Al	Other elements				
	Iron Cr-Al									
106	1178 B	0·21	0·22	0·07	0·75	1·69 Cr	0·977	95·2	86·5	6·00
107	1179 B	0·46	0·34	0·18	1·06	3·57 Cr	0·965	91·6	80·5	8·00
108	1178 D	0·18	0·25	0·12	2·40	1·56 Cr	0·957	93·4	83	3·52
109	1178 E	0·22	0·19	0·08	4·40	1·60 Cr	0·921	91·7	78·5	1·77
	Iron Cr-Si									
110	518	0·76	1·02	0·29	—	2·11 Cr	0·977	88·2	—	—
111	517	0·86	1·96	0·40	—	1·96 Cr	1·016	86·2	—	—
112	1185 F	0·54	2·20	0·22	—	3·50 Cr	0·977	90·2	—	—
	Iron Cr-Cu									
113	1255 A	0·85	0·31	0·50	—	1·83 Cu 5·79 Cr	0·982	86·7	—	—
	Iron Cr-W									
114	1189 B	0·26	0·05	0·25	—	0·66 Cr 1·99 W	1·004	94·7	95·5	5·30
115	1773 B	0·76	—	0·50	—	2·75 Cr 20·00 W	1·098	48·7	—	—
	Iron Co-Mn-Si									
116	1209 C	0·25	0·64	1·04	—	1·80 Co	0·978	98·6	98·8	—
117	1209 F	0·52	0·79	0·79	—	6·91 Co	„	98·9	—	—

		IRON NI-MN-CU											
118 *a*	1424 B	...	...	...	...	0·83	—	5·90	—	{14·44 Ni, 2·25 Cu}	1·007	0·7	—	—
118 *b*	1424 B	...	...	...	...	„	—	„	—	„	1·011	„	—	—
		IRON CR-MN SI											
119	608	...	...	...	...	1·32	1·50	4·23	—	2·02 Cr	0·9762	79·9	—	—
		IRON NI-MN-AL											
120	1411	...	...	...	...	0·43	0·61	5·30	2·30	14·10 Ni	0·9615	74·4	—	—

Specimens 13 *a*, 13 *b*, 14 and XIV *a* were heat-treated in the form of a small fragment of the ingot, weighing about one ounce. Specimen XIV *b* was heat-treated in the form of a ground specimen $\frac{1}{4}$ in. in diameter by 1 in. long.

Specimens 15 *a*, 15 *b* and 35 were small fragments also weighing about one ounce, the piece for the magnetic test being cut out of the heated piece.

Specimens 16 *a*, 16 *b*, 17 *a*, 17 *b*, 17 *c*, 18 *a*, 18 *b*, 36 *a* and 36 *b* were quenched in water from the crucible in which they were melted, and the magnetic test-piece cut out afterwards.

All other specimens were heat-treated in the form of short rods 1 inch long by 0·22 in. diameter, the magnetic test-piece being afterwards cut out. To avoid superficial decarburisation, the specimens were heated as quickly as possible for temperatures up to 1050° C., and those about 1050° C. were encased in an iron sheath. Although this may not have entirely done away with decarburisation, it is believed the method adopted was sufficiently effective to prevent any loss of carbon taking place from the centre of the mass from which the magnetic test-piece was taken.

Replying to the discussion Professor B. Hopkinson said: Professor S. P. Thompson asked a number of questions, and also asked for further information on certain points. I can answer him with regard to several of them at once, but others may require further consideration. He inquired to what value H was carried for the various pieces in Appendix IV. I think in every case, with quite a few exceptions, those pieces were tested up to a maximum of 25,000 C.G.S. units. That was the highest we could reach. There were a few which were taken up to about 14,000 or 15,000 and they were done by that differential

method which I have described. At all events, every piece was tested up to saturation. Saturation was shown either by comparison of I at 25,000 with the value at 2000, or else it was shown by the straightness of the magnetising curve. He also asked for the ultimate permeability, and suggested that I might broaden the table. There was a good deal of information we should have liked to put into that table, but I am afraid if we had done so it would have become of inordinate breadth. The value of I is given in the table as a percentage of the value of pure iron, and the absolute value is at once obtained by multiplying by 1680. The permeability is then readily worked out; in the case of pure iron the permeability in a field of 25,000 is 1·85. Professor Thompson made some remarks of considerable interest about the permeability of the non-magnetic manganese steel. He said that he had always had difficulty in understanding how this body could have a constant permeability of 1·4. My father, Dr John Hopkinson, found a constant permeability of about that amount, but he worked with weak fields. I think there can be little doubt now that there is no steel which has a constant permeability of 1·3 or 1·4 in fields exceeding 1000 c.g.s. None of the manganese steels examined by us have that property. The reason of the apparent discrepancy with my father's result is, I think, not difficult to understand. In low fields, such as he experimented with, viz., up to about 200 units, it might well happen that a steel would show a constant permeability of 1·3 or 1·4. That would occur if the material contained only a small amount of magnetic stuff, and was therefore magnetically speaking, very soft. Yet that same material could easily have, in consequence of the small quantity of magnetisable material, a very small ultimate magnetic intensity. The actual permeability of the manganese steel 1010 W.T. that we experimented with in these high fields certainly did not differ from unity by more than 1 per cent.—of that there is no doubt at all—but it is quite possible that if we had been able to test it in a field of 200 units we should have found a permeability materially exceeding unity. Ewing's result obtained with that same steel is more difficult to explain. I have put a possible explanation in a footnote in the paper. Professor Thompson remarked that no end correction was needed for the non-magnetic materials. Of course that would be so, and when I spoke of the end correction of 16 per cent. I meant the correction applied to the measurement of I. In a nearly non-magnetic material 16 per cent. of the magnetism is an unmeasurable quantity from our point of view. As regards the effect of quenching, which was also referred to by Professor Thompson, and the possibility of discriminating between α, β, and γ iron, that is a very thorny question. I think it may be possible in the future to learn something about the modifications of iron by these means, but at present I think it better to say nothing about them. "Magnetic hardness" is a phrase which I believe was used by Barrett, Brown, and Hadfield in the joint paper read before the Institution some years ago. It was the reading of that paper which suggested the phrase to me, and I have used it in much the same sense as Barrett did. I do not attach any definite numerical meaning to it, but I use it to express in a general way the rate at which the magnetism of the material approaches the saturation value.

Mr Sears asked whether it had been proved that a compound does behave as a mixture. I should put it a little differently—rather that a mixture behaves as a mixture. Take the instance of an iron-carbon alloy. In the case of an annealed iron-carbon alloy we have proved experimentally that the loss of magnetism is a linear function of the amount of carbon. There is good reason to suppose that the stuff is a mixture of iron and iron carbide, and that those two constituents

do not influence each other magnetically. On that assumption we have calculated from the results what the magnetism of the iron carbide is. Of course it may be as Mr Sears suggests, that the presence of the iron affects the magnetism of iron carbide, but I do not think that is so, because we are there dealing undoubtedly with a mixture. The microscopical results prove that, and chemical analysis proves that the constituents of the mixture are iron and iron carbide of the composition Fe_3C. We know, as certainly as we know anything about a mixture of that type, that the magnetism of the whole would be the sum of the magnetism of the constituents, and we have used that fact to find out what is the magnetism of the second constituent, a quantity hitherto unknown. That is really what that experiment amounts to. Mr Mordey raised one point which I will refer to because it has an important bearing on the methods of investigation described in this paper. He said that pure iron was a most unsatisfactory material because of its want of magnetic permanence. He said it was very unstable, and that he could hardly look at it without its magnetic properties changing. That is true, of course, in the fields employed in the ordinary methods of testing, but it quite ceases to be true when you push the induction to very high values, and that fact illustrates very well, I think, the value of this method of testing as a means of research—not so much for finding out magnetic properties which are likely to be of practical use, but as a method of research into the constitution of materials. The same pure iron which in low fields is completely upset magnetically by, say, a small amount of mechanical disturbance—a little hammering, or something of that kind, which does not distort it in any way—is wholly unaffected, if you only push the induction sufficiently high, even by a very large amount of mechanical disturbance. I have tried, for instance, taking a piece of nearly pure Swedish iron and placing it between the poles of a big magnet, subjecting it to a force of about 5000, which is adequate to saturate it, and pulling the piece out by means of rods with a screwing arrangement going through the pole-pieces, so as to give it a considerable amount of permanent extension, stretching it, in fact, beyond its elastic limit, and even when subjected to that drastic treatment the saturation value of the magnetism of the iron remains quite unaffected. It is, in fact, a remarkably constant property, and a variation in it implies a real change in the nature of the material, and not a mere rearrangement of parts. We did not make any measurement of specific resistance nor of hysteresis losses. Measurements of that kind were made on a considerable number of these materials by Barrett and Brown and Hadfield.

Mr Murdoch referred to the unsatisfactory character of the ballistic method of testing. Here again I think there is something to be said for that view as applied to low fields, but I do not think it affects the matter very much in very high fields when the material becomes saturated. It does not matter how you approach the ultimate magnetic force so long only as it is high enough. The same intensity of magnetism is always reached. Apparently there is no time effect. Of course all these things are to be expected according to Ewing's theory. Mr Stoney asked whether we could not get a non-magnetic steel with 30 to 40 tons tensile strength and good elongation. The interest of that remark to me is that it suggests the possibility of using this method of research, not so much for testing properties of immediate use in electrical engineering, but as a means of investigating the constitution of steels and so producing new steels having different magnetic properties and also different mechanical properties from the older varieties. Mr Morris asked a question as to the manner in which the saturation value of I varies with the temperature. That is a matter which

has recently been investigated by an advanced student at Cambridge, Mr E. F. Clark, who has fully confirmed the conclusions to which M. Curie was led some years ago by experiments in rather lower fields. The effect of raising the temperature is to reduce the saturation value of the intensity of magnetism. I am speaking now of pure or nearly pure iron. It reduces the saturation intensity from the very beginning. An increase of temperature even of 400° C. reduces the maximum intensity of magnetisation, and the reduction goes on at an increasing rate until the critical point of about 750° C. is reached, when the magnetism disappears altogether. It disappears rather suddenly at the finish, but it diminishes steadily from the very beginning. I think some reduction is perceptible even at 200° C. At all temperatures the material shows saturation just as distinctly as it does at low temperatures. Those results are rather suggestive as to the nature of magnetism. The fact of a reduction of magnetism produced by heating, say, to 400° C., proves to my mind that the temperature has a direct effect upon the individual molecules or magnets. The interesting question is, how does it affect them? Does it destroy the magnetism of some by changing their constitution completely and leave the magnetism of the others unchanged, so that what we have got at a higher temperature is fewer magnetic molecules, but those few of the same kind as before? Or does it change all the molecules in a gradual way? I confess I think the first hypothesis seems a good deal the more probable. It seems to me that probably the effect of temperature, which we conceive as a motion of the molecules as a whole rather than as an internal movement, is to change the nature of some molecules and leave the others totally unchanged. That, however, is largely a matter of speculation; the facts are as I have stated.

Communicated: Professor Arnold suggested that the falling off in the magnetic capacity of quenched steels is possibly due to cracks. Professor MacWilliam made a similar suggestion with regard to the results obtained with 1391 B. I do not think, however, that such cracks can affect the matter in the intense fields used in this research, though in the moderate fields employed in ordinary testing their effect would be considerable. It is indeed one of the chief advantages of magnetic testing in a very strong field, regarded as a means of investigating the constitution of steel, that the ultimate magnetism of steel (which is revealed in such tests) is independent of such accidental factors as the presence of cracks, the size of crystalline grains, etc.

Mr Hoyle asks about the relation between the "end effect" and the length of the specimen. In the tests between flat pole-pieces in fields of about 5000 C.G.S. the end effect is equivalent to a constant addition to the length of the specimen amounting to about 0·12 mm. I think there is no doubt that the correction can be applied in this form on specimens of about a quarter of an inch long, such as are described in the paper, but it would not be advisable to apply it to shorter specimens since the ends would interfere with each other. I do not think that the size of the pole-faces makes much difference so long as it is adequate to give a uniform field along the whole length of the specimen. The junction between the specimens and the pole-faces plays a large part in determining the end effect in moderate fields, but is practically without influence when H exceeds about 5000 units; this is shown in Fig. 4 of the paper.

The expression in Appendix III to which Mr Hoyle refers means that the difference has to be taken between the values of the expression in the square bracket, when for z and r are substituted, first, z_2 and r_2 respectively, and second, z_1 and r_1 respectively.

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THE MAGNETIC AND MECHANICAL PROPERTIES OF MANGANESE STEEL*.

[By Sir R. A. HADFIELD, F.R.S., and B. HOPKINSON, F.R.S. "JOURNAL OF IRON AND STEEL INSTITUTE," 1914.]

THE present paper is mainly concerned with the well-known alloy of iron with about 12 per cent. manganese and $1\frac{1}{4}$ per cent. carbon, known as "manganese steel," which was discovered by one of the authors in 1883. For ordinary commercial use this material is generally heat-treated, the treatment consisting in heating to about 1000° C. and quenching in water. It is then very tough and strong; a piece $\frac{5}{8}$ inch by $\frac{3}{8}$ inch section can be bent double without fracture, and the Brinell hardness number is about 200. Its tensile strength varies from about 54 to 63 tons per square inch, with 30 per cent., and in some cases 50 per cent., elongation. It is also practically non-magnetic, the permeability, in fields of high density ($H = 2000$ or more), not exceeding 1·01. It was observed by one of the authors many years ago that by heating at a high temperature followed by slow cooling, the material could be made magnetic and very brittle. In a recent paper by the authors† some measurements of the magnetism of the steel so treated were recorded, and it was suggested that the important part in the treatment was probably the speed of cooling rather than the prolonged heating which preceded it. The present research is a further study of this problem.

The tests were carried out on material made at the Hecla Works, different samples of which contained from 1·2 to 1·3 per cent. of carbon and from 12 to 12·6 per cent. of manganese. The magnetic tests were made by the differential method described in a previous paper†. The test-piece is a cylinder $\frac{1}{8}$ inch in diameter and $\frac{1}{4}$ inch long. This is placed between the parallel pole faces of an electro-magnet along with a comparison piece of brass. The test-piece and comparison piece were placed in a pair of testing-coils consisting of 16 turns of No. 30 S.W.G. copper wire wound on a thin brass tube. The two coils were cemented together with their axes parallel. The areas of the coils were adjusted to exact equality, which could be

* In a preliminary account of this research, communicated by Sir Robert Hadfield to the American Institute of Mining Engineers at New York in February 1914, and published in their *Transactions*, the problem of the relation of the magnetism to the other physical qualities of the material is discussed from various sides.

† *Proceedings of the Institution of Electrical Engineers*, vol. 46, p. 235. Page 121 of this volume.

tested by connecting them in series and in opposition between the parallel faces of the magnetic poles, and reversing the current. The fling obtained when one coil contains a magnetic test-piece and the other a brass dummy is then proportional to the value of I, the magnetising force term H in the flux for each coil ($H + 4\pi I$), which is the same for the two coils being automatically deducted. The magnetism can thus be immediately compared with that of pure iron by substituting for the magnetic test-piece a standard iron piece of the same dimensions. The standard iron used for this purpose was that known as "S.C.I.," and contained about 0·01 per cent. of impurities. The magnetism is expressed as a percentage of that of pure iron, and this percentage is probably correct to within 0·5; the chief possible source of error is that dependent on the measurement of the dimensions of the test-piece. Where a steel is only slightly magnetic, however, so that this source of error is practically without effect, the measurements of magnetism are correct to within 1/500 part of the magnetism of pure iron.

The material as first prepared and water-toughened has no magnetism that can be detected by this method of testing, though in most specimens a slight attraction is perceptible with a hand magnet if the specimen is in the form of a small rod suspended by a thread. Pieces were heated at various temperatures which could be accurately controlled and kept constant. These treatments were carried out partly at Sheffield and partly at Cambridge. At Sheffield the water-toughened rod, $\frac{1}{4}$ inch in diameter, was packed in lime and heated in a gas-furnace for the desired period; it was then allowed to cool in air, and two magnetic test-pieces were subsequently prepared from it by grinding and tested for magnetism. In the treatments at Cambridge the test-piece was first prepared from the original non-magnetic material and tested for magnetism. It was then placed in a hard glass tube, which was exhausted of air and sealed up. The heating was done in a small electric furnace containing a recording pyrometer. It was found that by this procedure oxidation was completely prevented at the temperatures employed.

The results obtained by heating for various periods to a temperature of about 500° C. are shown graphically in Fig. 1. This curve is shown in two parts, on different time scales. The heat treatments corresponding to the curve marked "500° C." were carried out at Sheffield, and the plotted points correspond to different pieces of material. The curve marked "520° C." was determined at Cambridge on four pieces, two of which had previously been heated for 60 hours at 500° C. at Sheffield, while the other two were in the original water-toughened and non-magnetic condition. These four pieces were used throughout the test, being heated together in the furnace for a number of hours, taken out and tested, and then replaced and the heating continued. For the purpose of plotting, the two pieces which had been heated at Sheffield were credited at the start with 60 hours at

520° C. There may have been a difference of a few degrees between the Cambridge and Sheffield temperature scales, as no special precautions were taken to get exact correspondence. It will be seen that the approach to the ultimate state of equilibrium follows the course usual in such cases, being rapid at first and subsequently becoming much slower. It would appear that the curve would, if sufficiently prolonged, become asymptotic to a line representing the limit of magnetism possible at that particular temperature. The test was not sufficiently prolonged to enable an accurate judgment to be formed as to the place of this asymptote. It seems probable the change was still continuing even after 600 hours, and that the ultimate equilibrium form of this steel would possess an amount of magnetism exceeding two-thirds of that of pure iron. By heating at temperatures below 520° C.

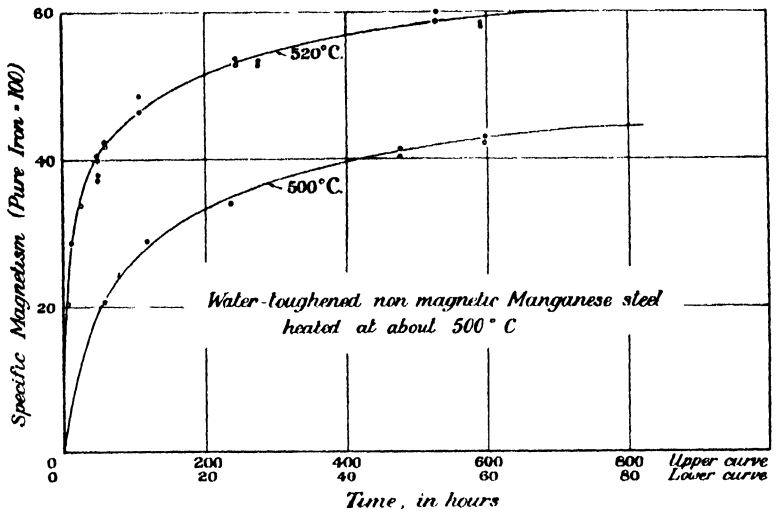


Fig. 1.

the material could be rendered magnetic, but the change was much slower. After fifty hours at 450° to 500° C. the magnetism was only 7 per cent. of pure iron, but was still increasing. A piece which had been kept for twenty-three days at 300° C. showed just perceptible magnetism. At higher temperatures, up to about 650° C. the change was relatively rapid, but the asymptote to which the curve approaches appears to be lower than that proper to 520° C., at which temperature the material became more magnetic than it could be made by any other treatment. At 600° C. the magnetism went to 27 per cent. in ten hours, to 33 per cent. in twenty-six hours, and to 34 per cent. in thirty-five hours. Heating at 700° C. rendered the material only slightly magnetic.

Experiments were then tried on the destruction of the magnetic quality which had been induced by the heat treatment just described. For this purpose a series of pieces of the water-toughened material were heated

at 520° C. for a period long enough to make them about half as magnetic as pure iron. After heating for a certain period at the desired temperature each piece was quenched in water in its glass case, tested for magnetism, sealed up again, and the treatment continued. It was found that:

(1) Heating the magnetic material at 550° C. or at any lower temperature did not diminish the magnetism, which remained about half that of pure iron. It will be remembered that prolonged heating of the original non-magnetic water-toughened metal at 450° C. or lower temperatures did not confer anything approaching this amount of magnetism.

(2) Heating at a temperature exceeding 640° C. resulted in diminution of magnetism, the rate of diminution becoming more rapid, and the ultimate value reached becoming less as the temperature rose. The magnetism is almost completely destroyed by a few minutes' heating at 750° C.; and the resulting product approaches the water-toughened condition, but is less tough and shows distinctly more magnetic attraction to the poles of a magnet. At 655° C. the magnetism does not entirely disappear; it settles down to an apparently constant value of about 31½ per cent. after thirty hours. It is interesting to note that by heating the non-magnetic material at 655° C. the magnetism is only raised to 16 per cent., and it would appear probable that, according as the material approaches equilibrium at any temperature (below 650° C.) from the magnetic or from the non-magnetic condition, it may exist in either of two states, and that these two states cannot be brought into identity by prolonging the heating.

The following are details of the effects of heating the magnetic material to various temperatures. Substantially the same results were obtained whether the material were quenched out from the high temperature or allowed to cool slowly in air. Initially the magnetism was in all cases about 50 per cent.:

640° C. Reaches constant value of about 48·5 per cent. in fifteen hours.
655° C. Constant at 31 per cent. after thirty-one hours.
670° C. After fifteen minutes 28·5 per cent.; after nineteen hours constant at 16 per cent.
690° C. After ten minutes 7·5 per cent.; after one hour constant at 1·5 per cent.
720° C. After five minutes 2 per cent.; after fifteen minutes 1 per cent.
750° C. After five minutes 1 per cent.
780° C. After five minutes 0·5 per cent.

Heating to 750° C. for one hour followed by quenching or by cooling in air reduces the magnetism to a mere trace.

Effect of Immersion in Liquid Air.

The effect of liquid air temperatures on manganese steel is very interesting. The water-toughened steel remains completely non-magnetic after immersion in liquid air, and steel which has been heated so long at 520° C. as to approach the maximum susceptibility of which it is capable is also unaltered by this treatment. But steel which is in an intermediate

condition, having been made partially magnetic by heating for a few hours to about 500° C., is made more magnetic by immersion in liquid air. These results are exhibited in the following table, the magnetism in the second column being that found at ordinary temperatures after removal from the liquid air bath:

Magnetism before immersion	Magnetism after immersion	Increase
Per cent.	Per cent.	Per cent.
0·0	0·0	0·0
2·6	3·0	0·4
9·2	18·5	9·3
15·0	26·4	11·4
25·0	39·0	14·0
31·0	38·0	7·0
46·0	47·0	1·0

The change here indicated seems to take place very rapidly, the time of immersion having no effect, so long as it is sufficient to allow the whole mass to acquire the temperature of the liquid air. That water-toughened manganese steel remains non-magnetic after immersion in liquid air, but that the same steel in the magnetic condition may be made more magnetic by this treatment was observed by one of the authors in 1905*. It is possible that this effect is due to internal pressures produced by unequal contraction of the magnetic and non-magnetic constituents. Manganese steel which is partially magnetic probably consists of a net-work of hard material enclosing separate masses of more ductile stuff. Any difference in the contraction of the two constituents will produce internal stresses in each, the magnitude of which depends on their relative amounts. A small quantity of either constituent will yield, and give small stress; maximum stress will be reached when the quantities are more or less equal. Thus cooling will produce a maximum effect when an intermediate amount of magnetism is already present; there will be no effect if there is no magnetic material, nor if most of the material has already been otherwise made magnetic.

It may be noted as bearing on this point that cold work makes the water-toughened steel slightly magnetic. A magnetic test-piece cut from a bar which had been broken in a testing-machine showed specific magnetism 0·8 per cent., and hardness 540. It was also found by one of us that the non-magnetic form has greater density than the magnetic—there is an increase of volume on the acquisition of magnetism. In the same way the nickel steel containing about 24 per cent. nickel and low carbon changes from non-magnetic to magnetic when broken at ordinary temperatures and at the same time increases in volume†.

* Hadfield, *Journal of the Iron and Steel Institute*, 1905, vol. 67, p. 198.

† J. Hopkinson, *Proceedings of the Royal Society*, vol. 48, p. 7, and vol. 50, p. 121.

Heating and Cooling Curves.

In all steels the change from the magnetic to the non-magnetic form, or *vice versa*, is accompanied by the absorption or evolution of heat. Water-toughened manganese steel remains non-magnetic when heated above 750° C., and the magnetic changes which occur at lower temperatures are excessively slow. It is therefore not surprising that no critical points have been detected in this material hitherto. No magnetic change occurs in the course of any ordinary heating or cooling of the non-magnetic material. The experiments just described, however, showed that steel which has been brought into the magnetic form by prolonged heating at about 500° C. loses its magnetism rapidly at a temperature in the neighbourhood of 700° C. It was anticipated that this comparatively sudden change would be accompanied by a perceptible absorption of heat. The heating curves in Fig. 2, which were obtained at Sheffield by the inverse-rate method, clearly show such a heat absorption, and confirm the existence in this material of a change point at about 700° C. No corresponding evolution of heat is apparent on cooling the material again through the critical point, because, with the rate of cooling adopted, there is not time for the change to occur. Only by excessively slow cooling, occupying several days, can the reverse change from non-magnetic to magnetic be brought about. With sufficiently slow cooling the change does occur at about the same temperature, and there can be no doubt that it is accompanied by evolution of heat; but being spread over such a long period of time it is impossible to detect this evolution.

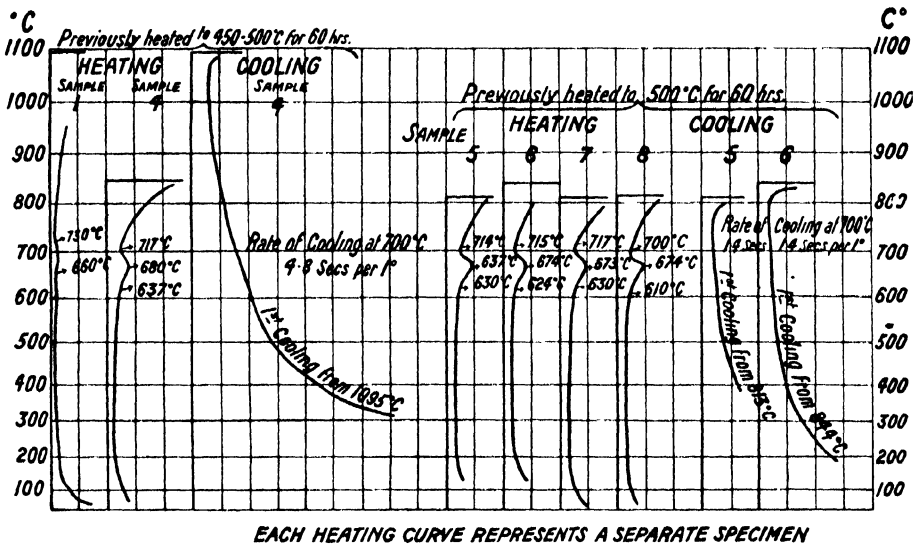


Fig. 2. Forged Material. Carbon, 1·26 per cent. Silicon, 0·32 per cent. Manganese, 13·38 per cent. Previously made magnetic by heating to about 500° C. for several hours.

The explanation of these effects appears to be that the stable form of the alloy at temperatures below about 750° C. is more or less magnetic, the proportion of magnetic substance present in the equilibrium state diminishing rapidly when the temperature approaches that figure. Above 750° C. the magnetism is all gone. From about 650° C. to 750° C. there is a critical range similar to that corresponding to the loss of magnetism in ordinary carbon steel. If the alloy be cooled from above this critical range the tendency, as it passes through any lower temperature, is towards the attainment of the amount of magnetism proper to that temperature. But the rate of approach to equilibrium is so slow that even when the cooling takes several minutes but very little of the magnetism is restored, and the effect produced is similar in kind to that produced by quenching a carbon steel. The important effect of the manganese is, apparently, this retardation of the attainment of equilibrium, rather than any very marked shift of the position of the critical range.

It is important to note that the attainment of equilibrium in the manganese steel at temperatures below the change point is opposed not only by resistances of the nature of fluid viscosity, which can be overcome by a very small force if continued long enough, but also by a resistance of the nature of solid friction, which requires a force of a definite amount to be exerted before motion takes place at all. Magnetic manganese steel heated at 650° C. attains ultimately a magnetism about twice as great as that reached by the non-magnetic variety heated at the same temperature. The final condition at this temperature is different according as it is approached, so to speak, from above or below, and, so far as it is possible to judge from observed facts, these two limiting conditions could never be brought into coincidence however much the heating were prolonged.

Mechanical Properties and Microstructure.

As stated above, manganese steel for commercial use is water-toughened by quenching from 1000° C. Before this treatment—that is, after the ordinary cooling in air following casting or forging—it is comparatively brittle, hard, and non-magnetic. After water-toughening, it becomes extremely ductile, remaining of course non-magnetic. For the purpose of the present research a number of bars, about $\frac{3}{8}$ inch × $\frac{5}{8}$ inch section, which had been treated in various ways, were broken by bending between supports $1\frac{1}{2}$ inches apart, and were also tested for hardness with a Brinell machine. The results obtained are given in the table on p. 181, which is arranged in such a way as to show the nature and extent of the correlation between magnetism and mechanical properties.

The water-toughened material bends double without breaking and has a hardness number of about 200. Any heat treatment of the water-toughened material which has the effect of making it magnetic, even to a comparatively slight amount, also renders it brittle and hard. Pieces

having 1 per cent. of the magnetism of pure iron, or even less, have so far lost their toughness as to be unfit for practical use. As the magnetism increases the hardness usually increases also, but the addition to the hardness is small as compared with that accompanying the first traces of magnetism. The following table exhibits the effect of various treatments which leave the material slightly magnetic. In every case the material before treatment was toughened by quenching from 1050° C., and was of course completely non-magnetic.

Treatment	Angle of bend at Breakage, Degrees	Ball hardness	Magnetism per cent.
7 days at 300° C.	161	215	Less than 0·2
23 days at 300° C.	124	217	0·3
16 days at 300° C. followed by 5 hours at 400° C.	21	322	1·25
1 hour at 750° C. quenched in water	29½	277	Less than 0·2
1 hour at 750° C. cooled in air	21	267	Less than 0·2

Heating for three weeks at 300° C. makes the material perceptibly less tough. The loss of ductility caused by a few hours at 400° C. is very marked. The last two treatments leave the material almost, if not quite non-magnetic, but distinctly brittle. Probably they bring the material to much the same condition as that in which it leaves the foundry or forge, when, as we have seen, it is practically non-magnetic and very brittle. The further commercial treatment of water-toughening removes the brittleness without affecting the absence of magnetic quality. As might be expected, though the mechanical properties are evidently connected with the magnetism, they depend on other variables as well. The table on page 181 gives fuller details of the tests.

Finally, some bars of manganese steel were made magnetic and brittle by heating for some hours to 530° C. One of them was then reheated for one hour at 750° C. and quenched out from that temperature. The following were the results of this treatment:

	Angle of bend, Degrees	Brinell hardness	Magnetism per cent.
Before reheating at 750° C.	2 to 3	390 to 440	24
After reheating and quenching	29	277	Less than 0·3

Comparing this with the last test in the previous table, it will be seen that the effect of heating to 750° C. and quenching is to bring both the magnetic and the non-magnetic material into the same condition—a condition in

which it is non-magnetic and fairly ductile, but not so ductile as when it is quenched from 1050° C.

The change from the ductile to the brittle form is accompanied by well-marked changes in the microstructure of the steel. Fig. 3, Plate I, is the usual water-toughened form with fine-grained polygonal structure. When

Magnetic and Mechanical Tests.

(Arranged in Order of Magnetic Quality.)

Specific magnetism S.C.I. 100	Hardness no.	Angle of Bend. Degrees	Treatment
Completely or practically non-magnetic (less than 1 per cent. magnetism).			
Non-magnetic*	207	180 unbroken	W.T.
,,	215	161	300/310 for about 1 week
,,	267	21	750 ,, 1 hour (air)
,,	277	29½	750 ,, 1 ,, (water)
,,	277	29	530 ,, 15 hours 750 ,, 1 hour (water)
0·2 and 0·2	337	8¼	450 ,, 6 hours
Non-magnetic	339	11	530 ,, 15 ,, 750 ,, 1 hour (air)
0·2	340	12	400 ,, 6 hours
0·2 and 0·2	408	2½	650 ,, 3 ,,
0·2 and 0·2	418	2⅝	650 ,, 6 ,,
0·6	340	8½	400 ,, 12 ,,
Partly magnetic (1 per cent. to 12 per cent. magnetism).			
0·3	217	124	300 for 7 + 16 days
1·2	317	26	300/400 ,, 16 days
1·2	328	17	,, ,, ,,
1·0	364	6¼	400 ,, 24 hours
1·2	361	4½	450 ,, 12 ,,
2·0	487	2	400 ,, 48 ,,
2·2	387	3	450 ,, 24 ,,
7·0	418	1¾	450 ,, 48 ,,
12·2 and 12·2	444	1	600 ,, 3 ,,
Very magnetic (20 per cent. magnetism and over).			
19·8 and 20·2	418	1	600 for 12 hours
20·0	418	1	550 ,, 6 ,,
20·4	351	11½	500 ,, 6 ,,
24·4 and 23·4	375	5	600 ,, 24 ,,
24·8 and 25·4	402	4½	550 ,, 12 ,,
25·8	438	2½	550 ,, 24 ,,
28·8	351	2¾	500 ,, 12 ,,
29·8 and 30·2	430	2¼	600 ,, 72 ,,
30·0 and 31	418	3½	600 ,, 48 ,,
31·4 and 31·4	495	1¼	550 ,, 48 ,,
34·4	444	1½	500 ,, 24 ,,
40·2 and 41·0	438	1½	500 ,, 48 ,,
42·0 and 43·2	444	Nil	500 ,, 60 ,,

* "Non-magnetic" here means less than 0·2 per cent. of the magnetism of pure iron— that is, a degree of magnetism which could not be detected by our method of comparison in strong fields. Most of these pieces are attracted faintly by a magnet.

the material has been rendered magnetic and brittle by heating at 500° to 600° C. the structure changes to the needle-like form shown in Fig. 4, Plate II, which bears a considerable similarity to a martensitic steel. The cause of the change of mechanical properties, and the nature of its correlation with the magnetic change, appears, however, more clearly in the series of photographs, Figs. 5–8, Plate III. The pieces here shown were polished without etching. Under this treatment the water-toughened material shows no structure whatever—the field of the microscope is absolutely blank. After heating for six hours at 400° C., followed by polishing, the surface became covered with interlacing lines as in Fig. 5, Plate III. These probably represent plates of hard material which is forming in the mass and whose edges are left up standing by the polishing. The steel has now become brittle; it bends about 13° instead of bending double, and its hardness number is about 340 instead of 200. The magnetism is about 1/500 of that of pure iron—still very small, but quite perceptibly more than in the water-toughened steel. Further heating at 400° C., or at a slightly higher temperature, causes a marked growth in the amount of the hard constituent as shown in Figs. 6, 7, and 8, Plate III. There is a corresponding increase in magnetism, but not much further change in mechanical properties. Apparently the hard constituent forms a stiff unyielding net-work enclosing separate grains of more ductile material. So distributed, a very small quantity can evidently profoundly affect the mechanical properties, and further additions do not produce a proportionate additional change. In the same way a trace of lead added to gold forms a brittle eutectic layer between the crystals and completely destroys the ductility. The hard constituent in the steel is of course not a eutectic, but it is so distributed as to have a similar effect.

The existence of a hard net-work in manganese steel which had been made magnetic by heating was observed in 1894 by Stead*. In 1910 Arnold and Read made a chemical and micrographic analysis of annealed manganese steel (carbon about 1 per cent., manganese 11 to 15 per cent.), which was brittle and must, from its treatment, have been strongly magnetic†. They isolated a carbide, or mixture of carbides, of iron and manganese. It seems probable that the effect of annealing in causing hardness and magnetism is due to some change in the relation of these carbides to the iron. It may be that in the water-toughened steel they are completely diffused or dissolved, whereas in the annealed steel they are in certain parts segregated like the carbide of iron in pearlite. The correlation between microstructure and magnetism shown in Figs. 5–8 suggests that the seat of this change is the constituent which forms the net-work, and that the enclosed grains are unchanged, or only partially changed.

* *Journal of the Iron and Steel Institute*, vol. 45, p. 193.
† *Ibid.*, vol. 81, p. 169.

Magnetic Properties of Carbon Steel.

Effects closely analogous to those which have been described have been observed in a steel containing only a small proportion of manganese. The alloy had the composition: Carbon, 1·96 per cent.; silicon, 0·36 per cent.; manganese, 0·14 per cent., and after annealing at 850° C. and cooling in the furnace it has 88·5 per cent. of the magnetism of pure iron—a proportion corresponding closely with the amount of Fe_3C present under these conditions. By quenching very rapidly from about 1200° C. the magnetism

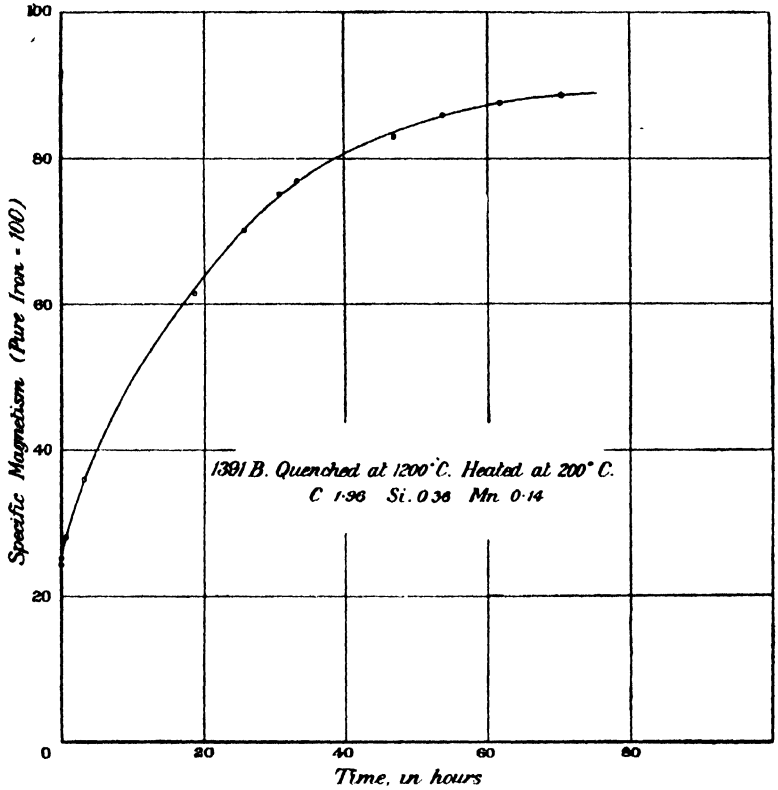


Fig. 9.

is greatly reduced. The amount of reduction varies much in different specimens, depending apparently upon the precise rate of cooling. In one instance the magnetism fell as low as 24 per cent. In another, in which the treatment was nominally the same, but the quenching apparently in reality less effective, the magnetism was reduced to 60 per cent. By reheating the quenched steel to 200° C. the magnetism is gradually restored, as shown in the curve (Fig. 9). The change is practically complete after seventy hours, when the magnetism has been restored to its original value of nearly 90 per cent. There is, however, a perceptible change in ten minutes. Heating to 100° C. for one hour produced no perceptible increase in magnetism.

The magnetic changes in the carbon steel are accompanied by well-marked changes in constitution, as revealed by the microscope. Fig. 10, Plate IV, shows these changes. The specimens were polished, and each was etched for two and a half minutes in a 1 per cent. solution of HCl in alcohol. *A* is the quenched steel with 24 per cent. of magnetism. *B* is the same steel after heating for seventy-five hours to 200° C.—magnetism, 90 per cent. It will be observed that in *A* the greater part of the material is austenite, which is left light by the etching reagent. In *B* practically the whole of this has been converted to dark etching martensite, which is magnetic. *C* is an imperfectly quenched steel, not reheated, in which the magnetism is 60 per cent. This is intermediate in constitution between *A* and *B*, about half of the ground mass consisting of dark etching martensite, the other half of austenite. The correspondence between the proportion of austenite and the reduction in magnetism seems to be complete.

Reference may be made in this connection to the steel containing carbon, 1·94 per cent.; silicon, 0·94 per cent.; and manganese, 2·20 per cent., prepared by one of the authors. Maurer found that if quenched from 1050° C. in iced water this steel is rendered non-magnetic. It is intermediate in composition, and properties between the nearly pure carbon steel referred to in the last two paragraphs and the steel containing 12 per cent. of manganese. In all three cases the magnetic form is the stable form at ordinary temperatures, and the non-magnetic at temperatures above the change point. Probably also in all these cases the change on slow cooling may be described as analogous to the precipitation of crystals from a solution, while the effect of sufficiently rapid cooling is like the formation, from the same solution, of a jelly or glass which is for practical purposes stable at low temperatures, but becomes crystalline if heated for a sufficiently long time to a temperature which may be considerably below the melting point. The principal effect of adding manganese is to make the latter change take place with greater difficulty. It probably also has some effect upon the position of the change point (the analogue of the melting point), but not so great an effect as has been supposed hitherto, since even with 12 per cent. manganese the change point is certainly above 650° C.

The experimental work on which this paper is based extended over about three years, and was carried out in detail by our assistants at Sheffield and at Cambridge. We desire especially to place on record our sense of the value of the work done by Messrs Milne and Main, who did the mechanical testing and the heat treatment at Sheffield, and by Mr H. Quinney, who carried out at Cambridge all the magnetic tests.

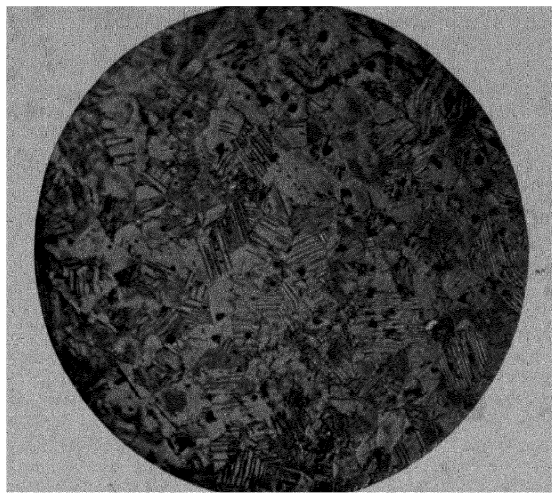
Sir Robert Hadfield, F.R.S., Past President, in opening the discussion, said that he had with him some of the specimens which were referred to, which the members might care to inspect. It would be seen that they were very clearly magnetic, showing that the quality of non-magnetic manganese steel had been

HADFIELD'S MANGANESE STEEL.

Carbon, 1·23 per cent. Manganese, 12·64 per cent.

Original, water-toughened from 1050° C. Non-magnetic.

Etched 4 per cent. picric acid solution.



Magnified 100 diameters and slightly reduced.



Magnified 600 diameters and slightly reduced.

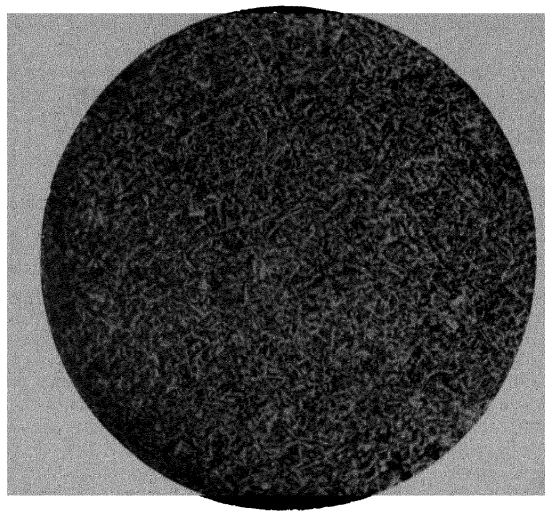
Fig. 3.

HADFIELD'S MANGANESE STEEL.

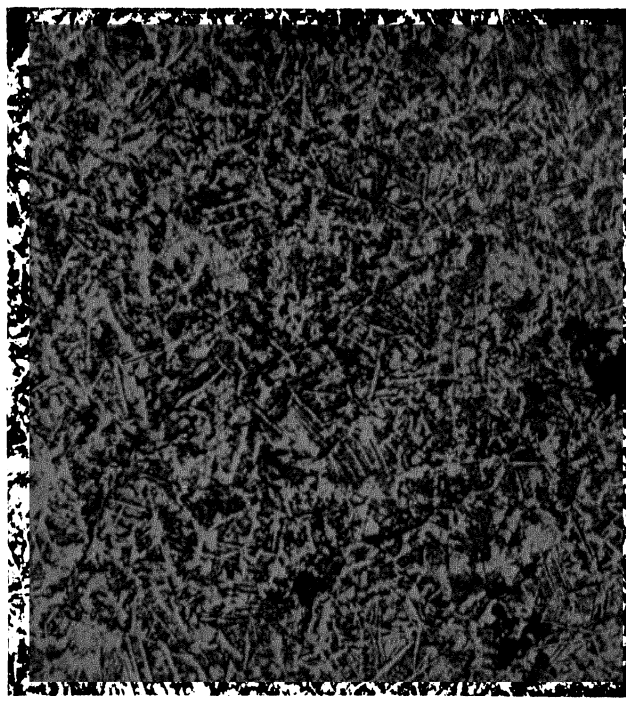
Carbon, 1·23 per cent. **Manganese, 12·64 per cent.**

Reheated at 520° C. for 12 hours. 30 per cent. magnetism.

Etched 4 per cent. picric acid solution.



Magnified 100 diameters and slightly reduced.



Magnified 600 diameters and slightly reduced.

Fig. 4.

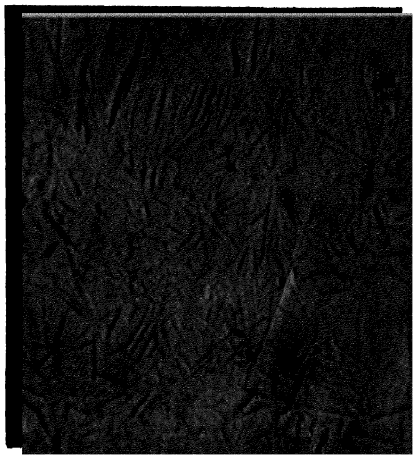


Fig. 5.

Heated 6 hours at 400° C.
Sp. mag., 0·2 per cent.
Angle of bend, 12°.
Hardness no. 340.

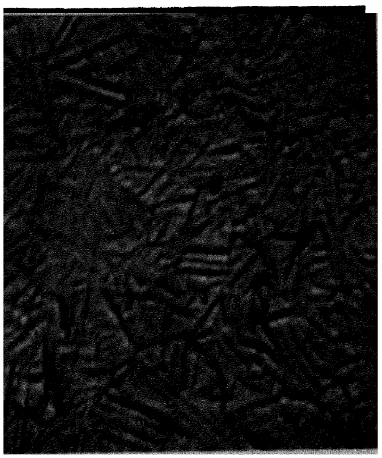


Fig. 6.

Heated 6 hours at 450° C.
Sp. mag., 0·8 per cent.
Angle of bend, 10°.
Hardness no. 321.

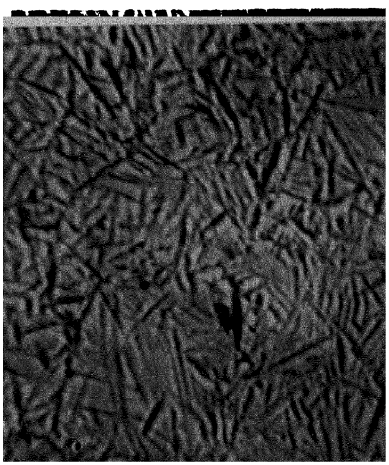


Fig. 7.

Heated 18 hours at 400° C.
Sp. mag., 2 per cent.
Angle of bend, 2°.
Hardness no. 387.

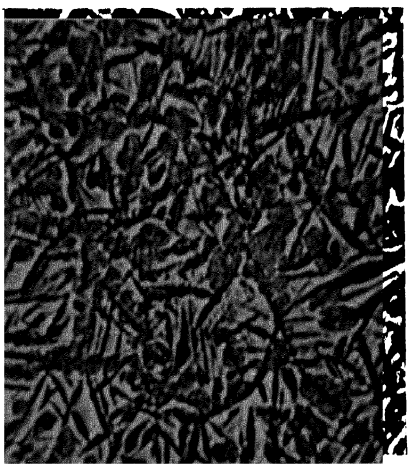
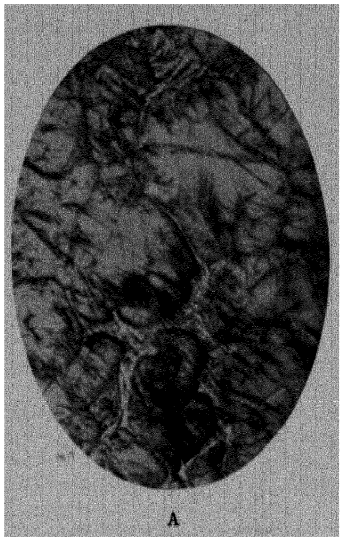
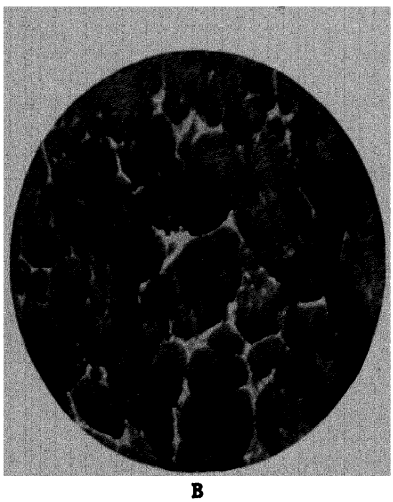


Fig. 8.

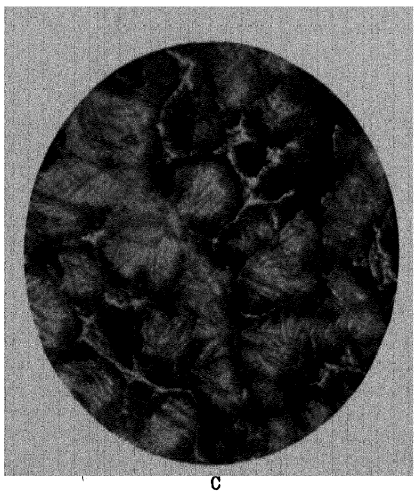
Heated 15 hours at 530° C.
Sp. mag., 23 per cent.
Angle of bend, 3°.
Hardness no. 387.



A—Quenched from 1200° C.
Magnetism, 24 per cent.



B—Quenched from 1200° C.
Reheated for 75 hours at 200° C.
Magnetism, 90 per cent.



C—Quenched from 1200° C.
Magnetism, 60 per cent.

Fig. 10. 1391 B.

Carbon, 1·96 per cent. Silicon, 0·36 per cent. Manganese, 0·14 per cent.
Magnified 200 diameters and slightly reduced in reproduction.
Etched 2½ per cent. solution of HCl.

entirely changed. There was one other fact to which he desired to refer. The research probably did not bear only on manganese steel, but on the complex problem, which so many of the members had been studying for the last twenty years, of what happened when a piece of carbon steel was hardened. Specimens of manganese steel in the various stages described in the paper had been taken and the carbide carbon determined. The total carbon present in that particular manganese steel was 1·21 per cent., of which carbide carbon was 0·16 per cent. On treating it at 650° C. for six hours, the material possessed a specific magnetism of 0·2 per cent., the carbide carbon was increased to 0·55 per cent., and the hardenite or hardening carbon was 0·66 per cent., a very considerable difference compared with the 0·16 per cent. carbide carbon present originally. If the specimen was heated to 500° C. for forty-eight hours the carbide carbon remained practically the same, 0·53 per cent., and the specific magnetism was 40 per cent. Toughened manganese steel, that was, the same material which had been passed through the same treatment and then reheated and toughened, went back again to practically nil, as regards specific magnetisation, the carbide carbon going back to 0·16 per cent. That was a remarkable proof that the form of carbon could be changed, and many varieties of toughness and hardness obtained, apparently to a great extent independently of the particular form of carbide present. The hardness of the magnetic specimens ran as high in some specimens as 438 ball, while in the toughened material it went down to about 200 ball. The members would appreciate how extraordinary those anomalies were. It showed how much research work still remained to be done before a satisfactory explanation could be arrived at with regard to those very interesting specimens. In order to make the point quite clear the table on page 186 was given. It was believed that in the near future those facts would prove of great value with regard to this question of the forms of carbon in different steels and their effect upon physical qualities whether as regards toughness or ductility or hardness, including the various grades of glass-scratching or flint hardness.

The table showed that the carbon in the residue might remain about 0·53 per cent. and yet the material might be practically non-magnetic, as in specimen No. A. 5303 F, or as in specimen No. A. 5337 F, it might show no less than 40 per cent. specific magnetism. It also showed that the fracture of the 1010 test-bar not strained and possessing a ball hardness of 215 contained 0·13 per cent. of carbon in residue, and yet in the strained condition, where the ball hardness increased to 490, the carbon in the residue remained practically the same. In other words, notwithstanding the extraordinary change in physical qualities, the change in carbon only took place when the specimens were submitted to heat treatment. That was shown more clearly by the table on page 187.

Those figures suggested that the change in carbon did not appear to play an important part in the change in the quality of the material from non-magnetic to magnetic condition. As would be noticed in the results given, there were two non-magnetic specimens, in each of which the carbide varied greatly, and two specimens in which the carbide carbon was the same, and yet their magnetic properties varied considerably.

He asked all those who were studying the problem to bear in mind that manganese steel, although it could be made hard, never became hard like hard carbon steel. The very highest ball hardness they had been able to obtain was not more than a maximum of 550, and, as the members knew, glass-scratching hardness could not be obtained until a Brinell ball hardness number of at least 600 was reached. The difference therefore was considerable. If once it was ascertained what was the cause and why the hardness of manganese steel could

Colour Tests on 12½ per cent. Manganese Steel containing 1·20 per cent. Carbon, in different conditions.

Test-bar No.	Chemical tests—treatment with HNO_3 (specific gravity 1·2)				Carbon in residue after solution in HCl (specific gravity 1·02)	Heat treatment	Physical tests		
	Attack in cold		Boiled up and dissolved, colours compared with 4306 as standard				Bending test	Ball test	Specific magnetism
	Residue	Solution							
			Per cent.	Difference in carbon per cent.	Per cent.				Per cent.
A. 4306 F ...	Kept shape of drillings	Light clear	1·20	Nil	0·16	Water-toughened	180° Unbroken	207	Nil
Fracture of 1010 test-bar	Kept shape of drillings	Light clear	1·18	−0·02	0·12	Water-toughened	...	490	...
Grip end of 1010 test-bar	Kept shape of drillings	Light clear	1·20	Nil	0·13	Water-toughened	...	215	...
A. 5873 F ...	Kept shape of drillings	Light clear	1·15	−0·05	0·17	Water-toughened 300°C. for 23 days	...	217	1·25
A. 5374 F ...	Amorphous	Dark	1·27	+0·07	0·52	Water-toughened 450°C. for 48 hours	...	418	7·00
A. 5337 F ...	Coarse amorphous	Dark	1·24	+0·04	0·53	Water-toughened 500°C. for 48 hours	1½° Broken	438	40·2 41·0
A. 5327 F ...	Fine amorphous	Dark, very turbid	1·64	+0·44	0·64	Water-toughened 600°C. for 48 hours	...	418	31·0 30·0
A. 5303 F ...	Amorphous	Dark, rather turbid	1·50	+0·30	0·55	Water-toughened 650°C. for 6 hours	2½° Broken	418	0·2
A. 5298 F ...	Fine amorphous	Dark, turbid	1·52	+0·32	0·56	Water-toughened 650°C. for 12 hours	...	337	0·2

not be increased beyond about 550, that might help to solve many of the problems relating to other steels. The paper was not presented with the idea of specially calling attention to the particular properties of manganese steel, but because some of the facts met with in the research seemed to bear a still wider and more important problem. He was glad to be able to add those figures relating to carbide carbon, because he believed it was the first time they had been placed before any institution, and they represented new and original data.

Carbide Carbon in Manganese Steel.

Mark	Specimen	Specific magnetism per cent.	Carbide carbon per cent.	Difference (hardenite or Ledebur's hardening carbon) per cent.	Total carbon per cent.	Brinell ball hardness number
A	Manganese steel heat-treated	0·2	0·55 0·56	0·66 0·65	1·21	418
B	Manganese steel heat-treated	40·2	0·53 0·55	0·68 0·66	1·21	438
C	Manganese steel toughened in the ordinary manner	Nil	0·16 0·17	1·05 1·04	1·21	200
D	Exactly same bar as C after being broken in the tensile machine	0·30	0·12	1·09	1·21	490

Professor B. Hopkinson, in reply to the discussion, said that Professor Le Chatelier's remarks were of great interest, and he would deal with them in writing if necessary. He had not much to say in reference to the other remarks that had been made on the paper. He was very much interested in what Dr Rosenhain said about his experiments on the material, and about the heat evolutions or absorptions which he had observed at lower temperatures. In one instance mentioned in the paper a considerable heat change must have occurred at a very low temperature, namely, that of liquid air or some temperature of that order, in the case where by putting a partially magnetic piece of manganese steel into liquid air it was made more magnetic. If, as he thought there was good reason to believe, any change of magnetism was accompanied by a proportional absorption or evolution of heat, then such an absorption or evolution must have taken place in that case. The point of the remark was that in investigating the absorptions or evolutions of heat it was necessary to bear in mind that the material under observation was either completely magnetic or completely non-magnetic. Dr Stead had referred to the rather positive statement made on page 182 as regards the microscopic appearances that "these represent plates of hard material which is forming in the mass and whose edges are left upstanding by the polishing." He would take counsel with his colleague about that statement and one or two other points of the same kind, and it was possible that in the paper in its final form the authors would find it desirable to make their language rather less positive[1]. Nevertheless he thought that the statement in question was at least fully consistent with the observed facts, and he believed Dr Stead himself

[1] This has now been done.

explained the same appearance in the same way when he first noticed it about twenty years ago. One point of considerable interest was raised by more than one of the speakers, namely the effect of overstraining upon manganese steel. As far as his experience went the effect of breaking a bar was invariably to make it very slightly magnetic, perhaps of the order of $\frac{1}{2}$ per cent. that of pure iron. It also made it hard, in fact it produced the same kind of change in both respects as was obtained by heating it for a long period to, say, 300° or 350° C. He desired to deal with one other point, namely the relation between the amount of magnetism present in the test-piece which was used for taking a heating curve and the absorption of heat. He did not carry out those tests himself, but he thought he was right in saying that they were all carried out with material which had been made as magnetic as it could be made, half as magnetic as pure iron. It would be rather difficult by that method to arrive at a quantitative relation between the amount of magnetism and the amount of heat absorbed, although it might be worth trying to get it. He thanked the members for the way in which the paper had been received.

Sir Robert Hadfield said he desired to add a few words to the reply of his co-author. Dr Hatfield raised a very interesting question in regard to the separation of the carbon. The carbon had been determined in a strained bar of manganese steel which in its toughened condition was practically non-magnetic, and showed a tensile strength of about 60 tons and about 50 per cent. elongation. Singularly enough the hardness of that particular specimen rose to 550 ball hardness, almost approaching, but not actually, glass scratching-hardness, and yet the carbide carbon remained just the same as in the original toughened condition, about 16 per cent. That was a remarkable fact, and he was glad that Dr Hatfield's question enabled him to refer to the point again. He wished to add one other remark. Some people might say, as Dr Hatfield had suggested, that there might be some difference produced by grinding. Dr Hatfield could quite disabuse his mind that any change was thus produced. The specimen would be absolutely the same in its magnetic or non-magnetic qualities no matter whether ground or not ground at all. The magnetic qualities were therefore independent of any grinding. There was no question whatever about that. He gave a paper on the same subject in America about a couple of months ago, and one of the members present at one of the discussions said: Was it possible that the material had been made magnetic because it had been decarburised? He wished to say most positively that there was no decarburisation – when he said no decarburisation, if the total carbon was 1·2 per cent. there would not be decarburisation of 0·05 per cent., which was practically nil. There was, of course, surface decarburisation, but that did not extend more than immediately beyond the surface skins. Therefore the members might be quite certain that all the extraordinary magnetic changes referred to in the paper were definite physical changes. They had nothing to do with grinding or decarburisation, but represented the actual change throughout the mass. Possibly if a large mass of manganese steel were produced many inches in thickness those extraordinary physical changes would be found present in the same manner. The experiment showed, therefore, that the results obtained represented an absolutely new and novel condition in manganese steel. In conclusion, he was very much obliged to the members for their kind attention.

Professor Hopkinson, in further reply, wrote that the observations made by Sir Robert Hadfield on the distribution of the carbon as between carbide and hardening carbon were a most important addition to the paper. The property

of brittleness seemed to be more closely correlated with the state of the carbon than with any other factor which the authors had observed. The precise significance of those analyses from a chemical point of view was not to his mind perfectly clear; it seemed to him possible that the carbon dissolved out by the HCl solution was really present in the steel as carbide just as much as that which remained in the residue, and that its different behaviour under the action of this reagent might be due to the size of the aggregates of carbide, or something of that kind. In other words, the changes in mechanical properties produced by heat treatment might be due (at any rate when unaccompanied by any substantial magnetic change) to changes in the distribution of the various constituents of the steel rather than to any difference in the chemical nature of these constituents. He did not say that that was the fact, but he put it forward as a possibility which was consistent with all the observations, and in particular with the absence of magnetic change, which to his mind was difficult to explain if there was really a change in the chemical relations of the carbon and the iron. The precise interpretation of the chemical observations did not, however, matter very much. The important point was the existence of a definite property—namely, the behaviour of the steel to certain chemical reagents which seemed to be in very close relation with its mechanical properties. As the paper showed the brittleness was also related in some way to the magnetism; it appeared that steel which was perceptibly magnetic was always brittle. But the converse did not hold; a piece might be non-magnetic and yet brittle. The correspondence between the chemical properties of the steel and its toughness was still closer; it seemed from Sir Robert Hadfield's observations that tough material always contained a relatively small proportion of what, without prejudging the chemical question to which he had alluded, he would (following Sir Robert Hadfield) call "carbide carbon." But it was worth while to point out that even here the correspondence was not absolute. Cold work, such as breaking the piece in a testing machine, did not increase the proportion of "carbide carbon" in the previously toughened steel, but it destroyed the toughness, and made it hard, at the same time rendering it slightly (but only slightly) magnetic.

14

THE CALORIMETRY OF EXHAUST GASES.

CALORIMETRY OF THE GASES EXHAUSTED FROM AN INTERNAL COMBUSTION ENGINE.

[Abstract of paper read before Section G of the British Association at Cambridge, August 19, 1904: reprinted from "ENGINEERING," August 26th, 1904.]

So far as I am aware, in all gas engine tests hitherto made, the indicated work, the heat in the cooling water, and the calorific value of the gas used, are the only quantities which have been directly measured. The balance of heat unaccounted for has been put down to exhaust gases, radiation, and conduction. In some cases an attempt has been made to separate the last mentioned items by estimation, but these attempts have been very rough in character, and have led to very divergent results.

The special point of the tests to be described is that the exhaust gases were passed through a calorimeter, and so cooled to near atmospheric temperature, the heat rejected by them being measured. In this way a complete heat account has been obtained, in which the only item not directly determined is the loss by conduction and radiation.

The first set of tests were made on a Crossley engine in the Engineering Laboratory, Cambridge, giving about 5 horse-power on the brake. This engine is several years old, and its performance must not, therefore, be taken as representative of the more modern engines made by the same firm. The following are the particulars of the engine:

Speed, 250 revolutions per minute.
Cylinder diameter, 7½ in.; stroke, 9 in.
Compression space, 143 cubic inches.
Ignition, hot-tube with timing-valve.
Cylinder only is water-jacketed, piston and exhaust-valve being uncooled.

Cambridge coal-gas was used, having an average calorific value of 680 British thermal units at 0 deg. Cent., and 760 millimetres, of which about 70 British thermal units are the heat evolved in condensing and cooling the steam produced in the explosion.

The calorimeter for the exhaust gases is shown in Fig. 1, from which its construction will sufficiently appear. It consists of a section of flanged cast-iron pipe fitted with baffle-plates, by which the gases are caused to pass several times backwards and forwards through its length, and finally to bubble through the water collected at the bottom. The water, after passing through the jacket surrounding the calorimeter, meets the exhaust gases in a fine jet, and is partly caught by them and carried round with them, and partly trickles down the baffle-plates. It was found that in this way the gases could be cooled, with the engine fully loaded, down to 120° Fahr. or less, the cooling water rising in temperature from about 60° to 90°. This instrument was designed of ample proportions, so that it should be certain to work satisfactorily; there can be no doubt that it

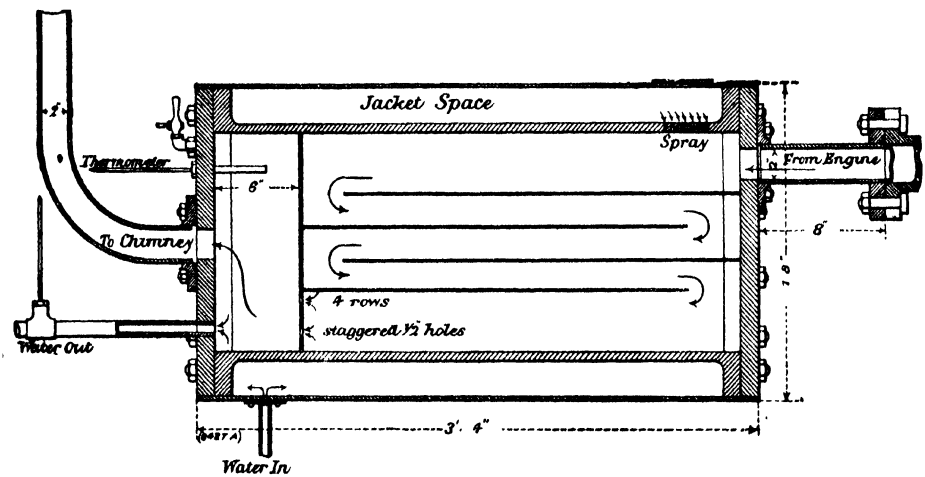


Fig. 1

is big enough to serve for a much larger engine. In a test on an engine of double the power—to be described later—the calorimeter was very much smaller and the results were quite satisfactory.

There is no need to particularize the methods of indicating the engine, or of measuring the amount and temperature rise of the jacket-water, about which there was nothing peculiar. Some pains, however, were taken to secure accurate measurement of the heat put into the engine. The gas was measured with a wet and dry meter in series, and both meters were calibrated with a standard holder at the rate of flow of gas actually used. The calorific value of the gas was determined with a Junker calorimeter at the time of the test on the engine. The amounts of heat stated as having been supplied to the engine are probably correct to within 2 per cent.

The following are the results obtained:

Table I.

No. of trial	Heat value of gas, B.TH.U. per explosion.	Indicated work, B.TH.U. per explosion		Heat to jacket-water	Heat to exhaust calorimeter	Heat up chimney	Balance unaccounted for		
1	13·3	2·5	= 19 p.c.	5·5	4·0	...	1·3	= 10 p.c.	
2	18·5	3·4	= 18½	8·3	5·2	...	1·6	= 8½	
3	16·0	2·9	= 18	6·4	5·2	0·4	1·1	= 7	Late ignition
4	13·0	2·7	= 21	5·1	4·1	0·1	1·0	= 8	
5	12·6	2·5	= 20	4·2	4·6	0·1	1·2	= 9½	
6	15·1	2·9	= 19	5·9	4·2	0·2	1·9	= 12½	
7	15·0	2·8	= 18½	6·4	4·0	0·1	1·7	= 11½	
8	16·1	2·7	= 17	6·4	4·3	0·1	2·6	= 16	Late ignition
9	15·2	2·65	= 17½	5·7	4·2	0·4	2·2	= 14½	Late ignition

No. of trial	Explosions per minute	Release pressure, pounds per square inch absolute	Energy of gases at release, B.TH.U. per explosion	Release temperature, Cent.	Liner temperature, Fahr.	Temperature of jacket-water (exit), Fahr.	Temperature of exhaust gases after passing calorimeter, Fahr.	
1	32	49	6·4	780	102	72	80	
2	23	53	7·5	870	106	72	80	
3	32	56	7·7	930	86	70	100	Late ignition
4	110	46	6·0	900	122	80	105	
5	117	43½	5·5	800	156	200	105	
6	116	45	6·0	880	126	92	118	
7	120	45	6·0	880	126	81	112	
8	91	53	7·2	1250	103	75	107	Late ignition
9	111	52	7·0	1200	100	82	144	Late ignition

All the figures are stated in British thermal units per explosion, and the first four columns need no further explanation.

The fifth column is an estimate of the energy still left in the exhaust gases after passing the calorimeter in those cases in which it is not negligible. It is based on the assumption that the volume of gas discharged per cycle is that of the engine stroke, corrected for the suction pressure (observed) and the suction temperature (assumed), and that these gases are saturated with water vapour. Their temperature is observed, and thence can be calculated the heat which they would evolve (by cooling and condensation of steam) on cooling to 80° Fahr., which is about the temperature at which the gases leave the Junker calorimeter. This estimate is a very rough one, as the volume may be in error by 10 per cent. in

certain cases, and the gases may not be fully saturated; but the item is a very small one. The sixth column shows the balance unaccounted for, and consists of the radiation and conduction losses, and the sum of the errors in the first five columns.

In the first three trials, as appears from the seventh column, there was no load on the engine, and practically all the explosions were preceded by one or more scavenging strokes. In the other trials, the engine was nearly fully loaded, so that very few of the explosions were preceded by a scavenging stroke. The release pressure in the eighth column is the pressure at a point one-tenth of the stroke short of the out-centre at or about which point the exhaust-valve begins to open. The energy of the gases at release is calculated from the release pressure on the assumption that we have then perfect gas mixed with the steam produced by the explosion, the amount of which is known from the gas analysis. The heat of condensation and cooling of this steam is added to the heat in the gas considered as perfect gas. The actual energy is probably more than this, since the steam and CO_2 are almost certainly somewhat dissociated at the temperature of release. In the tenth column are the temperatures of release. These are calculated from the suction temperature (assumed) and release pressures. In the case of the light-load trials the suction temperature cannot be far different from the temperature of the gas before admission, and I have taken a round figure of 27° C. or 300° absolute. In the full-load trials the suction temperatures are very uncertain; I have drawn on Professor Burstall's measurements for these, and have assumed a round figure of 77° C. (350° absolute) for Nos. 4, 5, 6, and 7, and 127° C. (400° absolute) for the others in which the release temperatures are higher. It will be seen that the temperature measurements are extremely rough, but I have only based conclusions on their relative order of magnitude in the several trials, and that is, no doubt, correctly shown. The "liner temperature" is that of the outer end of the cylinder liner where it projects in front of the cylinder. Since this portion is not water-jacketed, is not exposed to the flame, and is in all trials except No. 5 hotter than the jacket-water, it must receive its heat entirely from the piston, which comes into contact with it on the out-centre. Its temperature is, in the case of the full-load trials, a rough indication of the heat being given to the piston. In trials Nos. 3, 8, and 9 the ignition was retarded, so that it commenced about one thirtieth of the backward stroke after the in-centre. In the other trials the ignition was normal and commenced slightly before the in-centre.

The following points are worthy of note:

1. The gases lose much heat between release and entering the calorimeter. The following table shows the difference between the estimated energy at release and the energy given to the calorimeter, and sent up the exhaust-pipe. The actual difference is greater, since, as already explained, the energy at release is certainly under-estimated.

Table II.

Number of trial ...	1	2	3	4	5	6	7	8	9
Release temperature ...	780	870	930	900	800	880	880	1250	1200
Temperature of passages	cold	cold	cold	hot	hot	hot	hot	hot	hot
Loss in passages ...	2·4	2·3	2·1	1·8	0·8	1·6	1·9	2·8	2·4

The gases lose heat after release, partly in the exhaust-valve and passages, and partly to the cylinder walls in the exhaust-stroke of the piston. The latter part of the loss goes to swell the jacket losses; the former is largely radiated away from the hot exhaust-valve. We should expect the amount of loss to depend mainly on the difference of temperature between the gases and the passages or cylinder walls. Thus, high-release temperatures should mean much loss of heat, and in the light-load trials, where the passages are fairly cool, the loss should be greater than in the full-load trials, where they are kept hot. These conclusions are confirmed by the last table.

The smallest losses are in trials 4, 5, 6, and 7, where the release temperature is low, and the passages hot. Then come Nos. 1, 2, and 3, where, though the release temperature averages about the same as in 4, 5, 6, and 7, the passages are cold and the losses undoubtedly higher. Finally, we get the largest losses of all in Nos. 8 and 9.

2. The balance unaccounted for (column 6 of first table) seems to consist mainly of radiation from the exhaust-valve. The biggest balances are those in trials Nos. 8 and 9, where, by reason of the late ignition, the release temperature is high. The exhaust-valve in these trials was observed to be very hot. It will be noticed that in these cases the temperature of the liner, and therefore of the piston, was lower than in the other full-load trials; and the temperature of the jacket-water was by no means excessive. In No. 5, although the jacket-water was nearly boiling, the balance is low, because here the exhaust-valve was fairly cool.

3. There is no marked improvement in efficiency caused by scavenging.

4. The piston is cooler with the late ignition. Compare the liner temperature of Nos. 8 and 9 with the other full-load trials Nos. 4, 6, and 7.

One object of these trials was to ascertain whether a practical method of testing gas engines for efficiency could be based on the calorimetry of the exhaust gases. At the present time manufacturers are mainly concerned with making their engines reliable; the thermal efficiency, being with any design far and away better than in the steam engine, is a secondary consideration. But it cannot be doubted that in the future gas engines will have to be as closely tested for efficiency as steam engines now are. Now to measure the heat put into a large engine worked with producer gas is a very difficult and costly undertaking. The volume of gas is very great, and

its quality may vary largely in the course of a day's trial. It seems to me that it may be easier and more accurate to measure the indicated work and the heat rejected, as is usually done in testing a steam engine. One large part of the rejected heat—that is, the heat in the jacket-water—presents no difficulty; we have therefore only to measure the heat carried away in the exhaust gases.

The only question is whether the balance of heat unaccounted for, which cannot be measured, is too large a percentage of the total heat.

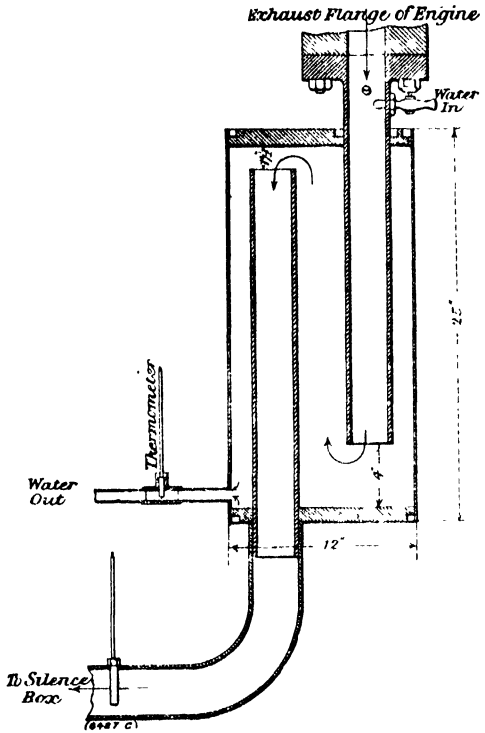


Fig. 2.

From this point of view the tests made on the small Crossley engine are not very satisfactory, the heat unaccounted for in the full-load tests ranging from 8 to 16 per cent. of the total. Moreover, in those tests the calorimeter was very much more bulky than was necessary for the purpose. It appeared to me from consideration of the results that the lost balance of heat must have gone mainly in radiation from the exhaust-valve and other uncooled parts of the engine. I therefore made a similar trial on a second and larger engine in the laboratory, in which, as it had a water-cooled exhaust-valve, it appeared probable that the radiation loss would be substantially less.

This engine is by the Forward Engineering Company, and gives about 10 horse-power on the brake. The following are the principal particulars of it:

Speed, 250 revolutions per minute.
Cylinder diameter, 7 inches.
Stroke, 15 inches.
Compression space, 140 cubic inches.
Ignition, hot-tube with timing-valve.
Cylinder and exhaust-valve are water-jacketed, piston being uncooled.
This engine compresses to 115 lbs. absolute against 70 lbs. in the Crossley.

In the case of this trial the exhaust-gas calorimeter was made very much smaller. Fig. 2 sufficiently shows its construction. The water is sprayed through two flat-flame gas-burners into the exhaust-pipe, so that the exhaust gases immediately on leaving the engine encounter a sheet of cold water, and they are further cooled by churning up the water at the bottom of the calorimeter.

The gases, after leaving the calorimeter, were taken into the ordinary exhaust-box of the engine, which was provided with a drain and acted as a water-separator. It was found, however, that very little water was carried over from the calorimeter by the gases. With this apparatus the gases were cooled down to a temperature of 120° Fahr. or less, the engine being fully loaded, and the apparatus itself occupied but very little more space than the bit of exhaust-pipe which it replaced. The following are the results of a trial made on this engine.

Table III.

	B.Th.U. per explosion	per cent.
Jacket-water	7·0	32
Exhaust calorimeter	7·5	34·5
Heat up chimney (estimated) ...	0·3	1·5
Indicated work	5·7	26
Balance unaccounted for	1·3	6
Heating value of gas used	21·8	100·0

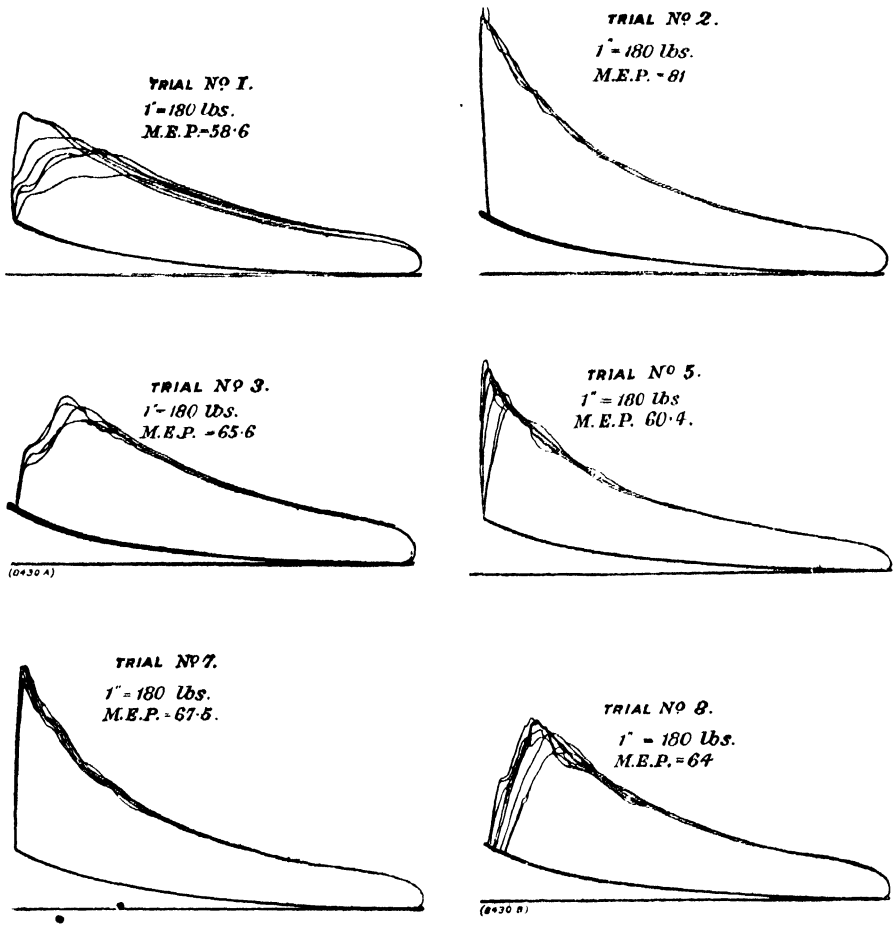
Temperature of exhaust gases after passing calorimeter, 107° Fahr.

Temperature of jacket-water at exit = 105° Fahr. Explosions per minute, 106. Indicated horse-power = 14·2.

The gas measurement in this trial is correct to within 2 per cent. The indicated power is correct to within 5 per cent., and the other measurements to within 2 per cent.

It will be seen that the heat lost by radiation and conduction certainly does not exceed 10 per cent. of the heat put into the engine, and is probably a good deal less. I have made a large number of trials of the same kind on this engine, in all of which the balance unaccounted for is less than 6 per cent. even when the jacket-water was very hot. This seems to me a sufficiently good result to justify trying this method of testing engines on a larger scale, especially having regard to the very compact character of the

exhaust-gas calorimeter. In ordinary steam engine trials it requires extraordinary precautions to get a closer agreement than this between the heat put in and the heat rejected, *plus* indicated work. Moreover, I think it is reasonable to suppose that in a large engine with a water-cooled piston the loss unaccounted for would be smaller still. Of course, the precise design of the calorimeter for the exhaust gases would have to depend on



the arrangement of engine and piping in each case; but there is no doubt, I think, that the secret of success is to spray a sheet of water under high pressure into the exhaust-pipe close up to the engine. The gases rushing out break up this sheet into minute drops, and are thus very effectually cooled. The only difficulty then is to separate the suspended water from the gas, and to collect and measure its amount and rise in temperature. The separation is by no means so difficult as I had anticipated, and could no doubt be effected in a box somewhat similar to that which I have shown in Fig. 2. It will be found, on calculating out the heat contained in the gas

after leaving the calorimeter, that this item does not become seriously high so long as the temperature of the gases is below about 140° Fahr. I find, however, that the radiation and conduction from this particular calorimeter become rather large at temperatures above 115° Fahr. But it is easy to determine the amount of this by a separate experiment.

I append some typical indicator diagrams, sufficient to show the character of the explosion in each case. The whole of the experiments were carried out at the Engineering Laboratory, Cambridge, by my pupils—Messrs G. Harrison and A. Blackie, of Peterhouse. They also reduced most of the observations, and I wish to express my indebtedness to them.

15

EFFICIENCY TESTS ON A HIGH-SPEED PETROL MOTOR.

[Reprinted from "ENGINEERING," February 8th, 1907.]

So far as I have been able to ascertain, no accurate measurements of thermal efficiency have been made upon the high-speed internal-combustion motors used for motor-cars and similar purposes. The fuel consumption in such motors has not hitherto been regarded as of great practical importance, but the aim of designers has been, broadly speaking, to produce engines which shall develop the greatest possible power for a given size, without much regard to the fuel used. Moreover, these engines can only be indicated with rather special appliances, and, consequently, the only efficiency tests which can be taken without much trouble are those based upon brake-power, which, though they are no doubt of much practical interest, are not of great value from the scientific point of view. It, therefore, seemed worth while to carry out on a high-speed petrol-motor a series of tests for efficiency comparable in accuracy with those which have been made upon the ordinary slow-speed gas-engine. The experiments here described are the first portion of such a series which I have lately been engaged upon in conjunction with one of my students, Mr H. R. Ricardo, of Trinity College, Cambridge. Mr Ricardo, I should say at the outset, has done practically the whole of the experimental part of the work, and several points in the arrangements for testing are due to him. The engine on which the experiments were made is one of the Daimler Company's four-cylinder motor-car engines, rated to give 16 to 20 horse-power. It is capable of being worked at any speed between 250 and 1400 revolutions per minute. It will be seen, therefore, that its speed of revolution is of a different order of magnitude from that of an ordinary stationary gas-engine, and it can, moreover, be varied over a very wide range, so that the tests may be expected to throw some light upon the effect of speed on efficiency. The engine has been kindly lent by the Daimler Company for experimental purposes. The following are particulars of the engine:

Total volume of one cylinder with piston on out-centre	0·04 cub. ft.
Volume of compression space	0·0104 „ „
Compression ratio	3·85
Diameter of cylinder	3·56 in.
Length of stroke	5·11 „

The engine is fitted with the standard Daimler carburettor, which is so arranged that a portion of the air supply passes over the petrol-jet, and after doing so meets the main air supply and mixes thoroughly with it. That portion which passes over the jet is usually taken past the hot exhaust-pipe or otherwise heated. The power of the engine is varied by throttling the mixture after it has passed the carburettor or by altering the timing of the spark. The petrol was supplied to the carburettor by gravity under a head of about 2 ft.

The only difficulty about the measurement of efficiency is the determination of the indicated power. At speeds such as are here used the ordinary pencil type of indicator is, of course, quite unsuitable, owing to the inertia of the parts. Optical indicators of the diaphragm type, originally due to Perry, of which the best-known representative is the Hospitallier-Carpentier manograph, are in many ways convenient, but they suffer from the disadvantage for quantitative work that the scale of pressures is not proportionate, and the diagrams cannot therefore be measured up in the ordinary way. I have therefore designed a new type of indicator which is free from this defect, and which gives correct diagrams at high speeds. It is not necessary, for the purposes of this paper, to give a full description of the indicator; but it may be said that it is of the piston type, the piston being forced against the middle point of a piece of straight steel spring held at both ends, the deflection of which turns a mirror, and so moves a spot of light. In addition to the proportional scale the instrument has the further advantage that it is very easily calibrated by deadweights. In order to avoid the trouble of a great deal of photography I have devised an arrangement for observing the diagrams, by which they are projected on to a transparent screen, which is graduated vertically and horizontally in millimetres. The diagram can then be read off and plotted on a piece of squared paper while the engine is running. Most of the diagrams were taken in this way, but I give on Plate I some photographs to show the kind of diagram thus given.

Two methods of obtaining the indicated power were available. Of these, the first—that is, to indicate the engine when running fully loaded—involves a great deal of complication, since there are four cylinders which have to be indicated simultaneously. I therefore determined to adopt the second method—namely, to determine the brake power, and to make a separate measurement of the mechanical losses in the engine, which, when added to the brake power, give the indicated power. This only involves indicating one cylinder, and is certainly at least as accurate as measuring the indicated power directly. The tests here described, therefore, involved three sets of measurements—that is, engine losses, brake power, and fuel consumption.

Mechanical Losses. For determining the mechanical losses the engine

was driven without load by one cylinder only, and the indicated power of that cylinder was measured at various speeds.

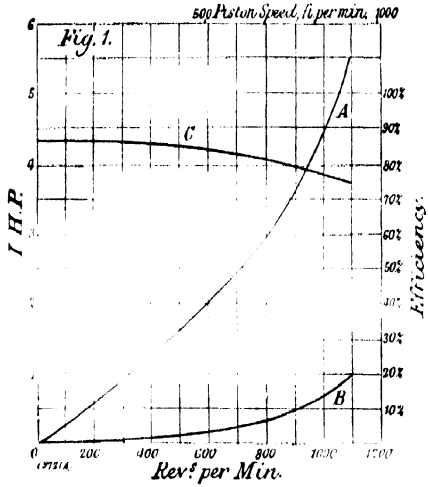
For the purpose of these experiments I take "indicated power" to mean the positive loop of the indicator diagram, and I regard the negative loop representing the power absorbed in suction and exhaust as part of the engine losses. When running with one cylinder in the manner described, the indicated power of that cylinder will be equal to the mechanical friction of the engine plus the negative work shown on the diagrams of all the four cylinders. When the engine is running fully loaded at the same speed the loss by mechanical friction will be substantially unaltered, but the suction losses will not be the same; for the negative loop indicated by a cylinder when firing is smaller than when it is not firing. Moreover, in the light-load tests with one cylinder, the three cylinders which are idly compressing and expanding air will indicate a certain amount of negative work, because of the loss of heat in compression, which causes the expansion curve to fall somewhat below the compression curve. This effect is well shown in the diagram, Plate I. Fig. 4, from which it appears that this negative loop on the compression curve gives a mean pressure of 1½ lb. per square inch, and at low speeds is comparable in area with the suction loop. From these causes the total power indicated in the light-load trial would somewhat exceed the engine losses when running at full load; but this fact is, to some extent, counter-balanced by the fact that in the light-load test I arranged matters so that two cylinders out of four drew in air from the atmosphere direct, instead of from the induction-pipe, thus reducing somewhat the suction losses. By taking diagrams from cylinders when firing and running idle at different speeds I have estimated the losses in pumping and compression, both when all cylinders are firing and when three are running idle. The power absorbed in this way is small, amounting to about one-sixth of the total power required to drive the engine round at 1000 revolutions with only one cylinder firing, or rather less than 1 horse-power. When the engine is running in the ordinary way these negative loop losses are rather less—perhaps two-thirds of a horse-power instead of 1 horse-power—but having regard to their small total amount, it did not seem worth while to take account of this difference. I assume, therefore, that the total loss—negative loop plus mechanical friction—is the same when all cylinders are firing and the engine fully loaded as it is when only one cylinder is working.

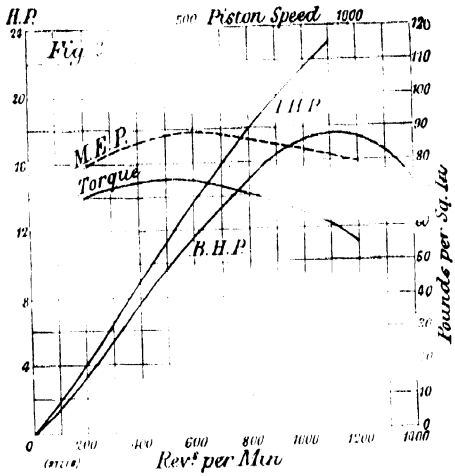
The curve A, Plate I, Fig. 1, shows the indicated horse-power required to drive the engine with a single cylinder at various speeds, and the curve B on the same figure shows the indicated horse-power lost in pumping and compressing, estimated as above. The purely mechanical loss is the difference between these two curves. It increases considerably more rapidly than in proportion to the speed, which is to be expected, because it is due mainly to fluid friction--that is, to the viscosity of the oil between

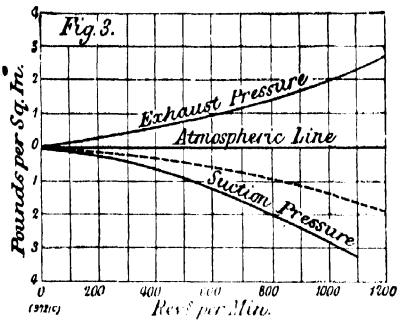
the piston and the cylinder. I take curve A as giving the power which must be added to the brake power observed at any speed in order to obtain the indicated power at that speed with the throttle fully open.

Brake Power. The power given by the engine at a given speed depends mainly on two factors—that is, the supply of petrol and the timing of the ignition, and regulation is effected by varying one or both. In this engine the petrol supply is regulated by throttling the air which passes through the carburettor. The jet is adjusted by the makers, and the air drawn over it by the engine takes up an amount of petrol per cubic foot which is determined by the velocity of the air stream and its temperature, and possibly also by the amount of moisture that it contains. The result is that the composition of the mixture of petrol vapour and air drawn into the engine is not under the control of the experimenter, though the amount of mixture taken per stroke can be controlled by the throttle. Regulation by throttling involves a reduction of pressure in the engine at the commencement of the compression stroke, and there is a proportionate reduction throughout the whole cycle. A considerable amount of pumping work has also to be done when the throttle is closed. In the tests here described the throttle was kept fully open, so that the pressure in the engine at the beginning of compression was only less than atmospheric by the amount necessary to draw the air over the carburettor and past the valves. Under these circumstances the quantity of fuel drawn into the engine per stroke at a given speed is determined by the carburettor, and is independent of the timing of ignition. In making a brake test the engine was run at a certain speed, and the timing of the ignition was then adjusted until the power shown on the brake was a maximum for that speed. It is to be noted that under these conditions the thermal efficiency (reckoned either on brake power or indicated power) is not necessarily a maximum. A different strength of mixture from that given by the carburettor might give a higher efficiency.

The brake-power so found is plotted in terms of the speed in Fig. 2 on the curve marked "B.H.P." Adding to the brake horse-power at any speed the mechanical and suction losses at that speed given by curve A (Fig. 1), the indicated horse-power is obtained, and this is plotted in the curve marked "I.H.P." From the latter curve, and from the speed and cylinder dimensions, is deduced in the ordinary way the mean effective pressure shown on the diagram, and this is plotted in terms of the speed in the dotted curve marked "M.E.P." The lower curve, marked "Torque," is related to the B.H.P. curve in the same way as the M.E.P. is related to the I.H.P. curve—that is, it represents the mean pressure in the cylinder which would be necessary to give the observed brake horse-power if there were no mechanical or pumping losses. The ordinate of this curve is, of course, proportional to the torque exerted by the engine at the corresponding speed. The mechanical efficiency, which is the ratio of the







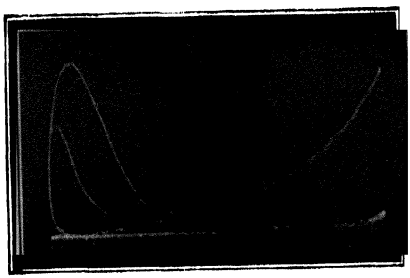


Fig. 4. 1 millimetre = 3·3 lb. per sq. in.

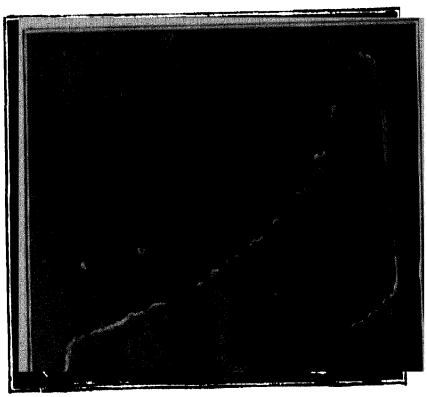


Fig. 5. 1 millimetre = 5·47 lb. per sq. in.



Fig. 6. 1 millimetre = 1·27 lb. per sq. in.

B.H.P. and I.H.P. ordinates, is plotted in terms of the speed in Plate I, Fig. 1 (curve C).

The curves for mechanical loss, mechanical efficiency, and I.H.P. are plotted as far as 1100 revolutions per minute, which is the highest speed at which the engine can be driven with one cylinder. At this speed the efficiency is approximately 75 per cent.; when the engine is loaded, one cylinder is occupied in overcoming losses, and the other three develop the brake power. The B.H.P. curve is carried as far as 1400 revolutions per minute; but at speeds above 1100 the action of the carburettor becomes rather uncertain, and the B.H.P. varies a good deal.

Attempts were made to confirm the accuracy of the engine loss determinations by running the engine without load and with all four cylinders working. The spark had to be greatly retarded in order to reduce the power sufficiently for this purpose, as regulation could not, of course, be effected by throttling. The result was that, in most cases, the explosion was of a very irregular character, and it was impossible to get even a rough approximation to the mean indicated power. The diagram, Plate I, Fig. 4, which was taken with all four cylinders firing, no load, and at a speed of about 450 revolutions per minute, shows two consecutive explosions, and the area of the smaller cycle is not more than about one-third of the larger. Moreover, it is probable that with the spark so much retarded as is here the case, the diagrams given by the four cylinders are far from being the same.

Thermal Efficiency. The determination of thermal efficiency involves a measurement of the consumption and calorific value of the fuel. The measurement of petrol consumption presented no difficulty, except that of keeping the speed constant. It was found very difficult to keep both the brake-load and the speed the same for more than a few seconds at a time. In order to eliminate the uncertainties introduced by varying speed, the petrol consumption in a definite number of revolutions—about 2000 or 3000—was observed, the speed being kept as constant as possible during the test; but the attention was centred rather on brake-load and petrol consumption per revolution than on measurements of speed.

The determination of calorific value proved to be a more difficult matter. The fuel used was Pratt's motor spirit, having a density of 0·715. The values given in books for the calorific value of petrol vary very greatly; results as low as 11,000 thermal units per pound having sometimes been recorded. Apart from adulteration, however, there cannot be much doubt that the calorific value exceeds 17,000. Robinson gives 19,800 (higher value) or 18,300 (lower value) thermal units as the value for gasoline, of specific gravity 0·69, obtained with a bomb calorimeter. I had not a bomb calorimeter at my disposal, and therefore tried burning the petrol in a coil lamp under a Junker calorimeter. The first result obtained was under 16,000 thermal units (lower value), which was certainly too low; and on searching for the cause of this, it was found that there was a

large deposit of carbon in the vaporiser coil of the lamp. The petrol had apparently cracked on coming into contact with the red-hot coil, and the calorific value of this separated carbon was, of course, lost. Determinations were then made with an external vaporiser, the temperature of which could be controlled and kept below the point at which petrol cracks. With this vaporiser in use considerably higher values were obtained, the best results averaging about 17,500 (lower value). This, I think, cannot be very far from the truth. After a large quantity of petrol had been burnt, the vaporiser was opened up, and was found to be quite free from any deposit of carbon or other residue. I think it very probable that the low calorific values of liquid fuels which are frequently obtained with the Junker calorimeter are mainly due to decomposition of the fuel in the coil lamp. The lower calorific value was obtained by deducting from the heat given to the calorimeter water the latent heat of the water actually condensed in the test.

Taking, then, the calorific value as 17,500 thermal units per pound, the petrol consumption and thermal efficiency found in a number of tests is given in the following table:

Speed	Petrol Consumption (Pounds)			Thermal Efficiency	
Revs. per Minute	Per I.H.P. Hour	Per B.H.P. Hour	Per 1000 Revs	On I.H.P.	On B.H.P.
400	0·78	0·9	0·30	18·6	16·1
400	0·75	0·87	0·28	19·3	16·6
600	0·685	0·81	0·26	21	17·9
600	0·655	0·77	0·24	22	18·8
800	...	...	0·24		
1000	0·6	0·75	0·22	24·2	19·3
1000	0·6	0·75	0·206	24·2	19·3
1100	0·59	0·785	0·202	24·6	18·4
1225	(0·65)	0·94	0·22	(22·3)	15·4

NOTE. At speeds 400, 600 and 1000, two tests are given to show the range of variation. At 1225 the indicated horse-power is uncertain, as no direct measurement of loss was made at that speed.

It is to be noted, in criticising these efficiencies, that the efficiency at each speed is not necessarily the best possible for that speed, since, as already stated, the strength of mixture could not be varied. Moreover, it should be stated that at low speeds the usual practice is to run the engine with the throttle partially shut; at such speeds, therefore, the tests do not quite represent the conditions of practice.

General Observations. It will be seen that the efficiency of the engine increases considerably with the speed. This, of course, is to be expected to some extent on account of the reduced heat losses; but since the heat

saved from going into the jacket-water is, in large measure, merely reserved to be discharged into the exhaust, but little of it being converted into work, it is difficult to suppose that the whole of the large difference between 400 and 1000 revolutions per minute can be accounted for in this way. I think that the difference is probably due in large measure to more complete mixture and combustion of the fuel at the higher speeds.

The petrol taken per revolution falls off very markedly as the speed increases. This is, in part, due to the fact that at high speeds the quantity of air taken per revolution is less than at low speeds. At a speed of 1000 revolutions per minute, with the throttle fully open, the pressure in the induction-pipe is about $1\frac{1}{2}$ lb. per square inch below atmosphere, and the pressure in the cylinder at the end of the suction stroke is slightly, though very little, below the pressure in the induction-pipe. At a speed of 400 revolutions per minute, the pressure in the induction-pipe is less than $\frac{1}{2}$ lb. below atmosphere. The cylinder contents in the former case will, therefore, be at least 6 per cent. less than in the latter. At high speeds, moreover, the temperature of the cylinder contents will become greater, because the amount of exhaust gases left in the cylinder bears a larger proportion to the whole charge. On all accounts, therefore, we may expect a considerable reduction of the air supply as speed goes up. It can hardly be supposed, however, that only two-thirds as much air is taken per revolution at 1100 revolutions as at 400; the fact that the petrol supply is reduced in nearly that ratio points strongly to the conclusion that at the lower speed the mixture taken into the engine is much richer than at the higher. At low speeds, running with the throttle wide open, the engine is apt to fire back into the induction-pipe, and this is usually supposed to indicate that there is excess of air in the mixture. It seems to me that the excess may be the other way, there being more petrol than can be burnt in the available air, and if that is so, it would go far to account for the low efficiency at the low speed. I am, however, investigating this point further.

It is interesting to compare the results obtained from this high-speed engine with the corresponding figures for an ordinary gas-engine of the same compression ratio. There happens to be in the laboratory a gas-engine in which the compression ratio is 3·79; speed, 250 revolutions per minute; cylinder diameter, $7\frac{1}{2}$ in.; and the stroke, 9 in. This engine gives about 5 horse-power on the brake—that is, about as much power as a single cylinder of the Daimler engine. It has been tested very accurately under all sorts of conditions, and it has been found that the thermal efficiency, when fully loaded, varies according to the strength of the mixture from 20·3 to 23 per cent. on the lower calorific value of the gas used. The best efficiency found is rather less than the best efficiency obtained with the Daimler engine in any test—that is, $24\frac{1}{2}$ per cent. The greatest mean pressure obtained in the gas-engine when fully loaded

was 67½ lb. per square inch. In the Daimler engine a mean effective pressure of as much as 85 lb. per square inch has been recorded at a speed of 1000 revolutions per minute. The difference between the engines in this respect is very marked. The calorific values of mixtures of petrol vapour and air, and of coal-gas and air, each of the maximum explosive strength, do not differ substantially. To obtain a higher mean pressure with one than with the other, it is therefore necessary either to use it more efficiently or to use more of it per cubic foot of stroke volume. The higher mean pressure given by the Daimler engine is mainly due to its greater efficiency. The gas-engine, when giving its highest mean pressure was using a mixture of maximum strength, and was working at low efficiency—about 20½ per cent. The Daimler engine, with an efficiency of 24½ per cent., therefore does one-fifth more work for the same fuel. In addition to this, however, the Daimler engine takes more fuel per cubic foot of stroke volume than the gas-engine, because the air supply is cooled by the evaporation of the petrol. The density of the mixture at the end of the suction stroke is proportional to its pressure, and inversely proportional to its absolute temperature. The pressure is substantially the same in the two engines (gas-engine running at 250 revolutions, and petrol-engine at 1000 revolutions), being about 1½ lb. per square inch below atmosphere in each case. But the temperature is 15 deg. to 20 deg. lower, or 5 or 6 per cent. in the petrol-engine, and there is correspondingly more fuel taken per cubic foot of stroke volume, and a corresponding increase in the mean pressure over and above that due to the higher efficiency.

Fig. 3 shows the pressure at the middle of the suction and exhaust strokes at various speeds when the cylinder was not firing. These pressures were taken from light spring diagrams, and were checked by the use of a valve which was opened at the middle of the suction or exhaust-stroke, and placed the cylinder momentarily in communication with a mercury gauge. The dotted curve on the same figure shows the pressure in the induction-pipe. When the cylinder is firing, the pressure in the suction stroke is substantially unaltered, but the exhaust pressure is lower, sometimes falling below atmosphere.

Of the other diagrams, Fig. 4 has already been explained; Fig. 5 is a full-load diagram, at a speed of 900 revolutions per minute; and Fig. 6 is a light-load diagram, with the throttle partly closed; speed, about 1100 revolutions per minute. The slight rise of pressure at the end of the exhaust-stroke shown on No. 6 is due to the fact that the exhaust-pipes of the four cylinders are coupled; when the cylinder which is being indicated is just completing its exhaust, the exhaust-valve of the adjacent cylinder is just opening. A sharp puff of gas is then discharged into the exhaust-pipe at this point, and the consequent rise of pressure is felt in the other cylinder, and shown on the indicator.

16

ON THE GASES EXHAUSTED FROM A PETROL MOTOR.

By B. Hopkinson and L. G. E. Morse.

[Paper read before the Engineering Section (G) of the British Association at Leicester: reprinted from "Engineering," August 9th, 1907.]

There has recently been a considerable amount of discussion upon the nature of the gases exhausted from motor-car engines. Results of tests published in various technical journals show that in many cases a considerable percentage of carbon monoxide is present in these gases. The investigation here described deals with the conditions under which carbon monoxide is formed in a high-speed internal-combustion motor, and the relation between the composition of the exhaust gases, the strength of mixture, the power developed by the engine, and the thermal efficiency.

The experiments were made in the Engineering Laboratory of Cambridge University, on a four-cylinder 16 to 20 horse-power Daimler engine, which was kindly lent by the manufacturers. The following are the particulars of the engine:

Total volume of one cylinder with piston on out-centre, 0·04 cubic feet.
Volume of compression space, 0·0104 cubic feet.
Compression ratio, 3·85.
Diameter of cylinder, 3·56 inches.
Length of stroke, 5·11 inches.

The ordinary carburettor supplied with the engine was used in the experiments, but arrangements were fitted by which the quantity of petrol delivered from the carburettor could be readily controlled. In this carburettor, which is of the float-feed jet type, a certain amount of air passes at a reduced pressure over the jet where it takes up the charge of petrol to an amount depending on the pressure over the jet. The air, thus charged with petrol vapour, mixes with a further quantity of air before entering the inlet pipe of the engine. Separate controlling valves were provided on each of these air-streams, and by varying the relative amount of throttling the petrol consumption could be adjusted over a wide range*. In

* It is hardly necessary to point out that the normal running conditions were widely departed from in these experiments. In the carburettor as supplied, the petrol consumption at a given speed, and with the throttle full open, is practically a fixed quantity.

order to make the conditions as uniform as possible, the whole of the tests were conducted at a nearly constant speed of 700 to 750 revolutions per minute, and the main air-inlet was kept open to the atmosphere with but little throttling, so that the pressure in the inlet pipe of the engine close to the inlet-valves, which was measured by means of a mercury gauge, was always within $\frac{1}{2}$ lb. per square inch of the atmosphere. Under these circumstances the amount of air taken into the engine per stroke is very nearly constant, under constant conditions of external temperature and pressure.

The petrol used was Pratt's motor spirit, the density varied slightly with different samples between limits of 0·715 and 0·720. The calorific value of all the samples was practically the same, and amounted to 18,900 British thermal units per pound (lower value). The indicator diagrams were taken with the indicator designed by one of the authors. The power given by the engine was measured by a Prony brake of the ordinary type, the load on which could be read correct to about 2 per cent. When running at a constant speed and with constant suction, as in these experiments, the mechanical and pumping losses in the engine are also constant, and amount, at a speed of 725 revolutions per minute, to 2 horse-power, equivalent to a load of 4 lbs. on the brake at a radius of 43 inches*.

In making a test the air-passages were first set to give approximately the desired petrol consumption, which could be done with considerable accuracy. The timing of the spark was then adjusted until the brake-load was a maximum. When all the conditions had become steady, one observer watched the level of the petrol in the graduated tank from which it was drawn, while another read, on a counter attached to the half-time shaft, the number of revolutions corresponding to a given fall in the tank, the beginning and end of which were signalled by the first observer. Owing to the impossibility of keeping the speed quite constant, this method of taking petrol consumption is much superior in accuracy to that based on time and speed observations. The samples of exhaust gases were taken over mercury. The gases were analysed by the ordinary volumetric methods, the CO_2 being absorbed by potash, the oxygen by pyrogallol, the CO by an acid solution of cuprous chloride, and the hydrogen by palladianised asbestos.

The following table contains the results of a series of such tests made on two consecutive days, the conditions (other than petrol consumption) being as nearly as possible the same throughout. The first row of figures gives the measured consumption of petrol per 1000 revolutions, the second the brake-load, and the third the thermal efficiency on the indicated power based on the measured calorific value of 18,900 B.TH.U. per pound. The next four rows give the measured amounts of carbon dioxide, carbon mon-

* Determined by running the engine with only one cylinder firing and indicating that cylinder. See *Engineering*, February 8, 1907. Page 199 of this volume.

oxide, oxygen, and hydrogen, reckoned as percentages of the volume of dry exhaust gas. The residual gas after all these have been absorbed will

Petrol consumption (lb.) ...	0·181	0·191	0·197	0·217	0·250	0·293
Brake load at 43-in. radius (lb.)	25	27·5	29·3	29·4	29·3	27
Thermal efficiency	0·244	0·252	0·261	0·238	0·204	0·162
CO_2 —measured	10·9	12·8	13·5	10·6	9·6	6
O_2 —measured	3·6	1·5	0·2	...	...	...
CO —measured	...	...	0·7	5	6·25	11·6
H_2 —measured	...	...	...	2·1	2·65	8·7
N_2, by difference	84	84	84	81	80	73
Total O_2, calculated from N_2	22·4	22·4	22·4	21·5	21·3	19·4
H_2O calculated	15·8	16·2	16·8	16·8	17·2	15·2

be nitrogen, mixed possibly with some unburnt hydrocarbon vapours of negligible volume, and with sufficient water vapour to saturate it, which may be taken as about 2 per cent. The eighth row of figures shows the quantity of nitrogen found by difference in this way. Dividing the nitrogen by 3·76, the total amount of oxygen used is obtained, and this is given in the next row of figures. If from the total oxygen be deducted that which went to form the absorbed gases (its volume is equal to the sum of the CO_2, the O_2 and half the CO), the remainder is the oxygen which has combined with hydrogen to form steam. The volume of the resulting steam is shown in the last row of figures.

The composition of the petrol, determined by combustion and weighing the CO_2 and water produced, was as follows:

Hydrogen ...	...	14·86 per cent.
Carbon ...	...	84·66 ,, ,,
		99·52 ,, ,,

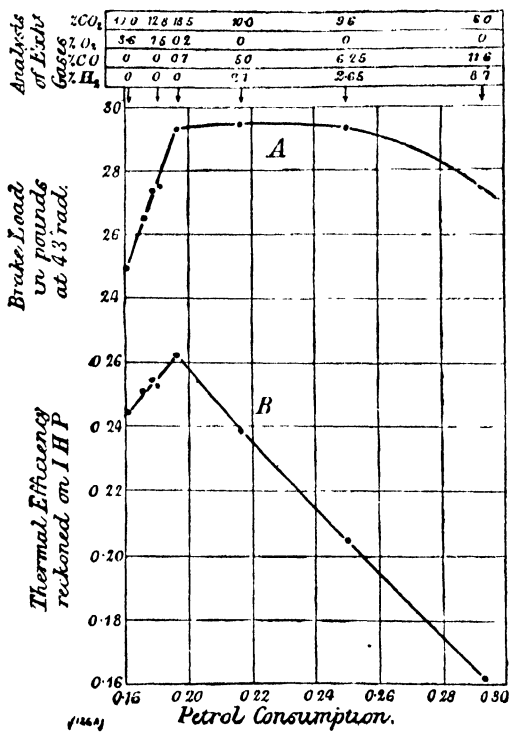
from which it appears that the volume of steam formed should be 1·05 times the combined volume of CO_2 and CO in any case in which the petrol is all burned. The ratio of these two volumes, as found above, is always considerably greater than this, the smallest value (corresponding to 0·197 lb. of petrol) being 1·18. Various causes contribute to this result. Some soot is always formed, and probably also some acetylene, both of which tend to reduce the burnt carbon as compared with the burnt hydrogen. Moreover, a small error of defect in the estimation of the CO_2 causes a large error in the ratio; if, for example, the CO_2 in the third analysis (0·197) had been 14 instead of 13·5, the calculated H_2O would have been 15·6, and the ratio 1·06.

The analyses show that when the petrol consumption is about 0·2 lb. per 1000 revolutions, the available oxygen is completely burned to CO_2 and steam. The petrol is, however, not completely burned, since there is always some soot and probably also some hydrocarbons in the exhaust. The quantity of available oxygen is probably about sufficient to burn 0·19 lb. of petrol under normal atmospheric conditions. If the supply of petrol exceeds 0·2 lb., oxygen disappears from the exhaust, but CO and hydrogen

are present in increasing quantities; if it be less than 0·2, there is excess of oxygen and no CO.

In Fig. 6, curve *A* shows the relation between brake-load and petrol consumption, the quantity of carbon monoxide and oxygen found in the exhaust gases being marked at various points on the curve. It will be seen that the curve has a very flat maximum, extending from 0·196 lb. to 0·25 lb. per 1000 revolutions. Within errors of observation, the power is constant over this range, and over the greater part of it carbon monoxide is present in considerable quantities.

Fig. 6.



It is at once apparent from this that if the carburettor be set in the usual manner, so that the engine gives its maximum power, no attention being paid to petrol consumption, the exhaust is almost certain to contain large quantities of carbon monoxide. If, however, the consumption of petrol be observed, and be kept down to the lowest figure consistent with the engine giving its maximum power, or something near it, the formation of carbon monoxide may be completely prevented. In order to secure this result a slight sacrifice of power—perhaps 1 or 2 per cent.—may be necessary; but it is obvious from an inspection of the curve that in this particular engine the power is, within errors of observation, the

maximum the engine can give, at the point at which maximum efficiency is attained, and at which carbon monoxide disappears.

In interpreting the relations between power and petrol consumption shown by this curve, it is necessary to remember that under the conditions of the experiments the amount of oxygen taken by the engine is practically constant, and that the power developed will depend upon the amount of heat produced by the combustion of that oxygen. The oxygen may be burnt either to carbon monoxide, to carbon dioxide, or to steam, and the development of heat depends upon the relative proportions in which these three gases are produced. A given quantity of oxygen combining with carbon to form carbon dioxide or carbon monoxide, or with hydrogen to form steam, gives amounts of heat which are in the proportion of the following numbers respectively:

CO_2	97,000
CO	58,000
H_2O	120,000* (lower value)

from which it is obvious that the engine would give the greatest amount of power, if the combustion could be so arranged that only the hydrogen in the petrol were burnt, the carbon being wholly discharged as soot. It is therefore quite possible that a greater amount of power might be developed, owing to the more efficient utilization of the oxygen, with a mixture in which the petrol was in excess of that which the air could completely burn.

The analysis given in the table above shows the manner in which the oxygen is utilized in each case. In the following table the proportion in which the oxygen is shared between carbon dioxide, carbon monoxide, steam, and unburnt, is shown for some of the analyses given above, and the corresponding quantity of heat developed is also given, being calculated from the percentages:

Petrol consumption	0·181	0·197	0·250	0·293
Per cent. of oxygen to CO_2	48·6	60·3	45	30·9
Per cent. of oxygen to CO	...	1·5	14·6	29·9
Per cent. of oxygen to H_2O	35·2	37·5	40·4	39·2
Per cent. of oxygen unburnt	16·2	1·2	...	...
Total heat*	89,500	104,400	100,570	94,300
M.E.P., from load (lb. per sq. in.) ...	76·6	88	88	82
Thermal efficiency (on indicated power)†	25·5	25	26	25·8

* Calories produced per 32 grammes of oxygen used.

† In taking out these efficiencies the heat supply is calculated on the assumption that the quantity of oxygen taken per 1000 revolutions is sufficient to completely burn 0·19 lb. of petrol—namely, about 0·66 lb., or 300 grammes. It is also assumed that the CO_2 and CO are formed from solid carbon, and the steam from gaseous hydrogen, no allowance being made for the heat absorbed in breaking up the hydrocarbons. The heating value of the petrol, so calculated from the analysis, is 20,450 B.TH.U. per pound; the measured value is 18,900 lbs.—a reduction of 7 per cent. The true efficiency in the second column, where the petrol is almost completely burnt, should therefore be about 7 per cent. greater, or 26·8 per cent. In the other cases the increase will be somewhat greater, since more petrol is decomposed than is burnt; but the general conclusion, that the efficiency on the basis of the actual heat of the combustion is substantially constant, will not be affected.

It will be apparent from this table that the mean pressure is nearly proportional to the calculated amount of heat, and that the thermal efficiency, reckoned on the heat actually produced by the combustion, is constant.

The curve *B*, Fig. 6, shows the thermal efficiency based on the actual petrol consumption and the indicated power. It will be seen that the curve reaches a very sharp maximum near the point 0·2 lb. per 1000 revolutions, at which the petrol is just sufficient to be burnt by the available oxygen. The falling off of the efficiency, according to an approximately straight line law, when the consumption is in excess of this, is, of course, to be expected, because the amount of heat developed, being dependent upon the constant quantity of available oxygen, remains nearly constant. The almost equally rapid decline in efficiency when the petrol consumption is reduced is also due to incomplete combustion of the fuel. As there is plenty of oxygen to burn it, this incompleteness of combustion is, at first sight, not easy to explain. It appears to be caused by the dilution of the mixture by the exhaust gases of the previous explosion, in consequence of which the mixture in the cylinder of the engine, as distinct from that in the inlet pipe, is already fairly dilute, even when there is as much petrol vapour present as can be completely burnt by the available oxygen. If the mixture be further diluted by adding excess of air, the explosion becomes so slow that the flame has not time to spread completely through the cylinder, and consequently a good deal of petrol vapour is discharged unburnt in the exhaust along with the oxygen which ought to have burnt it.

The analyses show that the quantity of oxygen taken into the engine per 1000 revolutions is nearly equal to that required to burn 0·19 lb. of petrol—that is, about 7·3 cubic feet; the corresponding quantity of air is 35 cubic feet, reckoned at 0° C., and 14·7 lbs. per square inch. The amount of mixed air and petrol vapour drawn into one cylinder of the engine in the course of a suction stroke will therefore be 0·0177 cubic foot under the same conditions of temperature and pressure. Now the mean temperature of the gases at the end of the suction stroke may be taken as 130° C. without serious error, and the pressure as approximately 14 lbs. per square inch; the total quantity of gas present reduced to standard conditions will therefore be about 0·0256 cubic foot; and of this, as we have seen, only about 0·0177 cubic foot, or 70 per cent., is explosive mixture, and the remainder, or 30 per cent., is exhaust gas. The mixture, when it enters the engine cylinder, is therefore diluted with nearly half its volume of inert gases.

The indicator diagrams, Figs. 1 to 5, Plate I, show clearly the character of the mixtures with different consumptions of petrol. It will be seen that Fig. 1, which corresponds to a consumption of 0·165 lb. per 1000 revolutions, shows the characteristics of a weak mixture, the ignition being slow. This

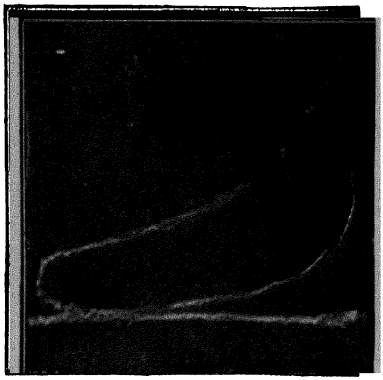


Fig. 1. Scale, 1 in. = 135 lb. per square inch.
Petrol, 0·165 lb. per 1000 revolutions.

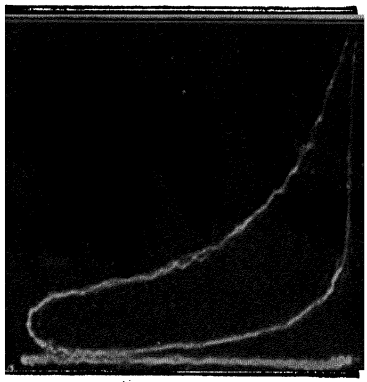


Fig. 2. Scale, 1 in. = 135 lb. per square inch.
Petrol, 0·197 lb. per 1000 revolutions.



Fig. 3. Scale, 1 in. = 135 lb. per square inch.
Petrol, 0·172 lb. per 1000 revolutions.

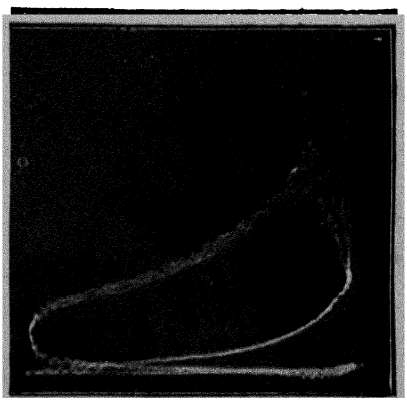


Fig. 4. Scale, 1 in. = 135 lb. per square inch.
Petrol, 0·297 lb. per 1000 revolutions.

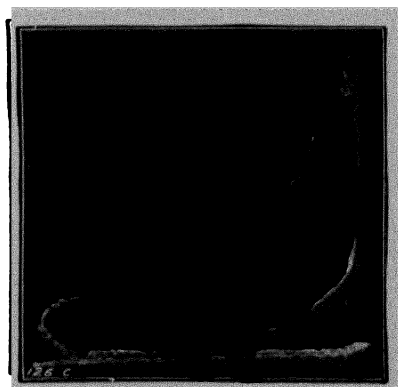


Fig. 5. Scale, 1 in. = 135 lb. per square inch.
Petrol, 0·293 lb. per 1000 revolutions.

may be compared with Fig. 2, corresponding to 0·2 lb. per 1000 revolutions: the best mixture, as regards power and thermal efficiency, for this engine. The setting of the spark is the same in *A* and *B* (Fig. 6). In Fig. 3 the consumption of petrol is about the same as in *A*, but the spark has been very much advanced, so that the diagram approaches more nearly in shape to that given by the stronger mixture; but the firing under these circumstances is irregular, and the power is no greater than before. Fig. 4 is that given by 0·3 lb. of petrol per 1000 revolutions, and Fig. 5 by a similar mixture, but with the spark more advanced.

In a gas engine using high compression the volume of the exhaust gas present in the charge is much smaller, and the power can be varied over a wide range by altering the strength of mixture without seriously altering the efficiency or causing incomplete combustion. The compression space in the Daimler engine is approximately one-quarter of the total cylinder volume, and in addition to the gases retained in that space after exhaust, a good deal of exhaust gas gets from the exhaust-pipe into the inlet-pipe during the overlap of the exhaust and inlet-valves.

The thanks of the authors are due to Dr Fenton and Mr Tasker for kindly making an analysis of the petrol, and to Mr de Morpurgo for assisting in some of the tests.

17

ON THE MEASUREMENT OF GAS ENGINE TEMPERATURES.

[From the "PHILOSOPHICAL MAGAZINE," January 1907.]

AN important point in the experimental study of the gas engine is the determination of the temperature of the gases at each point of the cycle. It is obvious that if the mean temperature at any one point is known, that at any other can be calculated from the indicator diagram, assuming the relation $\frac{pv}{\theta}$ = constant to hold throughout. This relation is probably true as a practical approximation for the gas engine mixture since it consists, as to 80 per cent. of its volume, of nitrogen and oxygen, which pass unchanged through the combustion. For the starting point of the temperature determinations it is most convenient to choose the temperature at the end of the suction-stroke, when the charge of gas has just been drawn in and its compression is about to commence. The measurement of this temperature, or "suction temperature" as it is generally called, is best effected by determining the quantity of air and gas drawn into the engine at each stroke, from which, knowing the pressure and volume of the mixed gases when the inlet-valves have closed, their temperature can be calculated. The accurate measurement of the large volumes of air used by a gas engine of considerable size is, however, a difficult operation; and it is desirable, if possible, to use some means of directly measuring the temperature of the charge at the end of the suction-stroke. Moreover, the subsequent calculations from the indicator diagram of the temperature at other points of the cycle, lead to mean values for the whole volume, and give us no information as to the difference of temperature between one part of the cylinder and another. Such differences may amount to several hundred degrees Centigrade during the first half of the expansion or working stroke. They arise partly from the cooling effect of the walls, and partly from the manner in which the inflammable mixture is ignited. As I have shown elsewhere*, the temperature round about the point of ignition is necessarily some 500° higher than at a distance, because of the relative slowness of the propagation of flame as compared with the time of reaction of the gases when the flame has once reached them. For the complete study of gas engine temperature therefore some method of local thermometry is necessary.

* *Proceedings of the Royal Society*, A, vol. 77, p. 387; *Engineering*, vol. 81, p. 777. Page 367 of this volume.

The platinum resistance-thermometer is the only means available for the direct measurement of rapidly changing gas-temperatures, since it alone can be constructed with the requisite low thermal capacity. Moreover, it gives the local temperature as distinct from the mean value obtained from a study of the pressure. It has been employed by Callendar and Nicolson* with great success for the measurement of steam temperatures in the steam engine cylinder; and Burstall has used it in the gas engine, but experienced great difficulties†. I have also used it in studying the explosion of gaseous mixtures in a closed vessel‡. The difficulty of the gas engine experiments is that the temperature rises locally much above the melting-point of platinum, and the wire fuses unless it is so thick that it fails to reach the maximum temperature of the gas surrounding it. But if the latter condition be fulfilled, it is obvious that the thermometer does not give the temperature of the gas at other points of the cycle, unless corrections are applied for the time-lag. Professor Burstall found it necessary to use wires 2/1000 in. diameter, in order to secure sufficient permanence in the thermometer to take a complete series of measurements of its resistance throughout the cycle by means of a rotating contact-maker. In my own closed-vessel experiments I was able to use the finest obtainable wire (1/1000 in. diameter), and it nearly always melted if placed near the point of ignition. But as a continuous record of the resistance was taken in each explosion prior to melting, this did not affect the measurements. Even with such fine wire the correction for time-lag was by no means negligible; and I was able to show that a wire double the diameter might be as much as two hundred degrees hotter or cooler than the gas surrounding it when the temperature changes so rapidly as in the working of a gas engine. The determination of the time-lag of the wire, or to speak more precisely of the relation between the temperature of the wire and that of the gas, is therefore a matter of much importance in these measurements. The difference of the two temperatures is due to the constant exchange of heat, which must go on between the wire and the gas to provide for the warming or cooling of the wire and for the loss of heat from the wire by radiation. If the temperature of the wire is known in terms of the time, it is easy to calculate the rate at which it is receiving heat from the gas. Let θ' be the temperature and t the time, then heat must be supplied to the wire at a rate $k\frac{d\theta'}{dt} + f(\theta')$, where k is the capacity for heat of the wire, and $f(\theta')$ the rate at which it loses heat by radiation *in vacuo* when at temperature θ'. The radiation term depends on the nature and extent of the surface of the wire and on the temperature; it is not important except at very high temperatures—for most purposes a sufficiently good approximation to its value can be obtained from the numerous published results for polished platinum, which are in fair agreement with

* *Min. Proc. Inst. C.E.*, vol. 131. † *Philosophical Magazine*, vol. 40 (1895), p. 282.
‡ *Proceedings of the Royal Society*, A, vol. 77, p. 387.

one another, and give the radiation per sq. cm. of surface up to temperatures near the melting-point.

The difference of temperature between the wire and the gas at a distance from it will, under any given conditions, be proportional to the rate of exchange of heat between wire and gas. If θ be the gas temperature, we have

$$k\frac{d\theta'}{dt} + f(\theta') = \lambda(\theta - \theta'),$$

whence θ can be obtained if λ is known. The constant λ is the rate at which heat passes between the wire and the gas, per degree difference of temperature between them, under the conditions of temperature, motion of the gas, etc., which actually obtain. These conditions vary from point to point of the cycle in the gas engine, and λ also varies. It is necessary, in fact, in measuring gas engine temperatures in this way to determine the relation between heat flow and temperature difference at each point of the stroke.

The experiments to be described in this paper consisted in a determination of the temperatures in the cylinder of a large gas engine by means of a platinum thermometer, the gas engine being motored round with the gas-supply cut off so that it simply compressed and expanded a charge of air. The object of the experiments was to test the platinum thermometer as a means of measuring rapidly varying gas-temperatures, rather than an enquiry into gas engine phenomena. The absence of explosions secures a fairly uniform temperature throughout the cylinder contents. The mean temperature of the charge given by the indicator diagram therefore does not differ much from the local temperature in the neighbourhood of the wire; and the wire temperatures can be checked by reference to the indicator diagram. I have used a simple method of getting the correction constant λ which seems to give good results, and a description of this may be useful to others who are desirous of measuring gas engine temperatures. The conclusion at which I have arrived is that by the use of this method of correction and sufficiently accurate measurements of resistance, the temperature of the gas can be obtained within a few degrees with a wire 4/1000 in. diameter, if the wire does not melt. But in large engines using high compression, a wire of this size will inevitably melt; and if still thicker wire be used, the correction for time-lag becomes unmanageably large. It appears that the measurement of temperature in such engines by this means is a very difficult operation, involving measurements of accuracy far exceeding that of the ultimate result, unless wires of a more refractory metal than platinum be used.

The briefest description of the gas engine on which the experiments were made is all that is necessary. It is one of Messrs Crossley Bros. well-known Otto cycle-engines, having a cylinder 11½ inches diameter, and a stroke of 21 inches, and it gives about 40 H.P. when running at 185 revolutions

per minute. The compression space is 407 cubic inches or $\frac{1}{5{\cdot}36}$ of the stroke volume, and the gases are compressed to about 170 lbs. per square inch absolute before firing. The engine has been lent to the Engineering Department of Cambridge University through the kindness of Mr F. W. Crossley. For the purpose of the experiments to be described it was belted to a dynamo (lent by Messrs Mather & Platt, Limited) by which it was driven, the gas-supply being cut off. Thus the engine drew in a charge of air through the air inlet-valve, compressed it to 173 lbs. per sq. in., expanded it again down to near atmospheric pressure, and expelled it through the exhaust-valve.

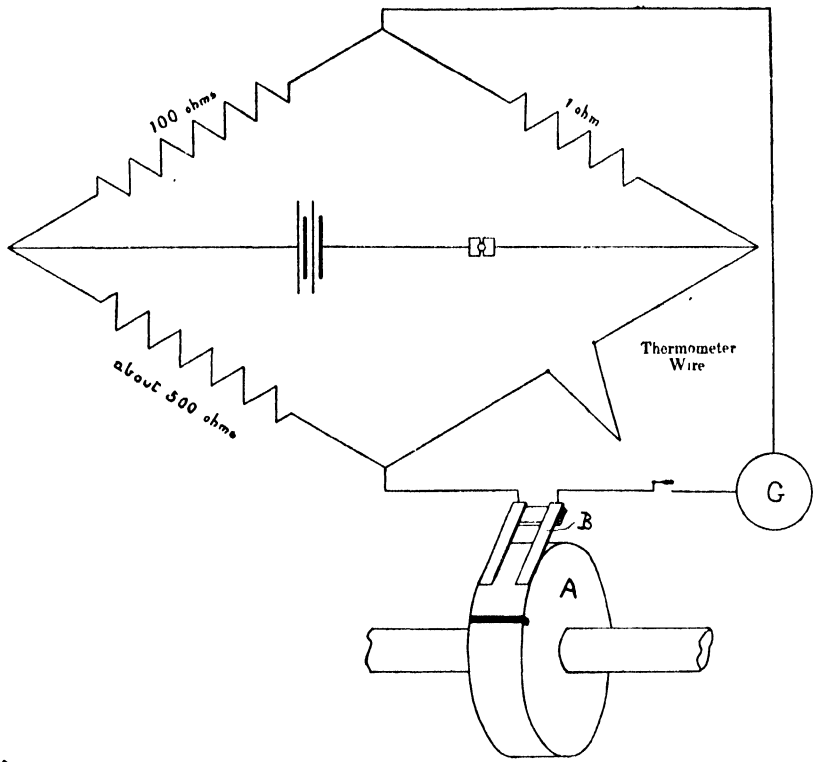


Fig. 1.

The thermometer is a piece of bare platinum wire, 4/1000 in. diameter and 29 cm. long. Its ends were hard-soldered to thicker platinum wires which were sealed into glass tubes passing through the exhaust-valve cover and projected about 1/20 in. into the compression-chamber of the engine. The wire was threaded through a hole in a thin mica plate, the plate being carried on the end of a steel rod 5 ins. long and 1/8 in. diameter, which was screwed into the exhaust-valve cover and projected into the compression-chamber. Thus the wire was supported in the form of a V, the ends being $2\frac{1}{2}$ ins. apart and close to the walls of the compression-chamber

and the apex in the centre of that chamber. The exhaust-valve is in the floor of the compression-chamber immediately below the wire. The inlet-valve is in the side of the compression-chamber. During the suction and exhaust strokes the motion of the air about the wire is violent and turbulent owing to its rapid ingress and egress through the valves. During the compression and expansion strokes, the air is probably almost at rest except in so far as it is moved by the piston.

The wire formed one arm of a Wheatstone's bridge, the connections being as in Fig. 1. The rotating contact-maker A is a block of hard wood soaked in paraffin and carried on the valve-shaft of the engine. It had a slip of brass 1/8 in. wide let into its periphery which made contact with the brushes B, and so completed the galvanometer circuit once in two revolutions of the engine, that is, once in the cycle described above. The moment of contact could be adjusted to any desired point in the cycle by altering the position of the brushes. The current in the wire with this arrangement of resistances was limited to about 1/100 ampere, and it had no appreciable heating effect upon the wire.

The resistance of the wire at 0° C. was 4·26 ohms, the temperature coefficient 0·00343, and δ (the factor for correcting platinum to Centigrade temperatures) was 1·57. The succession of temperatures determined in this way is shown in Fig. 2, curve A. Of the actual observations, those shown thus ● were all taken on the same day, and the conditions were approximately the same throughout. The temperature of the air in the air-pipe close to the engine was 17° C., the temperature in the exhaust-pipe 26½° C., and the temperature of the jacket-water 46° C. The mean speed throughout was 180 revolutions per minute. The observations shown thus ○ were taken on another day when the conditions would not be quite the same.

The connections were now altered as shown in Fig. 3. The conditions were kept exactly the same as before; in fact, the engine was not stopped. The current now passing in the wire (measured by the ammeter M) was about 0·75 ampere and produced a considerable heating effect. The external resistance of 50 ohms sufficed to keep the current substantially constant in spite of the varying resistance of the wire. The succession of wire temperatures under these conditions is plotted in curve B, Fig. 2.

From curves A and B the curve of gas temperature can be at once deduced. Let θ be the gas temperature, and θ'' the wire temperature given by curve B. Then if C is the current in the wire and R its resistance at the moment, heat is being supplied to the wire electrically at the rate C^2R, and by conduction from the gas at the rate $\lambda(\theta - \theta'')$. Neglecting radiation losses, we have therefore

$$\lambda(\theta - \theta'') = k\frac{d\theta''}{dt} - C^2R, \qquad \text{.....(1)}$$

the heat quantities being expressed in joules. Similarly, we have

$$\lambda(\theta - \theta') = k\frac{d\theta'}{dt}, \qquad \text{.....(2)}$$

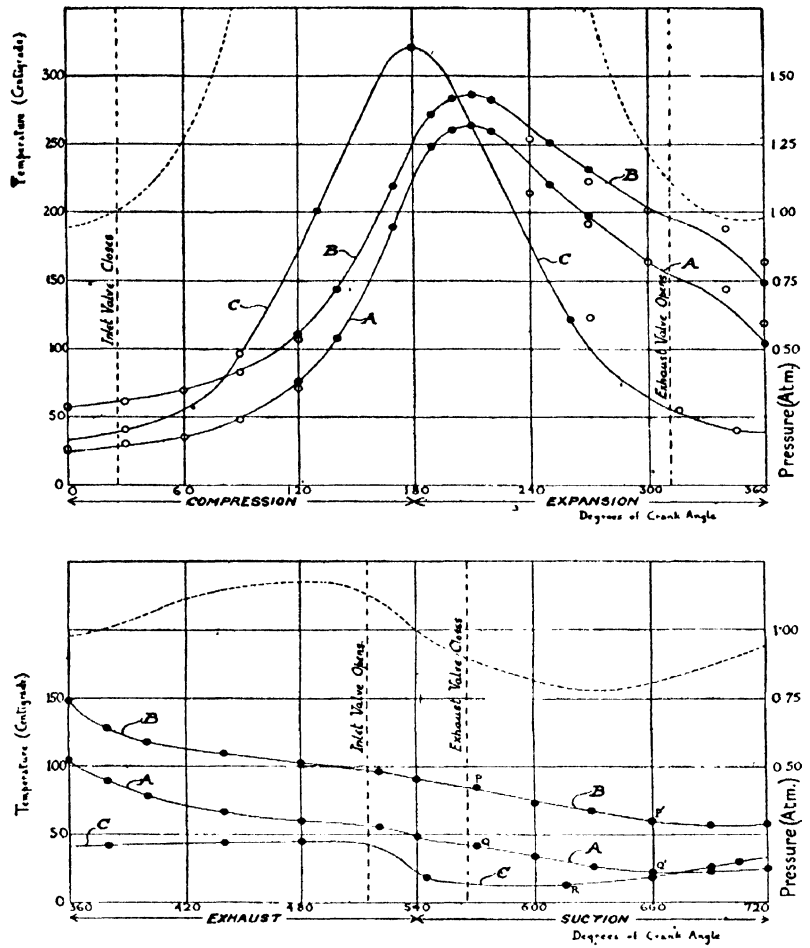

Temperature (Centigrade)
Pressure (Atm.)
Inlet Valve Closes
Exhaust Valve Opens
COMPRESSION
EXPANSION
Degrees of Crank Angle
Inlet Valve Opens
Exhaust Valve Closes
EXHAUST
SUCTION

Fig. 2.

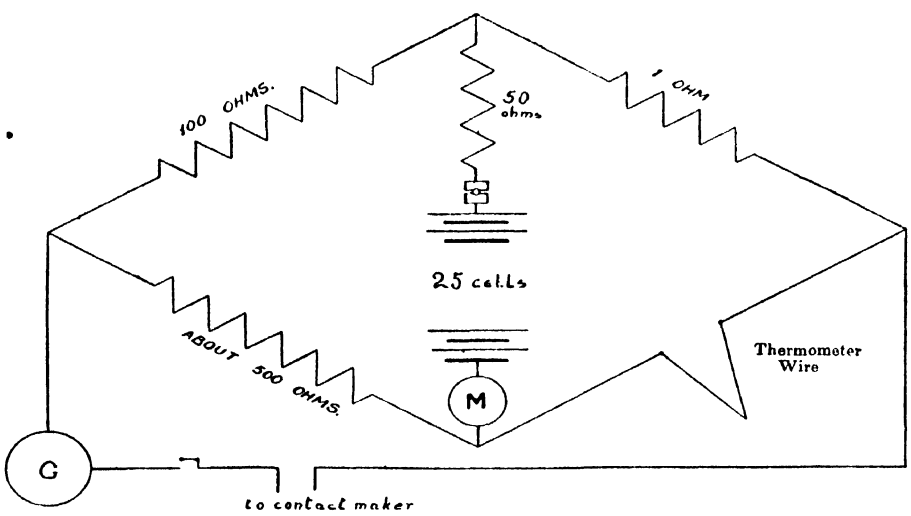

100 OHMS.
50 ohms
1 OHM
25 cells
ABOUT 500 OHMS.
Thermometer Wire
M
G
to contact maker

Fig 3.

θ' being the temperature shown on curve A. From these two equations, eliminating θ:

$$\lambda(\theta'' - \theta') = k\left(\frac{d\theta'}{dt} - \frac{d\theta''}{dt}\right) + C^2R;$$

so that λ can be determined. Having obtained λ, the gas temperature θ is calculated from the wire temperature by the aid of equation (2). The curve of gas temperature so obtained is curve C.

Before discussing the gas temperatures, it is advisable to give an example of the calculations by which they are obtained. Take, for instance, the points P, P', Q, Q', on the suction part of the stroke. These four temperatures were obtained in quick succession, so that the conditions would be substantially the same for all. This is a necessary precaution because it is difficult to keep the conditions absolutely constant over any long period of time, and calculations based, as these are, upon small differences of temperature are seriously vitiated by even a small change in conditions.

The following calculation is self-explanatory:

Temperature at	P	...	84·7° C.
,,	P'	...	59·6°
,,	Q	...	40·7°
,,	Q'	...	22·1°
Mass of wire (by weighing)			·053 gramme
Specific heat of platinum			·0324

Therefore capacity for heat of wire (k) is

$0{\cdot}0324 \times 0{\cdot}053 \times 4{\cdot}2 = 0{\cdot}0072$ joules per degree Centigrade.

Time interval from P to P' (90° of crank-angle) = $\frac{1}{1\frac{1}{2}}$ second.
Fate of fall of temperature along PP' = 300° per second.
Rate of fall of temperature along QQ' = 224° per second.
Rate of loss of heat of wire along PP' = $300° \times 0{\cdot}0072 = 2{\cdot}16$ watts.

$$\left[\text{This is } \quad k\frac{d\theta''}{dt} \text{ in equation (1).}\right]$$

Rate of loss of heat of wire along $QQ' = 224 \times 0{\cdot}0072 = 1{\cdot}61$ watts.

$$\left(\text{This is } -k\frac{d\theta'}{dt}\right).$$

The mean resistance of the wire between P and P' is 5·3 ohms, and the current is 0·765 amp. Heat is therefore supplied to the wire by the current at the rate of 3·1 watts. The rate at which the wire is losing heat to the gas along PP' is accordingly $3{\cdot}1 + 2{\cdot}16 = 5{\cdot}26$ watts. This is $\lambda(\theta'' - \theta)$. Along QQ' the rate of loss of heat is 1·61 watts, and this is the value of $\lambda(\theta' - \theta)$. The difference, viz. 3·65 watts, is eqal to $\lambda(\theta'' - \theta')$. The mean value of $\theta'' - \theta'$ over the range is 40·5°, whence

$$\lambda = \frac{3{\cdot}65}{40{\cdot}5} = 0{\cdot}09.$$

This is the rate (in watts) at which the wire loses heat to the gas per degree difference of temperature. The difference of temperature between the wire and the gas along QQ' is therefore $\frac{1{\cdot}61}{0{\cdot}09} = 17{\cdot}9°$ C., and the gas temperature at the middle point of the range (615° crank-angle) is 12° C. Thus the point R on the gas curve is found.

The wire temperatures are probably correct within 1°. If all four temperatures were in error to that extent, and if the errors were disposed in the most favourable manner, λ would be wrong by 0·0085, or about 10 per cent. of its value, and the error in the correction $(\theta' - \theta)$ would be about 3°. It is unlikely that all the temperature errors will combine to produce the maximum error in the result, and probable that this particular gas temperature is correct to within one or two degrees. At other points the gas temperatures are not so accurate; the possible error increases with the slope of curve B, and where that curve departs greatly from a straight line a smaller time interval must be taken to get the slope. Thus at the end of the compression or the middle of the expansion strokes, when the correction amounts to nearly 100°, the error may amount to 10 or 15 degrees.

The pressure in the cylinder throughout the cycle is shown by the dotted curve in Fig. 2. The pressure was obtained from indicator diagrams and (during the suction and exhaust strokes) by a water-pressure gauge which could be momentarily opened to the cylinder at any point in the cycle.

Several points of interest appear in a study of the curve of gas temperatures, and a comparison of it with the pressure curve. We may discuss these in order, beginning with the suction stroke [540°–720° crank-angle]. During the suction stroke the pressure in the cylinder falls considerably below atmospheric; but the work done on the air in forcing it through the constricted opening of the inlet-valve is almost wholly absorbed in heating it; for though the motion of the air in the cylinder during this part of the cycle must be very turbulent, the velocities cannot be such as to account for any considerable portion of the energy expended. The temperature of the stream of air just after it has passed the inlet-valve, and has spread out so that its velocity is greatly reduced, will therefore be very nearly equal to the external temperature; and it might be expected at first sight that a thermometer placed anywhere in the cylinder during the suction stroke would show that temperature. As a matter of fact, it will be seen from an inspection of the curve that the gas temperature shown by the thermometer falls in the middle of the stroke to about 10° C., that is some 7° below the temperature of the air in the inlet-pipe. Of this fall there cannot be any doubt, as it was confirmed on a number of different occasions. I think that it is probably due to the fact that the pressure at the middle of the suction stroke is lower than at the beginning, so that the air first drawn in has its temperature lowered by rapid expansion after it has

entered the cylinder and has been reduced nearly to rest. Towards the end of the suction stroke the reverse process occurs, since the air which entered at the middle of the stroke has had its pressure increased more or less adiabatically from about 11·3 lbs. to 14·7 lbs. per square inch. It will be seen that the temperature shown by the wire at this point is about 34° C.

The mean suction pressure exerted on the piston during the suction stroke is about $2\frac{1}{2}$ lbs. per sq. in., and the work done by the piston is 360 ft-lbs. per cubic foot of stroke volume. The pressure at the end of the suction stroke is very nearly atmospheric, but the air in the cylinder is then a little hotter than it was before it entered the cylinder, because some of the work done by the piston in suction has gone to increase its internal energy. The volume displaced by the piston exceeds the volume of air (reckoned at atmospheric temperature and pressure) which has entered the engine, by the increase of volume due to the rise of temperature, and work is done on the external atmosphere in the course of suction to an amount equal to such increase of volume multiplied by the atmospheric pressure. It is easy to see from this that the rise of temperature of the air in the cylinder (assuming that it receives no heat) must be equal to the work done upon it in drawing it in divided by its capacity for heat at constant pressure. Taking the temperature as 17° C., the latter is about 24 ft-lbs. per cubic foot, and the temperature at the end of the suction stroke should therefore be 14° above the inlet temperature, or 31°*. This agrees pretty well with the temperature shown by the thermometer at this point that is 34°. It is not to be expected that the agreement will be absolute because the air will have received some heat from the cylinder-walls, which have a temperature of 40° C.

The inlet-valve closes at crank-angle 25° (25° after the out-centre). The pressure is then almost exactly atmospheric, the temperature calculated from the wire thermometer is 37° C., and the volume is 1·45 cubic feet. From these data the mean temperatures can be calculated from the indicator diagrams at any point of the compression and expansion strokes until the exhaust-valve opens. It will be useful to compare the temperatures so calculated for one or two points with those given by the thermometer (curve *C*). At the in-centre (crank-angle 180°), the pressure reaches its maximum value of 173 lbs. absolute. The volume is then 0·236 cubic feet, and the mean temperature calculated from the pressure and volume is

$$\frac{173}{14{\cdot}7} \times \frac{0{\cdot}236}{1{\cdot}45} \times 310 = 590° \text{ absolute, or } 317° \text{ C.}$$

* If θ be the rise of temperature the volume of air drawn in per stroke is $\frac{290}{\theta + 290}$ of the stroke volume, since the absolute temperature in the inlet-pipe is 290°. The work done is 360 ft-lbs. per cubic foot of stroke volume, or $\frac{360 \times 290}{\theta + 290}$ per cubic foot of air drawn in. We have therefore $\frac{360 \times 290}{(\theta + 290)\,24} = \theta$, whence $\theta = 14{\cdot}4°$.

The temperature obtained from the thermometer is 320° C., but the correction here is about 100°, so that the possible error is considerable. The temperature near the wire is certainly above the mean, since the gas is losing heat to the cylinder-walls, and the layer in contact with the walls must be colder than the remainder. If we suppose that the gas in the neighbourhood of the wire has been compressed without loss of heat, the temperature there will be

$$\left(\frac{173}{14{\cdot}7}\right)^{\frac{\gamma-1}{\gamma}} \times 310 = 628° \text{ absolute, or } 355° \text{ C.}$$

The gas-temperature curve during expansion is, as it should be, nearly symmetrical with the compression portion. The pressures in expansion are slightly less than at the corresponding points in compression by reason of the loss of heat to the cylinder-walls; but the consequent differences of temperature are too small to be certainly indicated, especially in the latter parts of the stroke where the possible error is very great. The wire temperature (curve *B*) reaches a maximum at crank-angle 210° C. (30° after in-centre). The wire temperature must here be equal to that of the gas since there is no transfer of heat; it is 263° C. The pressure at this point was found to be 104·5 lbs. per sq. in. absolute, and the volume is 0·333 cubic feet. The mean temperature is therefore

$$\frac{104{\cdot}5}{14{\cdot}7} \times \frac{0{\cdot}333}{1{\cdot}45} \times 310 = 506° \text{ absolute, or } 233° \text{ C.}$$

The temperature calculated on the assumption of adiabatic compression from 14·7 lbs. to 104·5 lbs. is 542° abs. or 269° C. The temperature given by the wire falls, as it should do, between the two limits.

The exhaust-valve opens at crank-angle 310° (50° before out-centre). The pressure falls to near atmospheric at the out-centre, and then rises again during the exhaust stroke. At the out-centre the temperature shown by the wire is 40°, but is subject to a considerable possible error since the wire is still hot from the compression and is cooling rapidly. During the exhaust stroke the pressure rises, and there is a corresponding rise of temperature which continues till near the end of the stroke. The inlet valve opens at crank-angle 515° (25° before the in-centre) and provides an addjtional exit for the air. At the same time, the piston is slowing down and there results a rapid drop in the pressure, and a corresponding fall in temperature due to the expansion.

It appears therefore, that when corrected for lag in the manner described, the temperature of the wire gives that of the gas in its neighbourhood at least as accurately as the latter can be obtained from the indicator diagram. The corrections are, however, very large at certain points, and the temperature correspondingly uncertain; the possible error amounting sometimes to as much as 20° C. This large error is mainly due to uncertainty in the shape of the wire-temperature curve, which gives rise to errors in

the final gas-temperature greatly exceeding the errors of measuremen in the wire-temperature. It is doubtful whether the possible error can b reduced by using a much smaller wire, with a correspondingly smalle correction for time-lag*. There was no reason in these particular experi ments why the wire should not have been 1/1000 in. diameter instead o 4/1000 in. The larger size was chosen because it was intended ultimatel to use it for measuring the suction temperatures when the engine worke in the ordinary way, taking in and firing a charge of gas. But it was foun that even this large wire always fused before any observations could b taken. A still thicker wire might, of course, have been used for the purpose but the correction would then be so great as to make the results valueless It seems clear that in an engine of this size and compression ratio, it is matter of much difficulty to get accurate measurements of temperatur by the platinum thermometer, when the engine is firing. The temperatur in this engine rises in places to 2200° C. after the explosion, and, probably remains above 1710° C. (the melting-point of platinum) for $\frac{1}{300}$ second It is therefore not surprising that even fairly thick wires are melted.

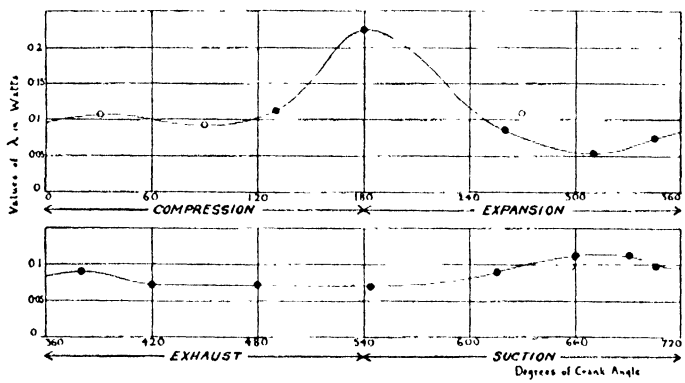


Fig. 4.

This paper may conclude with a short discussion of the value of λ or the rate at which the wire loses heat per degree difference of tempera ture between it and the gas. This quantity is plotted in Fig. 4 in terms o crank-angle. At the top of the compression λ is three times as great a at the latter end of the exhaust. This is, no doubt, mainly due to the fac that the thermal conductivity of the air increases with the temperature The increased pressure also probably has some influence, since we have her to do with air in turbulent motion, and λ is not a function of thermal con ductivity only. During suction λ is distinctly greater than during exhaust probably because of the more violent motion of the air in the former opera tion.

* Even with the somewhat rough methods of measurement that I used, accidental fluctuation in the wire-temperature were quite obvious—due, no doubt, to the eddying motion of the air i the cylinder. Such fluctuations will, of course, appear whatever the size of wire, but will intro duce greater uncertainty, the larger the wire. More accurate resistance measurement would no eliminate them.

It has sometimes been said in connection with attempts to measure gas-temperatures by the platinum thermometer, that the rate at which a fine wire loses heat to a gas per unit length is independent of its diameter. This assertion is based on the assumption that convection currents play an unimportant part. In that case the rate of loss of heat will involve the logarithm of the radius of the wire and will not change greatly with that radius, provided it be small compared to the dimensions of the vessel in which the wire is situated. In experiments such as those here described, however, convection is exceedingly important; and the rate of loss of heat is, in consequence, considerably greater for a larger wire. I recently tried placing two platinum wires, one 2/1000in. and the other 4/1000in. diameter, in a long wooden box 6 inches square, along which a current of air with an estimated velocity of 10 feet per second was flowing. The wires were at right angles to the length of the box and were heated by an electric current, the resistance being measured at the same time. It was found that the 4 mil wire lost heat about 50 per cent. faster than the 2 mil wire at the same temperature. In the case of the larger wire, the rate of loss was 0·0028 watt per centimetre length for every degree Centigrade by which the wire was hotter than the air current at a distance from it, and this relation held good up to a temperature of 600° or so, when the radiation became important. In the suction stroke of the gas-engine the loss is considerably greater, being about $\frac{0{\cdot}11}{29} = 0{\cdot}0038$ watt per centimetre per degree, a result easily explained by the more violent motion of the gas.

How great is the effect of convection is readily seen by comparing the figures just given for the rate of loss with that obtained by calculation from the thermal conductivity of air on the assumption that there is no convection. If a cylindrical wire of radius r, and at temperature θ, be surrounded by a concentric cylinder of radius r_0 at temperature θ_0, the intervening space being filled with air at rest, the rate of flow of heat from the wire per centimetre length will be

$$2\pi k\,(\theta_1 - \theta_0)\,\frac{1}{\log_e \frac{r_0}{r_1}},$$

where k is the thermal conductivity of air. Taking k as 0·00005, r_1 as 2/1000in., r_0 as 2 ins., we get for the rate of flow $4{\cdot}15 \times 10^{-5}$ calories per second, or $1{\cdot}74 \times 10^{-4}$ watts. This is only 1/16 of the rate found in a wire of the same size placed in a current of air flowing about 10 feet per second, the dimensions of the surrounding enclosure being of the same order as a cylinder of 2 inches radius.

I have great pleasure in acknowledging the help that I have received in this investigation from Messrs A. L. Bird and A. R. Welsh, two students at the Engineering Laboratory, Cambridge. They made and reduced the whole of the observations on which the work is based.

18

ON THE INDICATED POWER AND MECHANICAL EFFICIENCY OF THE GAS ENGINE.

[From "PROC. INSTITUTION of MECHANICAL ENGINEERS," 1907.]

IN the Report of the Committee of the Institution of Civil Engineers on the Efficiency of Internal Combustion Engines*, the following remark occurs (page 247):

It would be desirable, but for one circumstance, to calculate the relative efficiency only from the indicator horse-power. But it appears that in the case of gas engines, and especially gas engines governed by hit-or-miss governors, the indicator diagrams do not give as accurate results as is generally supposed. The diagrams vary much more than those of a steam engine with a steady load, and the mean indicator horse-power, from the diagrams taken in a trial, may, it appears, differ a good deal from the real mean power.

This statement is fully borne out by the tests of the Committee which show that the mechanical efficiency taken as the ratio of brake to indicated power varied from 80 per cent. to 94 per cent. in the three engines tested. These engines were of similar type, but of different sizes, and whereas the smallest of 5 H.P. showed a mechanical efficiency of 90 per cent. the intermediate engine of 20 H.P. showed a lower efficiency of 80 per cent. The Committee remarked that these values were obviously incorrect, and the values adopted by them for the mechanical efficiency were obtained by running the engine light and making an estimate of the indicated horse-power under these conditions. Assuming that the mechanical loss is constant at all loads the indicated power at full load can be determined by adding the power absorbed at no load to the brake power. The mechanical efficiencies of the three engines found in this way were respectively:

Engine	L	R	X
Mechanical efficiency	0·86	0·866	0·888

These results are just what would be expected; the mechanical efficiency showing a slight improvement with the size of the engine.

The opinion of the Committee quoted above is obviously important, and may be expected to have a widespread effect in gas engine testing. It throws doubt upon many of the efficiency tests on gas engines which have hitherto been made and published. Moreover, the method which the Committee themselves adopted for getting the indicated power from the brake-power seems to require further investigation before it can be

* *Proceedings of the Institution of Civil Engineers*, vol. 163 (1905–6).

accepted as accurate. It may no doubt be assumed on the evidence of steam engine tests that, under given conditions of lubrication, the friction is practically independent of the pressure in the engine. But whereas in the steam engine the whole of the mechanical losses are to be ascribed to friction, that is not the case in the gas engine in which a considerable amount of power is wasted in pumping and is usually included in the mechanical losses. Moreover, with a given supply of oil, lubrication conditions in the steam engine are practically constant, but in the gas engine that is by no means the case. Great changes can take place in the temperatures of the cylinder walls in a comparatively short time, and this will affect the viscosity of the oil and therefore the work spent in friction. The author therefore determined to undertake an investigation with the object of finding whether the indicator power of the gas engine does, in fact, vary so much and is so difficult of determination as the Report of the Committee referred to suggests. If it were found that the indicated power could be accurately determined directly, it was further desired to test, by direct comparison of brake and indicated power, the validity of the Committee's method of getting the mechanical efficiency. Briefly, the conclusions reached are:

(1) If precautions are taken to keep the pressure of the gas-supply constant, the diagrams given by the engine are remarkably regular, and whether the engine be missing ignitions or not, it is possible, by the use of a sufficiently accurate indicator, to obtain the indicated power from diagrams within 1 or 2 per cent. It seems probable that the difficulty experienced by the Committee was due either to the essential defects, for this purpose, of the ordinary form of indicator, or to casual variations in the gas-supply per suction due perhaps to variation in the gas-pressure at the engine.

(2) The difference between I.H.P. and B.H.P. is rather less than the H.P. at no load under the same conditions of lubrication mainly because of the difference in the power absorbed in pumping. In the particular engine tested by the author the error from this cause in obtaining the indicated power would amount to about 5 per cent. The friction is substantially constant from no load to full load, provided that the temperature of the cylinder walls is kept the same, but the influence of temperature is very great.

The engine used in the tests was kindly placed at the author's disposal by Messrs Crossley Brothers. It is intended to give a maximum output of 40 H.P. on the brake, and the following are the particulars of it:

Cylinder, 11½ inches diameter by 21 inches stroke.
Speed, 180 revs. per minute.
Compression space, 407 cubic inches.
Compression ratio, 6·37.
Compression pressure, 175 lbs. per sq. in. absolute.

When exploding every time, the indicated horse-power at 180 revolutions per minute is equal to the mean effective pressure multiplied by 0·495.

The engine works the ordinary "Otto" cycle, governed by hit-and-miss. The ignition is by magneto. The engine was loaded by belting it to a dynamo (lent by Messrs Mather and Platt), which also served to motor it round when required. The fuel used was Cambridge coal-gas. When an accurate measurement of brake-power was desired all-round rope-brakes were used, one on each flywheel, and as the measurements were such that the brake-tests only lasted a few minutes it was not necessary to use any water cooling. The engine was fitted with an exhaust-gas calorimeter of the spray type.

For measuring the gas-supply a standard holder by Messrs Parkinson and Cowan, having a capacity of 10 cubic feet, was placed between the main gas-supply and the engine, and as close as possible to the latter. In the ordinary running of the engine the holder stood at a constant level, the flow of gas into it just balancing the flow out, and under these conditions it served as a gas-bag, coming down by about 1/10 cubic foot at each suction of the engine. In a measurement of gas-consumption the supply to the holder was cut off, so that the engine took gas only from the holder, and the quantity taken in a definite number of suctions (usually about 50) was noted. The indicator diagrams were photographed at the same time as this measurement was made. After the completion of the measurement the inlet pipe to the holder was opened and the counter-weights adjusted, so that the holder slowly rose to nearly its highest position, when the measurement could, if necessary, be repeated. It was possible in this way to read off the gas-consumption correct to one part in 500, and, allowing for possible inaccuracies in the gas-holder divisions, small changes in temperature and pressure, etc., it may be taken as certain that the gas-consumption given is within one-half per cent. of the truth. This method of gas-measurement is, of course, especially adapted for cases like the present, in which the actual gas used in a particular cycle or series of cycles is desired, but it may be noted that it is almost equally suitable for the measurement of gas-consumption for a long period. It was found that it made no perceptible difference to the power given by the engine whether the inlet-pipe to the holder was open or closed, and it may be assumed therefore that the gas-consumption remains the same under these two conditions. The rate of consumption determined by the holder may therefore be assumed to hold during the intervals when the holder is filling or is standing at a constant level. This method of measurement, which is much superior in accuracy to any meter, and is very convenient, might easily be applied to much larger engines, since all that is required is a holder of capacity sufficient to run the engine for about one minute.

The work of making the tests and reducing the results was done almost wholly by two students of the engineering laboratory, Messrs A. R. Welsh,

of Trinity College, and A. L. Bird, of Peterhouse. To these gentlemen the author must express his thanks. Without their assistance it would have been impossible for him, with the time at his disposal, to have carried through the series of tests which is here described.

The first requirement for the investigation proposed was an accurate indicator. In order to get at all satisfactory results it was necessary to construct an instrument which could be relied upon absolutely to give the indicated power within 2 per cent. Further it was necessary that the instrument should be capable of working for long periods without breaking down, so that large numbers of diagrams could be taken under given conditions. The author's experience of indicating gas engines has convinced him that it is quite impossible to fulfil the first of these conditions, to say nothing of the second, with any form of pencil indicator. It is unnecessary to discuss here the various sources of error in the pencil indicator, but it may be noted that two of them, namely the inertia of the piston and looseness in the joints, are of especial importance in the gas engine. In the engine on which the experiments here described were made the pressure rises on explosion from 170 to 500 or 600 lbs. per square inch in less than 1/100 of a second. Now, the natural period of oscillation of a Richards indicator of the kind made by Casartelli, of Manchester, for gas-engine work, when working with a three-hundred spring is of the same order, that is, about 1/100 of a second. In consequence of this, as is well known, violent oscillations may be set up by the explosion and continue along the expansion line of the indicator. The magnitude of these oscillations depends upon the relation between the time taken by the pressure to rise to its full value and the period of oscillation of the indicator. Thus with certain gas charges there may be practically no oscillation, while with a slightly different mixture the oscillations may become so great as to make accurate measurement of the diagram impossible. The effect of indicator inertia on gas engine diagrams is well known, but the other defect referred to, namely back-lash in the mechanism, has not been fully appreciated though it is fairly obvious. The maximum pressure in a gas engine, in which the compression ratio is 6, is 500 to 600 lbs. per square inch, and it is necessary therefore to use a very stiff spring. Stiffness is also required to reduce the natural period of the instrument, and so to bring within reasonable limits the oscillations due to inertia. In practice the author has found that in the Crosby indicator a spring giving a deflection of 1 inch with a pressure of 300 lbs. per square inch is barely stiff enough. Now, the mean pressure is about 100 lbs. per square inch giving a mean height with this spring of 0·3 inch; slackness in the joints amounting to a total movement of the pencil of 1/100 in. will therefore cause an error in the diagram area of 3 per cent.

The author has examined a considerable number of pencil indicators for back-lash, and has not yet succeeded in finding one, even when perfectly

new, in which it amounts to less than 1·5/100 in., and in very few is it less than 2/100 in., giving an error of 6 per cent. in the diagram area. It is by no means surprising that this should be so, when one considers that in the Crosby indicator motion there are four pin-joints between the piston and the pencil and that the motion is magnified six-fold. Thus a movement of 1/100 in. at the pencil corresponds to only 1/2400 in. at each joint. Another error of the same kind is that due to the deformation of the lever carrying the pencil set up by friction of the pencil on the paper. The combined effect of this and of back-lash is to increase the mean height of the diagram by an amount which, from the nature of the case, is quite uncertain, but which may easily reach 1/30 in. even with a new indicator in perfect adjustment. This error is in most instruments counterbalanced to some extent, and in many overbalanced, by that due to motion of the pencil at right angles to the piston bore.

The pencil motion should, of course, be accurately parallel to the piston motion, but in all indicators looseness in the joints prevents this motion from being perfectly definite. The pencil can move anywhere between two parallel lines, and the friction between the pencil and drum is quite sufficient to make it take either of these. In the diagram, the expansion line and compression line are both shifted inwards and the area and mean pressure reduced. This defect is sometimes rather serious in the Crosby indicator, which in other respects is as good as a pencil indicator can be for gas-engine testing. The author has never found it amount to less than 2/100 in., and the mean pressure taken with this instrument is, in consequence, often too small. Using a three-hundred spring the mean pressure taken from a diagram $2\frac{1}{2}$ inches long from a gas engine compressing to 170 lbs. per square inch absolute, will be $3\frac{1}{2}$ per cent. too small if the horizontal back-lash amounts to 2/100 in. Stretching of the cord caused by drum friction of the kind discussed by Professor Osborne Reynolds acts in the same way. It would appear that on account of these disturbances, in themselves so minute, gas engine diagrams taken with a pencil indicator cannot be relied upon as accurate to within 5 per cent., and the error must often be more like 10 per cent. When the indicator is subjected for any considerable time to the wear and tear involved in recording the explosions of a gas engine with high compression and using heavy charges, its joints rapidly become so slack as to destroy its value for any but the roughest measurements. Indeed it may almost be said that the life of a pencil indicator (as an instrument of precision) when used on such a gas engine is limited to a few hundred explosions.

To overcome both these defects of inertia and back-lash, it is necessary to reduce very much the motion of the moving parts of the indicator and to use optical means for magnifying that motion. The diaphragm manograph first proposed by Perry and now in use to some extent as a commercial instrument in the form of the Hospitallier-Carpentier Manograph, is unsuited for accurate quantitative work for a number of reasons; the

chief of which are that the displacement is not proportional to the pressure, so that the diagrams cannot be integrated by a planimeter and that it is inconvenient to calibrate. The author therefore determined to get a new design of indicator of the piston and spring type with optical magnifying mechanism. In the form finally adopted, after a considerable amount of experimenting, the spring consists of a straight piece of steel strip held as an encastred beam in a steel frame. A piston slides in a bore communicating with the engine, the axis of this bore being at right angles to the spring and passing through its centre. The pressure on the piston deflects the spring and so tilts a small mirror about an axis at right angles to the bore, the pivots of this mirror being carried on a steel frame. To give the other motion to the mirror the whole apparatus (straight spring and mirror with its pivots) is positively connected to an eccentric on the crank axle by which it is rocked about the axis of the bore, thus giving the piston motion of the diagram without the possibility of any lost motion. This instrument is practically indestructible, and it has been left open to the engine for considerable periods without giving it any attention. The vertical deflection is accurately proportional to the pressure, so that the diagrams can be integrated with a planimeter. Finally, the period of oscillation is only about 1/700 of a second with such strengths of spring as were used in the mechanical efficiency tests. The indicator is very easily calibrated by dead weights. The diagrams used in these measurements were photographed, but for many purposes it has been found sufficient to observe them direct by means of a telescopic arrangement by which they are projected as a bright line of light on to a transparent screen with vertical and horizontal scales. It is easy to plot the diagram on to a piece of squared paper, and its area can thus be obtained within 5 per cent. without the trouble of photography*.

Fig. 1, page 232, is a facsimile of a normal diagram taken with this instrument; it represents about a dozen consecutive explosions. In order to determine the accuracy with which the indicated power can be determined, three diagrams similar to this were taken, each comprising twenty consecutive explosions; and these were integrated by separate observers directly from the negatives, each diagram being dealt with by two observers so as to get six independent integrations. In the result the mean of all the measurements was found to be 0·954, the maximum being 0·963, and the minimum 0·940.

This test was considered to have established the accuracy of the instrument subject to the calibration being correct. It also proves that the diagram, when the engine is exploding continuously, is quite remarkably regular. It is certain that in a continuous series of, say, a hundred explosions the diagram area does not vary more than 1 per cent. on either side

* A full description and drawing of the Indicator are given in Appendix I (page 241).

of the mean value. When the governor cuts out charges of gas, however, a slight enlargement of the diagram is sometimes perceptible in the cycle following a miss. Occasionally this enlargement amounts to as much as 4 per cent., while at other times it disappears completely. On theoretical grounds it is to be expected that the gas taken after a miss will be, if anything, slightly less than after an explosion*. On the other hand the efficiency is slightly higher. But whatever the cause of the slight difference in area, it is clear that it can have no practical effect in an ordinary full-load trial, since it only happens once in five or six cycles. It may therefore be taken as definitely established that, given a sufficiently accurate indi-

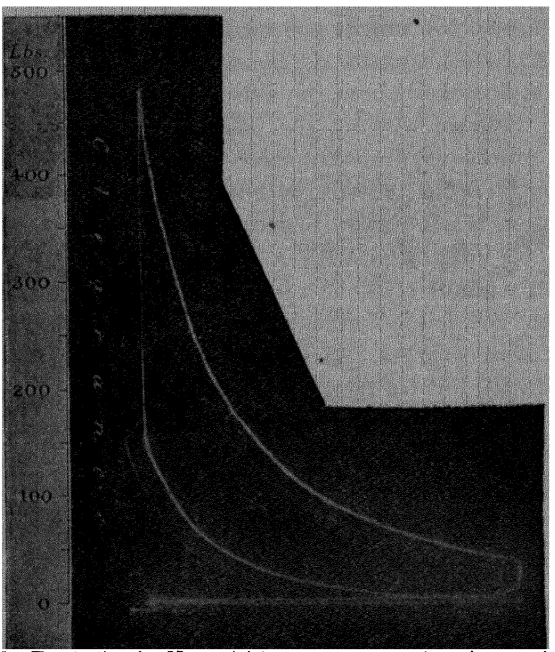


Fig. 1. Facsimile of a Normal Diagram, representing about a dozen consecutive explosions, taken with Indicator, Figs. 4 and 5.

cator and constant conditions, the I.H.P. of a gas engine may be determined from diagrams with an accuracy which is probably superior to that attainable in the steam engine.

The indicated horse-power of the engine is dependent, of course, upon the gas-supply. In the tests described above the amount of gas taken per cycle was, no doubt, very constant, but it is probable that under certain circumstances there may be casual variations of gas-supply which give rise to varying diagrams. The gas taken per suction with a hit-and-miss governor is determined by the temperature of the cylinder-walls and piston and by the opening of the gas-cock. So long as these variables and the

* See Appendix II (page 244).

pressure in the mains are kept constant there is no reason why the gas-consumption should vary. In the trials described in this Paper a gas-holder of considerable capacity was placed close to the engine, and the pressure in the holder was no doubt very nearly constant, but that of course may not be so in all practical tests. The mean pressure in the cylinder during the suction-stroke is about 2 lbs. per square inch below atmosphere, equivalent to about 55 inches of water. A variation of 2 or 3 inches of water in the pressure outside the engine, therefore, gives a substantial change in the gas-supply. Such a variation would naturally occur after a missed ignition, if the gas-bag were not of very ample capacity and placed close to the engine, and this may to some extent account for the very considerably enlarged diagrams which in some engines have been observed to follow a miss.

Changes of cylinder temperature considerably modify the gas-consumption per cycle with the same opening of the gas-cock, but these changes are, of course, comparatively slow and do not affect the accuracy of an estimate of indicated power where the conditions are fairly constant. As an indication of their amount the following test may be cited. The engine was first run light with a large flow of jacket-water; it was then fully loaded and the jacket-water throttled until the temperature at exit rose to 185° F. (85° C.). The load was then suddenly thrown off, and the engine was allowed to run light for a short time with the hot jacket. A large quantity of water was then passed through the jacket so that the temperature of the cylinder rapidly fell to 61° F. (16° C.). The gas taken per suction was measured at intervals with the results shown in Table I (page 234).

It will be noted that the gas-consumption varies between limits differing by 12 per cent. and the indicated power would change in almost the same ratio. The change in the gas-consumption at and following the points marked * is mainly due to the change of temperature of the piston which very rapidly follows any change of load.

It will be desirable to give a statement of the meanings which will be attached in this Paper to the terms "Indicated Power," "Mechanical Loss," etc.:

Indicated Power is the area of the positive loop of the indicator diagram multiplied by the number of explosions per minute and by the appropriate constant for reducing to horse-power.

Mechanical Loss is the difference between indicated power and the brake power delivered at the circumference of the flywheels. It includes the negative loop of the working diagrams and also the negative work done when the engine takes no gas.

Table I.

Jacket temperature (outflow)	Load	Gas (cu. ft. per suction)
58–65° F. (14–18° C.)	Nil	0·1217
*72° F. (22° C.)	Full	0·1124
114° F. (45° C.)	,,	0·1103
185° F. (85° C.)	,,	0·1072
*196° F. (91° C.)	Nil	0·1117
185° F. (85° C.)	,,	0·1141
179° F. (81° C.)	,,	0·1159
61° F. (16° C.)	,,	0·1205

Mean Effective Pressure is the mean pressure calculated from the positive loop of the indicator diagram. It is sometimes convenient to speak of the "mean pressure effective on the brake," and this is equal to the mean effective pressure multiplied by the mechanical efficiency.

Mechanical Efficiency is, as usual, the ratio of brake to indicated power (the latter being defined as above).

Thermal Efficiency is the ratio of indicated power to the lower calorific value of the gas used, the two quantities being, of course, reduced to the same units.

A large number of tests were made with the object of determining the mechanical losses by finding the difference between indicated power and brake power, and of comparing the result with that obtained from running the engine light under the same conditions. It will be well to describe one of these tests in detail and to summarize the results of the others. The engine was run with a fairly full load, missing about one explosion in five cycles. The load was applied by means of rope brakes and, as the test only lasted a short time, it was unnecessary to employ any water cooling. While careful observations were taken of the brake load (the difference between a dead weight and a spring balance) three photographs of indicator diagrams were taken, each photograph covering about a dozen explosions. At the same time the misses occurring in about a couple of hundred cycles were counted, so that the ratio of missed ignitions to cycles was accurately known. These observations gave all the data for the mechanical efficiency, no observation of speed being necessary; the speed was, however, kept approximately constant by the governor at 180 revolutions per minute, and in what follows brake power and indicated power are both calculated on the assumption that that was in fact the speed of the engine. The following are the results of measuring the three photographs together with the observations taken at the same time as the photographs:

Table II.

Jacket; exit temperature	Explosions / cycles	M.E.P. from diagram	Gas per suction	I.H.P.	B.H.P.
150° F. (65° C.)	0·804	100·3	0·1196	39·7	34·0
—	0·82	99·4	0·1182	40·2	34·6
160° F. (71° C.)	0·825	99·0	0·1164	40·2	34·9

The gas-charge was measured, as described above, by means of a standard holder, and is correct to within one part in two hundred. It will be seen that the mean pressures from the three diagrams show very good agreement, being all within 1 per cent. of the mean value after allowing for the small change in gas-charge. The mean of the three observations of brake horse-power is 34·5. The mean indicated power is 40·0, giving a mechanical efficiency of 86·2 per cent. The difference between brake and indicated horse-power ranges from 5·7 to 5·3 H.P., the mean being 5·5 H.P. In the course of the test the jacket temperature rose slightly, and, as will shortly appear, this is sufficient to account for the small diminution in mechanical losses observed in the course of the test.

Immediately after the completion of the above full-load tests the brakes were taken off and the engine run without load, the jacket-water being throttled so as to prevent the engine from cooling down rapidly. Photographs were at once taken of the indicator diagrams, the gas-consumption was measured and the ratio of explosions to total cycles was ascertained to be 0·141. The mean pressure was found to be 105·5 lbs. per square inch, the gas-consumption 0·1252, giving I.H.P. 7·35, or about 1·85 H.P. more than the difference between the I.H.P. and the B.H.P. in the full-load tests. Had the engine in the latter case been firing every time instead of four times out of five, this difference would have been increased in the ratio 5/4, becoming 2·3 H.P.

This result, that is, that the power taken to turn the engine round at light load is rather over two horse-power more than the mechanical losses at full load, which was verified on many occasions, is due almost wholly to the difference in power absorbed by the pumping strokes of the engine at light load and at full load. This difference appears in Fig. 2, which shows the suction loop in the two cases. The full line is the diagram of a cycle in which no gas is taken, the dotted line shows the negative part of a diagram of a cycle in which gas is taken. It will be seen that, as usual in these engines, the exhaust line after an explosion is considerably lower than when the engine is simply exhausting a charge of air. Moreover, when the engine takes gas the pressure at the middle of the suction stroke is about a pound higher than when the engine is taking air only—no doubt because of the less restricted opening in the latter case.

The mean pressure of the full load suction loop is 2·9 lbs. per square inch, corresponding to 1·4 H.P. at 180 revolutions, whereas in the other case the pressure is 5·0 lbs. per square inch, giving 2·5 H.P. In addition to this a certain amount of work is done in the compression and expansion of the charge of air which occurs when no gas is admitted, the compression line being rather higher than the expansion line. It is difficult to show the whole of this work area on a diagram, because, if the spring of the indicator is sufficiently stiff to take the maximum compression pressure of 170 lbs. per square inch, it is hardly sufficiently sensitive to record the small difference of pressure between the expansion and compression strokes. From a

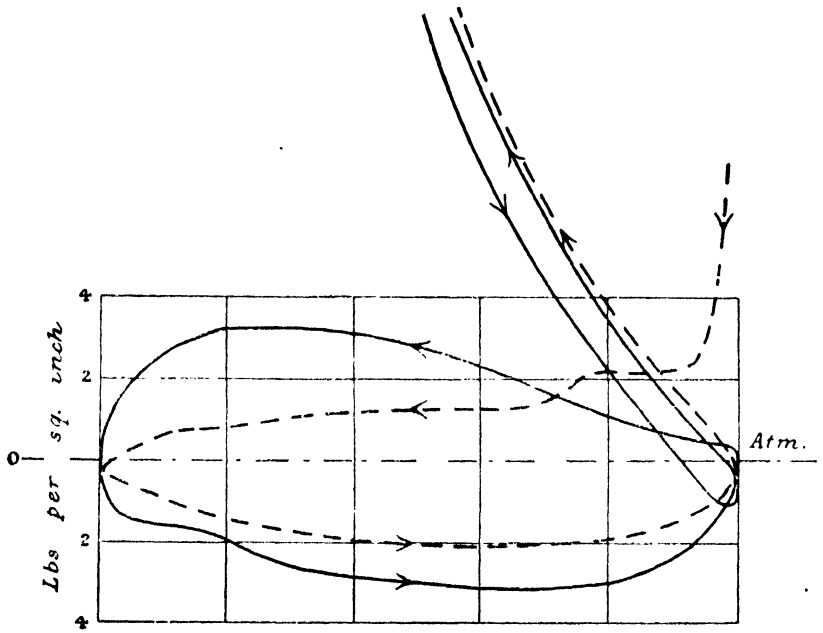


Fig. 2. Light spring diagram, showing power absorbed during light-and full-loads. Full line shows diagram of a cycle in which no gas is taken. Dotted line shows the negative part of a diagram of a cycle in which gas is taken.

good many observations of diagrams with different strengths of spring*, however, it would appear that the mean pressure of this part of the diagram is certainly between 1·5 and 2·5 lbs. per square inch. On this account, therefore, 1·0 H.P. must be added to the negative work done in each cycle in which no gas is taken, making approximately 3·5 H.P. at 180 revolutions per minute. This is 2·1 H.P. more than the power represented by the negative loop when the engine is firing every time, and is therefore just about sufficient to account for the difference between the full load mechanical loss and the light load indicated power.

An independent estimate of the power lost in pumping was made by

* See Appendix III (page 248).

belting the engine to the dynamo and motoring it round without allowing it to take gas. Two measurements were made, one immediately after the other; in one the engine was closed up as usual, in the other the exhaust-valve cover was removed so that there was no pressure in the cylinder. Assuming that the belt losses and friction losses are the same in the two cases, the difference between the powers absorbed by the dynamo (after deducting dynamo losses) should be equal to the power absorbed in pumping. Two tests gave the following results, the jacket temperature in both cases being 180° F. (82° C.):

	24 Aug.	25 Aug.
Engine closed ...	7·72	7·1
Engine opened ...	4·14	3·77
Difference	3·58	3·33

The mean is 3·45 H.P. against 3·5 H.P. estimated from indicator diagrams.

In comparing the mechanical loss with the light load indicated power, care was taken that the conditions of the engine as regards lubrication and cylinder temperature were as nearly as possible the same. It was found that these factors make a very great difference to the engine friction, and it is important to allow for this in basing any estimate of indicated power upon the power absorbed by the engine at light load. This point is strikingly illustrated by an experiment which was made immediately after one of the tests for mechanical efficiency. At the end of that test the jacket temperature was 179° F. (81° C.), the engine was absorbing about 7 H.P., exploding once in seven cycles, and the ordinary normal lubrication was going on. The gas-consumption was 0·1159 cubic foot per suction. With the engine running under these conditions and without stopping it or altering the conditions in any other way, the jacket-water, which had previously been shut off, was turned fully on, so that in a few minutes the jacket temperature had fallen to 61° F. (16° C.). The gas-consumption was then found to have risen to 0·1205 cubic foot per suction, or by about 3 per cent., and the engine was taking gas once in 4·85 cycles. Thus the power absorbed had increased, in consequence of the reduced cylinder temperature, to 10·0, or by about 40 per cent. on its value with the hot cylinder. A considerable quantity of water was then injected into the cylinder through the small suction valve which is fitted for the purpose of preventing pre-ignitions. The result of this was an immediate drop in the power absorbed to about 6 H.P., or rather less than the engine was using with the hot jacket and with the normal lubrication. This very great variation in the power absorbed by the engine in friction must, of course, be within the experience of many people who have worked practically with gas engines, and it is referred to here mainly because it is so important a factor in the testing of these engines for mechanical efficiency. In the full-load test immediately preceding this experiment the brake power was 36·2, and if the indicated power had been calculated by adding to the brake power the horse-power absorbed at no load, the result might have been anywhere

between 43·2 and 46·2 according as the light-load tests were done with a cold or with a hot jacket. The I.H.P. was determined by measurement of the diagram to be in fact 41·2.

Confirmation of the great variation of power absorbed with the condition of the cylinder was obtained by motoring the engine round. The exhaust-valve cover was taken off the engine so that the cylinder was open to the air, and there was no loss from pumping. The power taken to drive the dynamo was measured electrically, and the dynamo losses were deducted, so that the results given below are the total frictional losses in the engine plus the loss in the belt (probably about 0·5 H.P.).

Engine hot (about 180° F., 82° C.).	Normal lubrication,	Power absorbed 4 H.P.
Engine cold (70° F., 21° C.)	... ,, ,,	,, ,, 6·5 H.P.
Engine cold (70° F.)	... Excess of oil	,, ,, 4·7 H.P.
Engine cold (70° F.)	... Water injected	,, ,, 2·7 H.P.

A separate determination of the power required to drive the engine round with the piston removed was made, and it was found to be 1·4 H.P. This includes the main bearing friction, valve-lifting and belt losses. If this be deducted from the figures given above, the result is the piston friction, which, of course, is alone affected by the changes in cylinder temperature and lubrication. It will be seen that the piston friction varies according to conditions between 1·3 and 5·1 H.P., the normal value with jacket at 180° F., being about 2·5 H.P.

In normal working at nearly full load (41·0 I.H.P.) with the jacket at 180° F., the mechanical losses in this engine may be allocated as follows:

Table III.

Suction	1·4 H.P.	3·4% of I.H.P.
Piston friction	2·5 H.P.	6·1% of I.H.P.
Other friction (valve lifting, etc.)	1·1 H.P.	2·7% of I.H.P.
Total	5·0 H.P.	12·2% of I.H.P.

The apportionment between piston friction and other friction is somewhat uncertain, as it may be to some extent affected by the existence of pressures in the cylinder, the estimate of piston friction being based upon running the engine with the cylinder open to the air.

The effect of using water as a lubricant when the cylinder is cold is, of course, easy to understand, though the author hardly expected to find that it was so great in amount. It is not quite so marked, though still quite perceptible, when the engine is running fully loaded. In one test, in which the engine was fully loaded and the jacket-water at a temperature of about 180° F. (82° C.) at exit, it was found that injection of water in considerable quantity diminished the number of explosions per minute by about 3 per cent. with the same brake load and speed. This difference

is due entirely to the change of lubrication, the water having no effect whatever upon the indicator diagram.

The following Table gives the mechanical loss in this engine observed under different conditions. The lubrication is in each case normal, and the loss is the difference between the observed brake power and the indicated power. The engine was nearly fully loaded in every case, the proportion of idle cycles varying from 0·17 to 0·20.

Table IV.

Date	Jacket temperature	Loss (H.P.)
16 August, 1906	150–160° F. (65–71° C.)	6·0
22 August, 1906	185° F. (85° C.)	5·0
22 August, 1906	185° F. (85° C.)	4·9
31 January, 1907	69° F. (20° C.)	7·1
31 January, 1907	150–160° F. (65–71° C.)	5·5
31 January, 1907	203° F. (95° C.)	4·5

The results show the effect of jacket temperature. The slight reduction in loss as between the August and January experiments is to be expected because the engine was run a good deal in the interval.

The following is a summary of the various efficiency figures relating to this engine:

Table V.

Thermal efficiency	33½% to 37%, according to strength of mixture*.
Mechanical efficiency for medium charge	85% to 90%, according to jacket temperature
Air-cycle efficiency	52·2%
Efficiency relative to air-cycle	0·64 to 0·71

It may be interesting in this connection to give the results of a series of tests for mechanical efficiency which were made upon a gas engine of a very different type; that is, a four-cylinder petrol motor running up to 1200 revolutions per minute. The engine was kindly lent to the author by the manufacturers, the Daimler Co., and has cylinders 3·56 inches diameter with a stroke of 5·11 inches. The mechanical losses were determined by the method described by the author in a Paper in *Engineering* (8 Feb. 1907)†. The method consists in running one cylinder only of the engine and of indicating that cylinder, there being no load on the engine. The indicated power of the single cylinder is then equal to the mechanical friction

* The weaker mixtures give the higher efficiency. Full details of the tests establishing this fact will be given in a subsequent paper. See page 263.

† See page 199.

of the engine plus the negative work shown on the diagrams of all the four cylinders. When the engine is running fully loaded at the same speed, the loss by mechanical friction will be substantially unaltered, but the suction losses will not be quite the same owing to the same causes that operate in the case of the large gas engine. The suction loops in the three idle cylinders are different from those in the firing cylinder, and there is, moreover, the negative work done in the compression and expansion. The pumping losses, however, in the Daimler engine bear a very much smaller proportion to the whole than in the large gas engine, owing to the relatively great

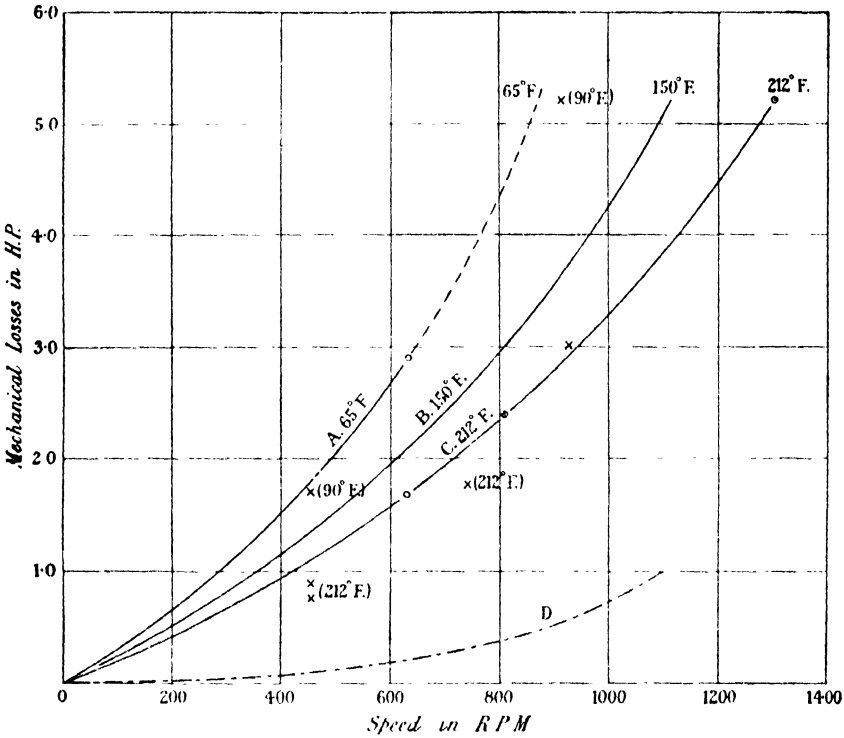


Fig. 3. Mechanical efficiency of Daimler engine with varying temperatures of water-jacket.

size of the ports, so that no serious error is involved in neglecting the difference in these losses as between firing and not firing. Fig. 3 shows the relation between the power indicated by the single cylinder and the speed for three different temperatures. In curve *A* the outlet temperature of the jacket-water was 65° F. (18° C.), in curve *B* about 150° F. (65° C.), and in curve *C* the water was just boiling. The dotted curve *D* shows the power absorbed in pumping, estimated from light spring indicator diagrams; so that the difference between this curve and any one of the others gives an approximation to the loss by mechanical friction. These curves show that the frictional losses at a temperature of 65° F. are nearly double those when the jacket-water is boiling. They also show that the losses increase

very much more rapidly than in proportion to the speed, as is to be expected from the fact that they are due to fluid friction. These experiments were done for the author by Mr L. G. E. Morse, of King's College, Cambridge.

Mr Morse devised another method of getting the mechanical efficiency of a multi-cylinder motor, which, as it is very simple and appears to be quite accurate, is worth giving here. It consists in running the engine loaded with all the cylinders working. The load is put on by means of a Prony brake clamped to the flywheel, carrying a dead weight which is partially supported by a spring balance having an open scale. The spring balance reads the excess of the dead weight over the brake load in the usual manner, and small changes in the brake load may be very accurately read. In making a test one cylinder is stopped from firing by cutting off the current, and the pressure on the brake-blocks is reduced until the speed has come up to its old value. The reduction in brake load is then read off, and is approximately that corresponding to the indicated power of the cylinder which has been cut out. The four cylinders are treated in succession in this way, and by adding the results the indicated power of the engine is determined. Points obtained by this method with the corresponding jacket temperatures are shown by crosses ($\times$) on Fig. 3, and it will be seen that they agree very well with those obtained by indicating only one cylinder.

The mechanical efficiency of this engine is remarkably high for its size. With boiling jacket-water the mechanical efficiency is 90 per cent. at a speed of 400 revolutions per minute. It falls off to 75 per cent. at a speed of about 1300 revolutions per minute. The thermal efficiency is also high, reaching over 26 per cent. under the most favourable conditions. The air-cycle efficiency corresponding to the compression ratio 3·85 is 41·5 per cent., so that the relative efficiency is 0·625.

APPENDIX I.

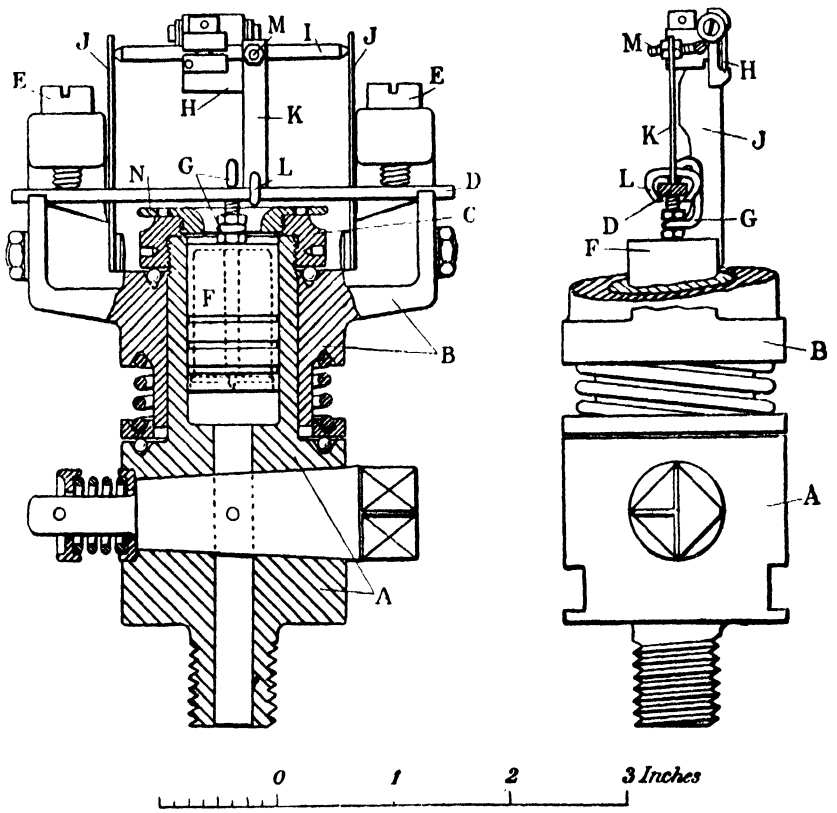
Description of Indicator.

Fig. 4 is a drawing of the instrument, partly in section. The block A is screwed into the ordinary indicator hole of the engine. The frame B fits over the block, sufficient clearance being left to provide for unequal expansion. The frame is held up by a spring into engagement with the lower face of the nut C (screwed to the top of A), a ball-race being interposed so as to admit of easy rotation of the frame about the axis of A.

The spring D is a piece of steel strip resting in grooves at the end of the frame B, and held by the screws EE. The spring is slightly bowed before insertion in the frame so that when the screws EE are screwed home, the spring is held straight with slight pressure on the four points of support.

The piston F slides in a bore in the block A. It is made hollow, but a plate closes its lower end. At the top it is provided with a hook G, the opening of which is slightly larger than the thickness of the spring. The piston is thus free to move laterally, and no binding action is possible between it and the sides of the bore such as would occur if the piston were rigidly attached to the spring.

The mirror H is clamped to a steel spindle I, the ends of which are pivoted in small holes in the vertical spring checks JJ. The motion of the



Figs. 4 and 5. Optical Indicator used in the experiments.

spring D is communicated to the spindle and mirror by means of the vertical piece of spring K. The lower end of this piece of spring is held firmly on the face of the main spring D by means of the jaws L; the upper end is firmly clamped to the arm M which projects at right angles from the mirror spindle. (*See* Fig. 5, which is a section in a plane at right angles to Fig. 4.)

The spring K, while sufficiently rigid to transmit the motion of the main spring to the end of the arm M without buckling, is flexible enough to allow for the angular motion of that arm. The mirror is thus turned about

the axis of the spindle by an amount which is proportional to the displacement of the main spring D, and therefore to the pressure under the piston.

In order to give the other motion to the mirror, the frame B is positively connected by linkage to a reciprocating part of the engine, and is thus caused to rock as a whole about the axis of the block A. The motion thus given to the frame B must be in phase with and proportional to the piston motion. In consequence of the smallness of the angular motion (about

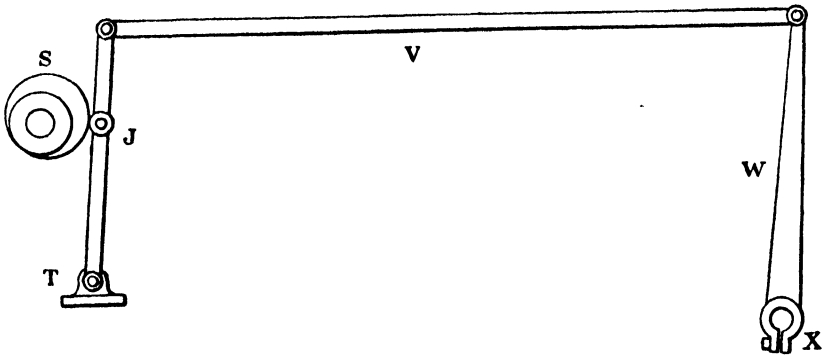


Fig. 6. Diagram of Levers for operating the Indicator Mirror.

1/16th of a radian) of the frame B, it is easy to secure this result with simple gearing, the effect of the curvature of the paths of the joints in the linkage being negligible. A convenient arrangement is that shown diagrammatically in Fig. 6, in which S is an eccentric fixed to the crank-shaft, and such that the diameter is to the throw as the crank-radius is to the length of the connecting-rod. The lever W is jointed at one end to the rod V, which takes its motion from one end of a lever pivoted at the other end T, and engaging with the eccentric by a roller J. At the other end W is fixed to a clamping ring X, which fits over the turned portion of the frame of the indicator, Fig. 4.

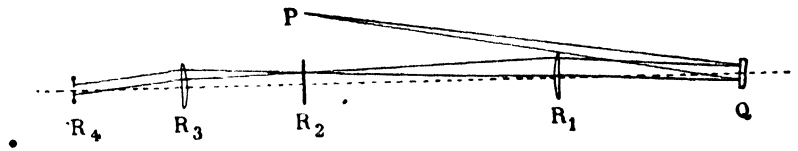


Fig. 7.

The manner in which the diagrams are made visible is apparent from the diagram, Fig. 7.

The source of light is a fine hole P illuminated by an ordinary four-volt incandescent lamp. The rays from this, after reflection from the plane mirror of the indicator Q, fall upon the convex lens R_1, which is placed about 18 inches from the mirror and is 4 inches in diameter. The focal length of this lens is about equal to its distance from the mirror, and the beam of reflected light is therefore refracted to a direction about parallel

with the axis of the lens. The beam comes to a focus in the plane R_2, and traces out in that plane the indicator diagram of the engine, the vertical displacements of the spot (corresponding to the tilting of the mirror about the spindle) being proportional to the pressure, and the horizontal displacements (corresponding to the rocking of the indicator frame) to the piston motion. With a sufficiently powerful source of light this diagram could be made visible by a ground-glass screen placed in the plane R_2, but in order to render it visible with a small lamp, a transparent screen is used and a second lens R_3 is interposed about 10 inches from R_2. This lens is of equal diameter with R_1, and refracts the beam of light (which before striking it is parallel to the axis) to its principal focus R_4 where the eye is placed. The beam is at the same time converted into a parallel beam, and the spot is seen sharply defined on the screen. The diagram traced by the spot is seen as a bright line of light. The screen is engraved with horizontal and vertical lines, on which the diagram is projected. The pressure at any point can thus be easily read off, or the diagram can be plotted down on squared paper.

Three pistons are used, the areas being in the ratio of 1, 2 and 4. There are two springs, which are ground so that the stiffnesses are in the ratio of 1 to 5. A wide range of sensibility is thus obtained. The smaller pistons fit inside liners which are inserted in the bore of the block A, Fig. 4. The spring is very easily changed by slacking the screws EE and slipping the springs out together with the spindle and mirror, the spring cheeks JJ being slightly separated to allow of the spindle being taken out. When the spring has been removed the piston can also easily be taken out by removing the cap N, Fig. 4, which serves also as a stop to prevent excessive bending of the spring.

The indicator is very easily calibrated by dead-weights. The calibration is found to remain constant within 1 or 2 per cent. when the spring is removed and replaced. Suction pressures are registered on the same scale as those above atmosphere, on account of the slight initial set in the spring.

APPENDIX II.

Conditions determining the Gas and Air taken per suction.

The total quantity of mixture drawn into the engine per suction is determined mainly by the temperature of the cylinder-walls and piston, and is not affected much by the temperature of the gas left in the compression space from the previous stroke. This is most easily seen by supposing the in-coming charge to be kept adiabatically separate from the residual gas. The total charge then drawn in will obviously be independent of the temperature of that gas. If now at the end of the suction stroke, when all valves are closed, the in-coming charge and the residual gas are mixed, the pressure will not change. For, the internal energy of a given

volume of gas having constant specific heat is a function of its pressure, and is independent of its temperature or of the distribution of temperature in it. It can make little difference whether the mixing takes place at the end of the stroke, as supposed, or during the stroke, as in fact it does. In either case the quantity of gas drawn in is nearly independent of the temperature of that with which it mixes. The author believes that this was first pointed out by Mr Dugald Clerk, in the Appendix to his Paper on the "Limits of Thermal Efficiency in Internal Combustion Motors*."

It follows that in an engine governing by hit-and-miss, the amount of gas and air drawn in will be substantially the same after a miss as after an explosion. The ratio of gas to air in the charge taken in, and the quantity of gas, will also be much the same, provided that the opening of the gas-cock and the pressure outside be the same. Since, however, the incoming charge mixes with cold air instead of hot products, the weight of the total charge present in the cylinder at the end of suction will be greater, its temperature will be lower, and the percentage of gas present in it will be less. The diagram should therefore be unaffected except in so far as the weakening of the mixture improves the efficiency, and so enlarges the area. This should cause an enlargement of from 4 to 5 per cent., or considerably more than has usually been observed in the engine used for the tests described in this Paper. The inference is that the gas-supply is, in fact, rather less after a miss than after an explosion†.

Fuller investigation of the matter shows that this must be so. Let

v = quantity of gas left in compression space;
t = temperature of gas left in compression space;
V = quantity of gas and air taken in;
T = temperature of gas and air taken in before entering;
v_0 = volume of cylinder contents at completion of suction;
t_0 = temperature of cylinder contents at completion of suction;

the quantities of gas being expressed in standard cubic feet‡ and the temperature on the absolute Centigrade scale.

Then at the commencement of suction we have V standard cubic feet of air and gas outside the engine at temperature T, and v inside at temperature t, all at atmospheric pressure p_0. Thus the stuff which is ultimately to form the total charge has at this point the energy $k\,(VT + vt)$, where k is the thermal capacity at constant volume or 19 foot-pounds.

When suction is finished the stuff is all inside the engine, and it then has the energy $k\,(V + v)\,t_0$.

* *Proceedings of the Institution of Civil Engineers*, vol. 169 (1906–7), p. 148.

† More recent tests have shown that the combustion of the charge following a scavenging stroke is sometimes incomplete, and this may to some extent account for the fact that the diagram is not enlarged. [March, 1908.]

‡ That is, cubic feet reckoned under the standard pressure of 760 mm. of mercury (14·7 lbs. per sq. inch) and the standard temperature of 0° C.

During suction work is done by the engine to an amount W, which can be calculated from the light-spring diagram. Some heat H is also received from the cylinder walls. Some of this work goes to increasing the internal energy, and some is done against the atmospheric pressure. The latter item is equal to the pressure multiplied by the increase of volume of the stuff, that is to

$$\left\{\frac{V(t_0 - T)}{273} + \frac{v(t_0 - t)}{273}\right\} p_0,$$

p_0 being the atmospheric pressure in lbs. per sq. foot.

Equating the net work done on the stuff plus the gain of heat to the increase of energy, we get

$$W - \left\{\frac{V(t_0 - T)}{273} + \frac{v(t_0 - t)}{273}\right\} p_0 + H$$
$$= k(V + v)t_0 - k(VT + vt).$$

Or, since $k + \frac{p_0}{273} = k_p$ the specific heat at constant pressure:

$$W + H = k_p\{V(t_0 - T) + v(t_0 - t)\}.$$

Now $\frac{(V + v)t_0}{273} = v_0$, whence, substituting for t_0

$$VT = -\frac{W + H}{k_p} + 273\, v_0 - vt.$$

If V' be the actual volume (before entering) of the gas and air taken in, so that $V' = \frac{T}{273} V$, and if v_s be the stroke volume $\left(= v_0 - \frac{tv}{273}\right)$, the last equation may be written:

$$V' = v_s - \frac{W + H}{k_p \times 273},$$

or the actual volume taken is less than the stroke volume by the quantity

$$\frac{W + H}{k_p \times 273}.$$

The hotter the contents of the compression space, the less heat will be taken in from the cylinder walls during suction. The charge taken in after an explosion will therefore exceed that taken in after a scavenging stroke, the cylinder and piston temperatures being the same.

It is assumed in this calculation that the pressure is atmospheric at the beginning and end of the suction stroke. With good valve action this is sufficiently nearly the case; and when it is not so it is easy to make the necessary correction. A further assumption is that the whole entering charge comes in from the air- and gas-pipes. This is not quite true, since in most engines the exhaust-valve remains open for some time after the suction stroke has begun and some stuff may back in from the exhaust-pipe.

In the engine on which these experiments were made the mean pressure in the suction stroke when taking both gas and air is 2 lbs. per square inch. Thus $\frac{W}{k_p \times 273}$ is $\frac{2 \times 144 \times v_s}{26{\cdot}75 \times 273} = 0{\cdot}04\, v_s$. The volume drawn in is therefore reduced to 4 per cent. below the stroke volume on account of the work done on the charge in drawing it in. The substantial correctness of this result has been verified by actual measurement of the cylinder contents at the beginning and end of the suction stroke, the engine being motored round and sucking air only, so that there was but little exchange of heat between the gas and the cylinder-walls. The temperature of the charge at each end of the suction stroke was measured by a platinum thermometer*.

The value of H cannot, of course, be calculated, but it is not difficult to measure the actual volume of air taken by means of an anemometer. Tests of this kind under various conditions were made on the Crossley engine, of which the following are the most important. In each case λ is the ratio of the total volume of gas and air taken in per suction (V') to the stroke volume (v_s). The stroke volume is 1·26 cubic feet.

(1) Engine motored round without firing, gas-cock shut, $\lambda = 0{\cdot}87$.

(2) Engine motored round without firing, gas-cock open, $\lambda = 0{\cdot}90$.

(3) Engine firing 80 per 100 cycles. Jacket 62–69° F. (16–20° C.), $\lambda = 0{\cdot}87$.

(4) Engine firing 87 per 100 cycles. Jacket 173–200° F. (78–93° C.), $\lambda = 0{\cdot}825$.

In No. (2) H cannot have amounted to much, as the jacket and piston were both cold. About half of the 10 per cent. loss of volume is to be ascribed to the work done (W) as already explained; the other half is due to gas coming in from the exhaust-pipe during the first portion of the suction stroke. In No. (3) the piston was hot because of the continual firing; it would have a temperature of at least 662° F. (350° C.). Thus H is considerable, and the result is seen in a reduced charge as compared with (2). In No. (4) H is further increased, and the charge is further reduced, by the hot jacket. It was found in these tests that the ratio of gas to air taken in varied somewhat, though the opening of the gas-valve and the gas-pressure were always the same. The range of variation amounted to about 5 per cent. on the ratio, and it could not be correlated definitely with any change in conditions. It takes place slowly, however, and does not affect the conclusion stated above as to the effect of a missed ignition on the charge of gas taken. It is probably due to changes in the temperature distribution in the engine, which would alter the relative amount by which the gas and air streams are warmed in coming in.

* "On the Measurement of Gas engine Temperatures," *Philosophical Magazine*, Jan. 1907. Page 214 of this volume.

The anemometer was calibrated by motoring the engine round, the setting of the exhaust-valve being altered so that it opened at the out-centre and closed a little before the in-centre. The overlap of the exhaust and inlet-valves was thus reduced to about 7° only, and the error due to air coming in from the exhaust pipe was practically eliminated. Measurements of temperature (by platinum thermometer) and of pressure were made at the opening and closing of the inlet-valve, and the cylinder contents at these points were thus determined. The calibration so made was accurate within 2 or 3 per cent. A somewhat similar method was used by the Institution of Civil Engineers Committee; but they did not make any measurement of temperature. On this account their calibration was probably somewhat too high; perhaps as much as 5 per cent. For their purpose this was near enough, since they only required the total volume of air passed through the engine for the purpose of estimating the amount of a small correction in their heat-quantities.

APPENDIX III.

On the Loss of Heat in Compression.

In this engine the pressure reached at the end of the compression-stroke, when compressing air only, is 173 lbs. per square inch absolute, with a normal barometer. The pressure at the closing of the inlet-valve under the same circumstances is almost exactly atmospheric, and the volume is then 6·2 times that of the compression space. Assuming that the compression curve is of the form pv^n = constant, the index n has the value 1·35. As a matter of fact, n is greater at the beginning of compression than at the end; for the first half of the stroke the compression is almost adiabatic, and n is practically 1·41; and for the second half n averages 1·31. The mean pressure of compression is 48·5 lbs. per sq. inch.

On the expansion curve the average value of n for the first half is 1·33 and for the second half 1·41, the average for the whole stroke until the exhaust-valve opens being 1·37. The exhaust-valve begins to open when the volume is 5·75 times the clearance, and the pressure is 15·7 lbs. per square inch. The mean pressure on the expansion-stroke is 46·5 lbs. per square inch, and the mean difference of pressure between the two curves over the whole stroke is 2 lbs. per square inch.

The above figures are derived from measurements of photographed diagrams, with different strengths of spring, the engine running light, so that most of the diagrams consisted of compression and expansion of a charge of air. The pressures in the first half of the stroke are accurately known, because an open scale can be used. In the other half the scale used is about 50 lbs. per square inch to the inch, and the difficulty of accurately measuring the difference between the two curves becomes considerable. The probable error in the mean difference does not, however, exceed ½ lb. per square inch.

It is interesting to estimate the percentage of the work done in compression which is lost as heat. This can readily be done from the above data. Assume that at the commencement of compression the cylinder volume is 1 cubic foot. The pressure is then atmospheric: the absolute temperature, say θ_0. In the compression the volume is reduced by the ratio $\frac{5{\cdot}2}{6{\cdot}2}$, the mean pressure is 48·5 lbs. absolute, and the work done is therefore

$$\frac{5{\cdot}2}{6{\cdot}2} \times 48{\cdot}5 \times 144 = 5850 \text{ foot-pounds.}$$

If there were no loss of heat, this amount of work would produce a rise of temperature of $\frac{5850 \times \theta_0}{19 \times 273}$ or $1{\cdot}11\theta_0$. The absolute temperature at the end of compression from the indicator diagram is $\frac{173}{14{\cdot}7} \times \frac{1}{6{\cdot}2} \times \theta_0 = 1{\cdot}90\theta_0$, so that the rise of temperature actually produced is $0{\cdot}90\theta_0$, or about one-fifth less than that corresponding to the work done. The loss of heat is therefore one-fifth of the work done in compression, and of this loss nearly the whole takes place, as might be expected, during the latter half of the stroke. When the engine in compressing a cold charge of air, θ_0 is about 313, the time-average of the temperature during the first half of compression is about 90° C., while during the second half it is 290° C. The average excess of the gas temperature over that of the walls will be four times as great in the second half as in the first.

During the first half of the expansion stroke the air takes in heat; the second half is nearly adiabatic. It is at first sight somewhat surprising that the air should be taking in heat, though its mean temperature is from 150° to 300° C. above that of the walls. The explanation is that the temperature is not uniform; the air in contact with the walls has the wall-temperature throughout the whole process of compression and expansion. The compression and expansion of the surface-layer of air will approximate to the isothermal—heat will be lost during compression and regained to an almost equal amount during expansion. On the other hand, in the central core of air both compression and expansion will be nearly adiabatic. Intermediate layers will, on the whole, lose heat during expansion as well as in compression, but the loss from this cause may be, and in fact is, over-balanced by the heat flow from the walls into the cool surface layer. The author's experiments with a platinum thermometer* support this view; they show that the central temperature is much above the mean. The total heat recovered during expansion, calculated from the diagram in the same way as for compression, is equal to about 15 per cent. of the work done; or rather more than two-thirds of the heat lost in compression.

In introducing the paper on Friday October 18, 1907, Professor Hopkinson said that the paper before the meeting was the first instalment of the experimental work which had been carried out during the last eighteen months at

* *Philosophical Magazine*, January, 1907. Page 214 of this volume.

the Engineering Laboratory of Cambridge University, upon an engine which had been lent by Messrs Crossley Brothers for the purpose. The paper dealt mainly with the experimental methods used in the research, and it was originally presented to the Council in the form of a preface to an account of some of the results obtained. He had not himself regarded it as of sufficient importance to present alone, but the Council had expressed a desire to postpone the consideration of the results until the publication of the report of the Gas Engine Research Committee, and had asked him to give the first part of the paper by itself. The paper as it stood must be regarded as a justification of the methods employed in order to obtain results of more general interest, which he hoped to have an opportunity of presenting to the Institution at a later date.

Professor Hopkinson then proceeded to draw attention to the more important points in the paper. Dealing with the inaccuracies of the pencil indicator, he confirmed the remarks in the paper by exhibiting the following table, the figures in which were taken from the Institution of Civil Engineers' Report on the Efficiency of Internal Combustion Engines:

Table VI.

Engine	I.H.P. from indicator	I.H.P. from brake
L	5·72	5·2 + 0·85 = 6·05
R	25·9	20·9 + 3·26 = 24·16
X	56·3	52·7 + 6·5 = 59·2

The second column in this table was the indicated horse-power obtained from the indicator; the third was the indicated horse-power obtained from the brake-power in one of the ways used by the Committee, that is, by the addition of the no-load indicated power to the brake-power. It would be seen that in engine *L* the indicator registered 5 per cent. too low as compared with the other method, while for engine *R* it was 7 per cent. too high, and for engine *X* again 5 per cent. too low. Dealing with the effect of back-lash upon the pencil indicator diagram, Professor Hopkinson exhibited Fig. 8, in which the full line was a correct indicator diagram and the dotted line was the same diagram distorted by horizontal back-lash in the pencil movement. He pointed out that the defects of the pencil indicator to which he had especially drawn attention were peculiar to gas engine testing, and were due to the very high ratio which the maximum pressure bore to the mean pressure; a ratio which, in the gas engine under consideration, was frequently as high as six. In the steam engine the ratio of these two quantities was much less; weaker springs could be used and the main height of the diagram would be consequently greater.

Professor Hopkinson then exhibited Figs. 9 to 11, Plate I, showing the arrangement of gear for giving the piston-motion of his indicator. Figs. 9 and 10 showed the gear used on the Crossley engine. *A* is an eccentric fixed on the crank-shaft, the line of centres being exactly parallel to the crank; the straight rod *B* and the lever *C* transmit the motion of a roller pressed against the eccentric to the indicator *D*. *E* is the observing telescope and *F* is the small gas-holder used for the gas-measurements. Fig. 11 showed the arrangement of the indicators on a small Belliss engine. Three indicators were used, A_1, A_2 and A_3; two on the high pressure and one on the low pressure cylinder. The telescope *B* could be

turned at will on to any one of the three indicators, and the diagram showed. In Plate II, Fig. 12 was a diagram taken from the Daimler engine, referred to in the paper, at a speed of 800 revolutions per minute, and the diagrams, Figs. 13 and 14, were taken from the Belliss engine at 600 revolutions per minute.

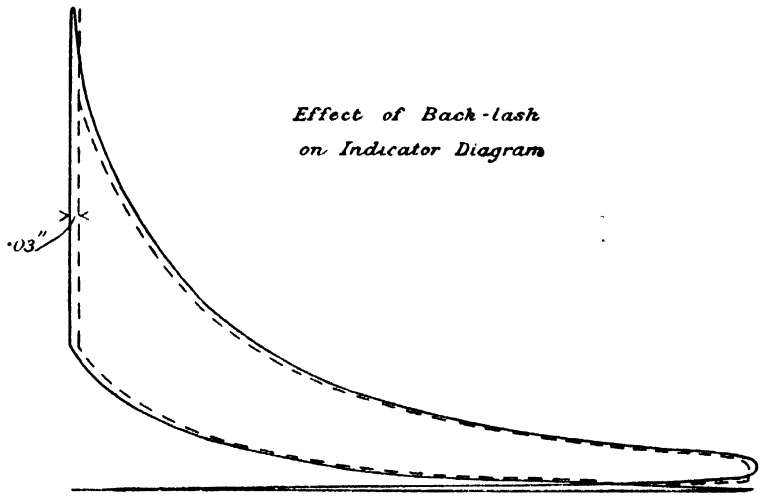


Fig. 8.

He would so far anticipate the results which had been obtained by the methods described in the paper as to exhibit the curve of thermal efficiency, Fig. 15. The points on this curve had been obtained at different times and by different observers, and represented in the aggregate a large number of observations.

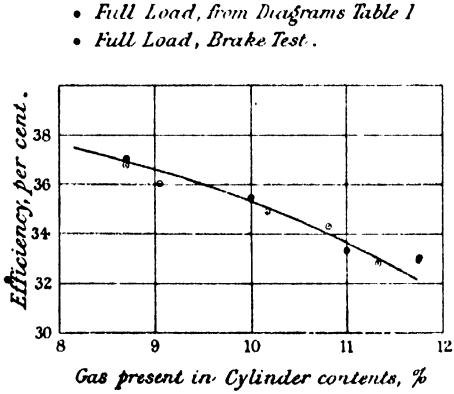


Fig. 15.

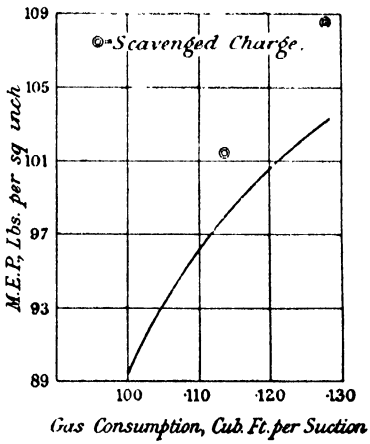


Fig. 16.

Some were obtained with the optical indicator described in the paper and others by observations of brake power, with a properly corrected addition for the mechanical and pumping losses. The full-line curve drawn among the points exhibited the relation between efficiency and strength of mixture when running fully loaded. None of the observed efficiencies differed by more than one-hundredth

part of its value from the mean efficiency as given by the curve. His main object in calling attention to this diagram was to illustrate the degree of accuracy obtained in the measurements, which might be said to be remarkably good, when it was considered that the measurement of efficiency involved both measurements of gas-supply and of indicated power, and was affected by errors in both. He would, however, point out the great diminution in efficiency as the percentage of gas in the charge increased. Fig. 16 showed the relation between mean pressure and gas-consumption, and he exhibited that figure (though it was not strictly within the scope of the paper before the meeting) in order to show the higher mean pressures realized with the same gas charge when the engine was scavenging. This was due to the fact that the gas was diluted with a greater quantity of air, the resulting mixture being weaker and the efficiency therefore higher, as shown in Fig. 15. The fact that in this engine, when running fairly fully loaded, the mean pressure in the cycle following a miss was little, if at all, greater than in cycles following an explosion, could therefore only be explained on the supposition that the quantity of gas taken in the former case was rather less than in the latter. As shown in Appendix II (page 245), this was to be expected on theoretical grounds*.

Professor Hopkinson, in reply (to the discussion), said that a great part of the discussion referred to the relative merits of different types of indicators, and he would deal with that part first. He was glad to find that his remarks upon the inaccuracies of the pencil indicator were confirmed by such authorities as Professor Callendar, Captain Sankey, and Mr Dugald Clerk. One or two speakers had, however, traversed his statements, particularly Professor Burstall, who referred to the work done in Germany by Professor Meyer and published in 1901. Professor Burstall suggested that he (Professor Hopkinson) had ignored this work. That was certainly not the case; the series of papers then published by Professor Meyer constituted an important piece of work which he greatly admired, and they could be read with advantage even today. But they were rather out of date, the conditions requiring to be met now differing from those which Professor Meyer had to meet. No one knew better than Professor Burstall the nature of the advance in gas engine practice which had occurred during the past eight years, but he could hardly have grasped the effect which these changes in gas engines must have upon the methods of testing them, or he would not have cited Professor Meyer's results as applicable without modification to the present day. The nature of the change might be best expressed by saying that while the mean pressure of the gas engine had not greatly altered during the last ten years, maximum pressures had more than doubled. Maximum pressures determined the strength of the spring that could be used in the indicator, with the consequence that while within as short a period as eight years ago, with a spring of 150 lbs. to the square inch, a large diagram was obtained $\frac{1}{2}$ inch high or more, it was now necessary to use a 400-lb. spring, giving a diagram only $\frac{1}{4}$ inch high. All the errors in transmitting the motion of the spring to the pencil, such as were caused by back-lash, etc., would be the same absolute amount in the two cases, and would therefore now bear about twice the proportion to the quantity which had to be measured—the mean height. To illustrate the point more in detail, he had copied one of the diagrams given in Professor Meyer's paper to which Professor Burstall had referred. The engine with which Professor Meyer worked, compressed to 75 lbs. per square inch, with a maximum pressure of about 300 lbs.; the engine he (the speaker) had tested compressed to 175 lbs.

* See, however, the note † on page 245.

absolute, and the maximum pressure sometimes exceeded 600 lbs. That indicated the difference in the conditions. The effect was shown in the following diagram, Fig. 17.

The full line was a copy of the diagram published in Professor Meyer's paper. The strength of spring was not given on the diagram, but from various statements in the paper it must have been about 150 lbs. That would be a reasonable spring to use, because with that spring the diagram would be about 2 inches high, the maximum pressure being 300 lbs. The dotted line was the diagram from the Crossley engine with which he had been working, compressing to 175 lbs. per square inch, given by the Crosby indicator with a 360-lb. spring. He had been using 300-lb. and 400-lb. springs. There was not much to choose between the two; what was gained with the 400-lb. spring in diminished vibration was lost in the size of the diagram; he believed that a spring of about 360 lbs. would

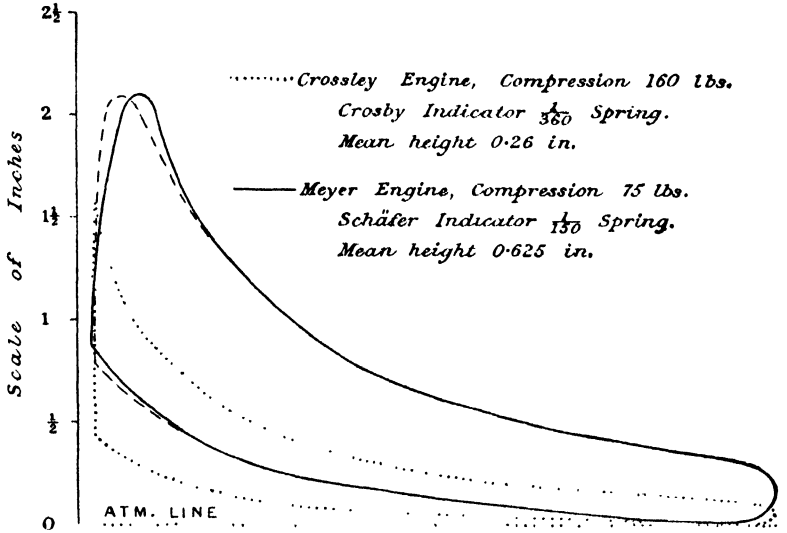


Fig. 17.

give the best results. The diagram was drawn with the oscillations smoothed out; in actual fact the pencil would frequently kick up above 2 inches, which might be taken as being the maximum permissible height of the diagram. It was quite obvious from the mean heights of the two diagrams, the one 0·26 inch and the other 0·625 inch, that two very different things had to be dealt with. The mean height of the diagram from a modern high-compression engine was necessarily very much less than it used to be, and it might be said that, speaking broadly, the errors that Professor Meyer found would have been doubled had he been working with a modern engine. Bearing that in mind he would refer to Professor Meyer's conclusion in the last sentence of the paper, which was as follows:

"The indicated work of the gas engine cannot therefore be quite reliably determined, even with great care. Though an accuracy of 1 per cent. will be reached in many cases, errors of 2 per cent. to 3 per cent. must be counted upon in others*."

* *Zeitschrift des Vereines Deutscher Ingenieure*, vol. 45 (1901), p. 1349.

If Professor Meyer had made his experiments at the present time, the latter sentence would have run "though an accuracy of 2 per cent. will be reached in many cases, errors of 4 to 6 per cent. must be counted upon in others." Having regard to that, he thought he could add Professor Meyer to the distinguished engineers whose names he had already mentioned as supporting his statement, that under the present conditions the pencil indicator could not be relied upon within 5 per cent.

Mr Peyrecave's and Mr Wilmot Spencer's remarks on indicators were subject to the same criticism as Professor Burstall's, for they gave their opinions as to the percentage of accuracy obtainable with the pencil indicator without specifying the strength of spring used, whereas with any given indicator the percentage of error due to back-lash and similar causes would be in proportion to the stiffness of the spring. Broadly speaking, his (Professor Hopkinson's) opinion was that the mean height of a pencil diagram could be measured correctly to about 2/100 in. equivalent to about 6 per cent. on a diagram 0·3 inch high, or 3 per cent. on Meyer's diagram which was 0·6 inch high. In this opinion he thought most users of indicators would agree with him.

He (the speaker) would like to refer at this point to the question of the effect of temperature upon engine friction. As he had stated in the paper he did not claim to be the first to have discovered that; he only alluded to it because it was a very important point in testing engines for mechanical efficiency. As Professor Burstall said, Meyer had dealt with the matter very fully and Mr Davis had also done so at an earlier date, but it was a point that was often overlooked in gas engine testing.

Professor Burstall and several other speakers had asked, in reference to the size of the spot of light given by the optical indicator, whether the outside, the middle, or the inside of the line should be taken in the diagram shown in the paper, Fig. 1, page 232. Naturally the middle must be taken. The spot in that case was wide, but the centre of the spot was a definite point, and it was that point which described the diagram. The "ghost" which accompanied the spot, and which was clearly apparent in the reproduction, showed how definite the line really was. It was quite easy to reduce the size of the spot by proper methods and to produce an extremely fine line; but for practical purposes—the purposes for which he used his indicator—the diagrams he was getting and such as he had shown were quite good enough. There was not the slightest doubt that those diagrams could be integrated, and the mean pressure obtained, correct within 2 per cent., and in many cases nearer still. In tests such as those he had mentioned on page 231, a large number of diagrams had been taken, and he had set different observers to integrate them quite independently, and he found, taking the centre of the line, he always obtained results which were covered by 2 per cent., even with thicker lines than those shown on the diagram which had been reproduced. However, Professor Burstall's remarks rather put the investigators on their mettle, and they decided to see what they could do in the way of producing more artistic diagrams.

The diagram reproduced in facsimile in Fig. 18, Plate III, represented about 100 consecutive explosions of the engine. It illustrated in a remarkable way one of the points that he had particularly insisted upon, namely, the extreme regularity of the performance in an engine of that kind, if only care were taken to keep the pressure of the gas constant. He thought there was no question that

the area and the mean height could be determined within 1 per cent. The width of the line, measured under a microscope, was about 1/150th of an inch, and the position of the centre of the line could be determined correct to 1/400th of an inch, which was 1/200th of the mean height. He (the speaker) did not suggest that they could get the mean pressure with that degree of accuracy, because other errors came in; but he did say it was possible to get the area of the diagram correct to within 1 per cent., and the mean pressure, whether of that diagram or of the one first published, Fig. 1, page 232, correct to within 2 per cent.

Fig. 19, Plate III, illustrated another point, namely, the practical identity, in that engine at any rate, of the explosions following a miss or following a scavenging stroke, and the explosions which did not follow such a stroke. This diagram covered 100 cycles, missing one in three, so that half of those cycles followed a firing stroke and half a scavenging stroke, thus: explosion, explosion, miss, going on for 100 cycles. This diagram was not so sharply defined as the last, but the mean pressure could still be measured within 2 per cent.

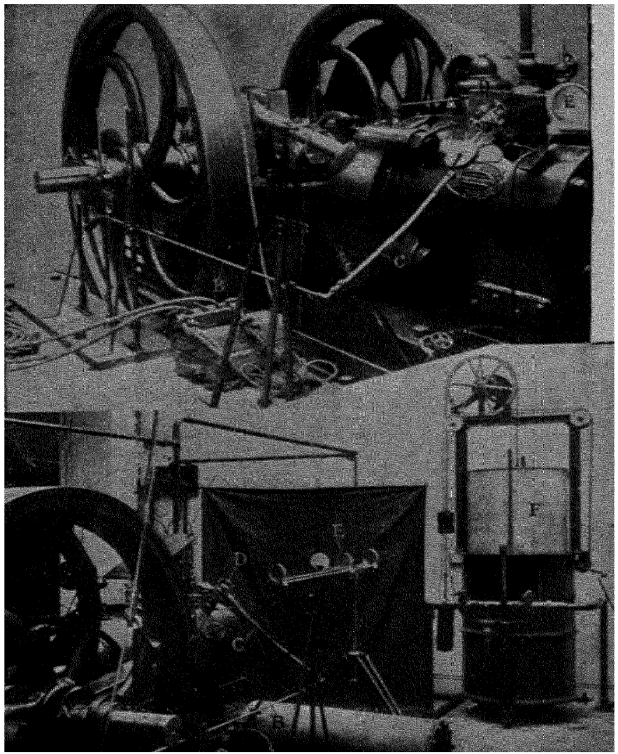
Fig. 20, Plate IV, was taken with the same spring as before, but with a piston of four times the area. The engine was simply motored round with the gas-supply cut off, the object being to show the difference between the compression and expansion strokes due to loss of heat. That was very clearly shown in the diagram. The diagram covered about 50 cycles with the engine motored round at 150 revolutions per minute.

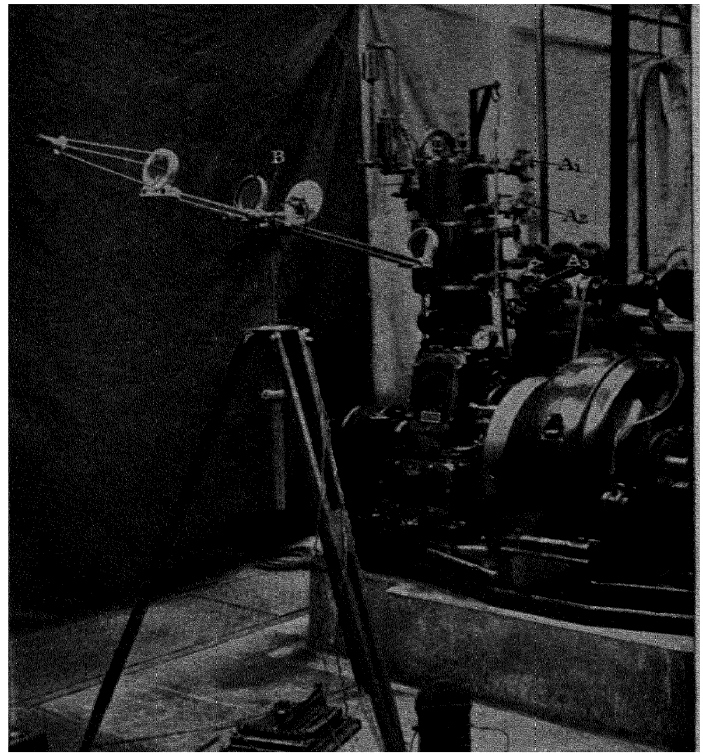
Fig. 21, Plate IV, was a light spring diagram. In this case there were 100 cycles: fifty explosions and fifty misses, occurring alternately. The line marked *ext* was the exhaust line after an explosion, and ext_1 that after a miss. The top compression line *c* was that following after the engine had taken a charge of gas. The line c_1 just below it was the compression line when the engine was taking in air only, giving a rather lower pressure at the start; and the line *exp* was the expansion following an idle compression. The diagram showed very well the difference between the two loops, according as to whether gas had been taken or not. Of the two suction lines, the upper, *s*, was drawn when the engine was taking gas, and the lower, s_1, when it was taking air only. There was a slight difference due to the fact that the gas-valve was not open, and it was necessary to suck a little harder.

While there seemed to be practical agreement that some form of optical indicator was necessary for accurate work with the modern gas engine, there was difference of opinion as to whether it should be of the piston or of the diaphragm type. Professor Callendar and Professor Dalby had both expressed a preference for the diaphragm indicator, stating that what seemed to the speaker an objection to the diaphragm indicator at the time he first began to use it—the non-proportional scale—had now been overcome, and that a perfectly proportional scale was obtainable. There were, however, a good many other objections to the diaphragm indicator, and if he had to do all his work over again he certainly would not use such an instrument. One of the points to which he particularly desired to draw attention was that the diaphragm which, of course, was spring and piston in one, was constantly in contact with the hot gases, and in consequence of its small capacity for heat must be subject to large variations of temperature. As the stiffness of the disc would diminish about 4 per cent. with a rise of temperature of 100° C., he thought that the indications of the diaphragm instrument could hardly be relied upon, unless means were provided for controlling and measuring the temperature of the disc, and he was not aware

that any such had yet been devised. Then the diaphragm indicator was a delicate instrument and, while this might not be an objection as long as it was in skilled hands, it rendered it unsuitable for some of the purposes which he had in view when designing the optical indicator. He wanted a good strong instrument for use in the laboratory by students and others on all sorts of engines—gas and petrol engines and high-speed steam engines. It must be of substantial construction, easy to calibrate and easy to use, and not very expensive to make. The instrument shown complied with all these conditions, and it was much simpler, cheaper, and stronger than any diaphragm indicator that he had seen or heard of. As regards convenience in use, it possessed the great advantage that the spring could be ground to give a round-number scale. This could not be done with the diaphragm. He used two springs in his indicator and three pistons; the areas of the pistons were in the ratios of 1, 2, 4, and one spring was five times as stiff as the other. The springs and pistons were very easily changed without removing the indicator from the engine, so that a range of sensibilities amounting to 20 to 1 could be obtained. The springs were ground so as to make the scales convenient round-numbers, say, 10 lbs. per square inch to the millimetre, with the small piston and the heavy spring, and ranging from that down to ½ lb. to the millimetre.

Captain Sankey had asked whether the replacement of the spring in the indicator altered the calibration. It did not; if the spring were removed and replaced the calibration remained the same within 1 per cent. This was another important advantage of his instrument as compared with the diaphragm indicator, in which the calibration was liable to change whenever the diaphragm was moved. The initial tension in the spring was put there for the purpose of registering the suction pressure. The spring was supported on four definite points in one line, two at each end, and in order to register suction pressures there must be an initial pressure on all four points of support. That was ensured by using a spring which, in its natural state, was not straight but slightly curved. When the spring was pulled down by suction it remained in contact with all four points, and suction pressures were registered on the same scale as those above atmosphere. Mr Spencer had referred to the thickness of the lines, but he had already dealt with that point. The same gentleman also referred to the statement in the paper that by plotting down the diagram, as shown on the transparent screen, the area could be obtained within 5 per cent. That was, of course, a question of accuracy in plotting; if it were plotted down quickly it could be obtained within 5 per cent. It was a question of the amount of trouble that was taken; the more trouble taken the nearer the result. If accurate measurements of mean pressure were required, it was better to photograph the diagram. The telescope arrangement was more especially useful for observing what was going on in the engine at the moment; observations could be made in broad daylight, and the pressure at any definite point in the cycle could be read off with all the accuracy that could be desired. The diagram could be watched, and any change in it consequent upon, say, injecting water into an engine, could be instantly noted. In view of these considerable practical advantages he thought that the piston optical indicator—whether of his pattern or another's—would come in, for gas engines and steam engines, at any rate up to speeds of 1000 revolutions per minute. For such speeds the speaker's piston-indicator, with a period of less than 1/500th of a second, was quite quick enough. For higher speeds the diaphragm instrument, on account of its quicker period, would probably continue to hold the field, at any rate for the present.





Optical indicator and gear

Figs. 9 and 10. On Crossley engine.

Fig. 11. On Belliss engine.

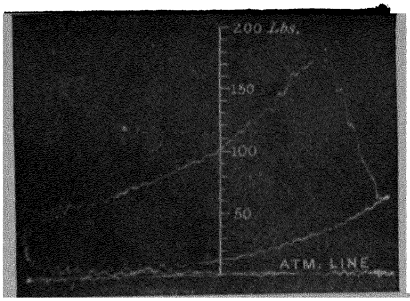


Fig. 12. **Daimler Engine. 800 revs. per minute. This diagram does not show suction pressures; the indicator was arranged to give positive pressures only.**

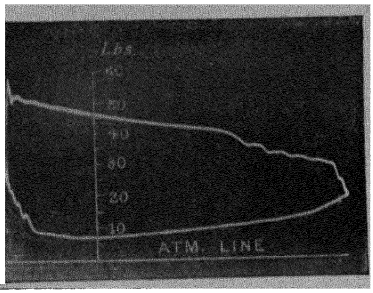


Fig. 13. **Belliss Engine. 600 revs. per min. H.P. cyl. Exposed for 400 revs.**

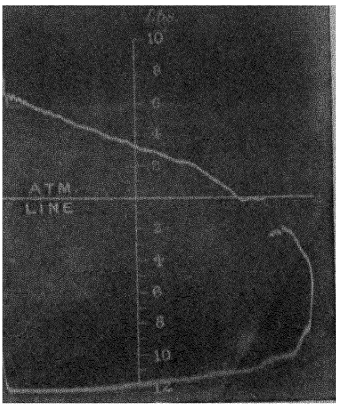


Fig. 14. **Belliss Engine. 600 revs. per min. L.P. cyl. Exposed for 400 revs.**

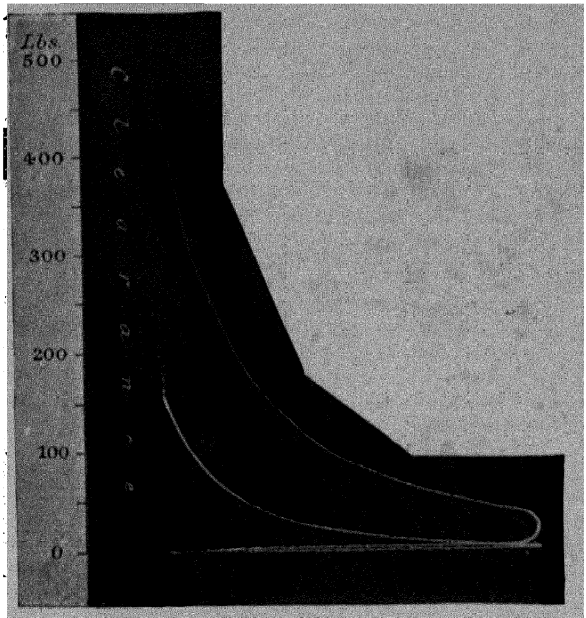


Fig. 18. 100 consecutive explosions.

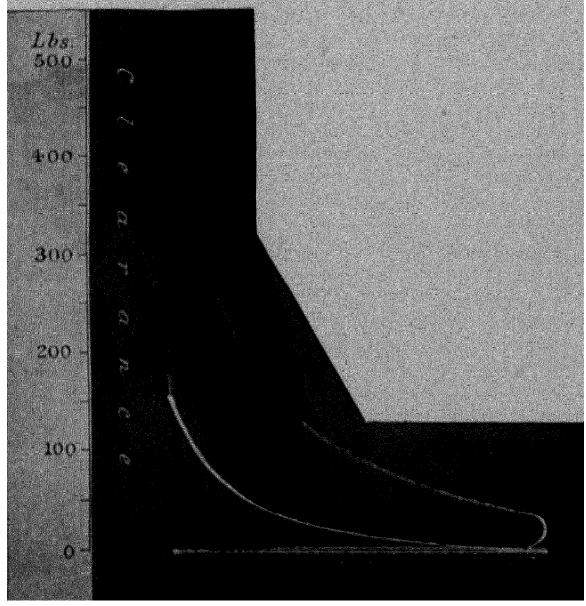


Fig. 19. 100 cycles. Missing every third cycle.

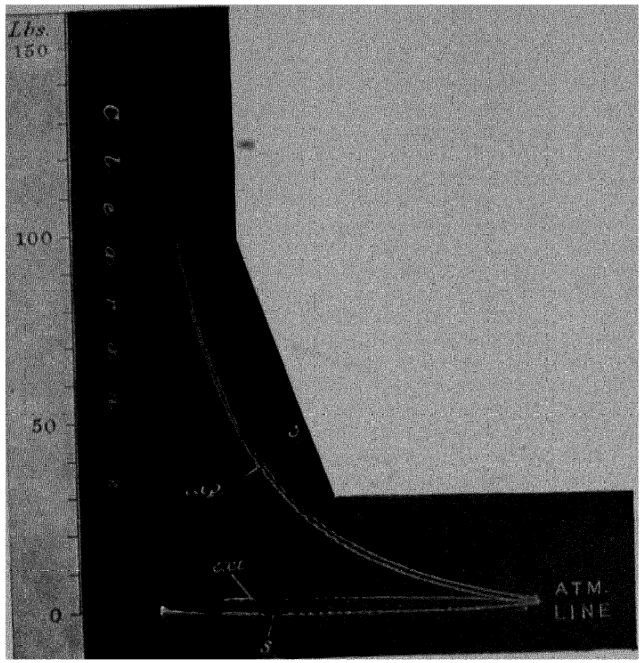


Fig. 20. Gas engine motored round. 150 revs. per min. Indicator with large piston to show difference between compression and expansion curves. 50 cycles.

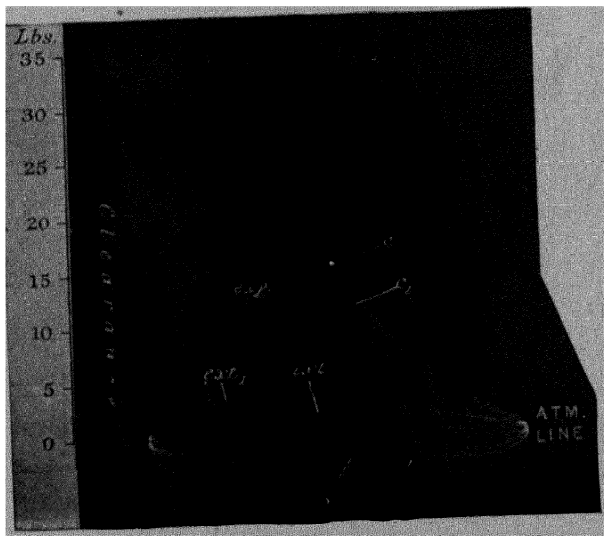


Fig. 21. 100 cycles. Explosions and misses alternately.

Mr E. J. Davis had asked a question about the effect of vibration of the engine upon the diagram, suggesting that the mirror recorded not only the displacement of the spring but also the movement of the engine. That was partly true; under certain circumstances, the indicator was affected by engine vibrations. Any such effect could, of course, be detected by taking a diagram with the cock closed. This had frequently been done on the Crossley engine, and it had been found that the line traced did not deviate from a straight line by so much as 1/200 in. It was quite certain, therefore, that in this engine, with the usual scale of photographed diagrams of about 200 lbs. per square inch, the error due to vibration did not exceed 1 lb. per square inch, or 1 per cent. The same was true of diagrams taken from a Belliss engine running at 600 revolutions per minute, which, however, was remarkably steady for an

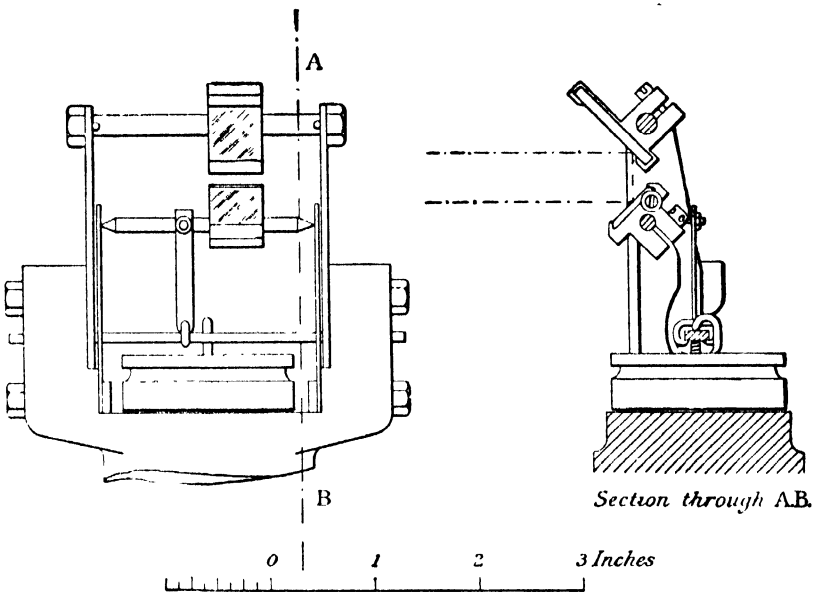


Fig. 22. Optical indicator fitted with second mirror to eliminate vibration.

engine of so high a speed. The Daimler engine, which was mounted in a somewhat rough fashion on a wooden stand, did show the effect of vibration at speeds exceeding 800 revolutions per minute, at which it amounted to as much as 5 per cent. on the mean height of the diagram. Vibration could, however, be eliminated by the use of a second mirror fixed to the indicator-frame, as shown in the diagrammatic sketch, Fig. 22. The beam of light, which in this case should be a parallel beam, was reflected successively from the two mirrors placed about 90° apart, and it would be apparent that any angular motion of the indicator as a whole about an axis perpendicular to the paper would, on the principle of the sextant, not be registered; only relative angular motion of the two mirrors would be registered. With this device it was possible to indicate engines with any amount of vibration, but he had not described it in the paper because it had not been found necessary to use it in any of the experiments there dealt with.

In reply to Mr P. H. Smith's question about indicating the Diesel engine, the speaker would refer to Fig. 20, Plate IV, which was about 3 inches high. A similar diagram could easily be obtained from the Diesel engine, using the same spring, but with a piston about one-fifth the area, this giving a maximum pressure of about 800 lbs. per square inch. The equivalent spring would then be about 270 lbs. per square inch.

The remainder of the discussion had referred for the most part to the Report of the Institution of Civil Engineers' Committee. In dealing with the remarks made in this connection he would like to draw attention to the conclusions stated at the commencement of the paper (page 226), which defined his attitude towards the Report, and to refer to the criticisms on these conclusions. The first conclusion was that the admitted difficulty experienced by the Committee in obtaining measurements of indicated power was probably due either to the defects of the indicator or to variations of gas pressure, with consequent variations of gas-supply and of diagram, from stroke to stroke. The Report gave no diagrams or other data from which an opinion could be formed on the point, and the conclusion was based upon the fact that his engine gave (with an improved indicator) perfectly regular diagrams which could be measured and were consistent among one another within 2 per cent., and which agreed with brake-power measurements with a similar degree of accuracy. Some light had been thrown on the matter in the course of the discussion by Professor Dalby, who had reproduced in Figs. 23 and 24 two of the cards obtained by the Committee. The first was described as a typical half-load card of the large engine *X*, and Professor Dalby stated that the smallest of the six diagrams shown on this card had a mean pressure of 83 lbs., whereas the largest had a pressure of 101 lbs., a difference of 20 per cent. Professor Dalby was inclined to ascribe the inaccuracy of I.H.P. measurement largely to this great variation of diagram. But he (Professor Hopkinson) thought from the appearance of the card that there must be a mistake in the figures, so he had measured up Fig. 23, and while he agreed with the 83 lbs. for the smallest diagram he had found the mean pressure in the largest to be not more than 90 lbs. Though the diagrams were by no means as regular as those with which he had dealt, the variation was not very serious after all, and was such that by averaging a moderate number of cards it should be possible to get results within 2 or 3 per cent. Such variation as there was, was probably due to the effect of the scavenging strokes when using a rather weak mixture. After a scavenging stroke the charge of gas and air was (if anything) rather less than after an explosion, and it was diluted with a greater weight of inert gas in the clearance space; therefore, the mixture was weaker and the ignition slow. The diagrams in Fig. 23 all agreed closely in the latter half of the stroke, and would appear to have had nearly the same gas-charge; but the ignition was slower in some, and these were probably cycles following scavenging strokes. Had the engine been firing every time with the same proportion of air to gas drawn in, the ignition would have been fairly quick in every stroke, and the diagrams would have been more constant approximating to the larger ones in Fig. 23. Professor Dalby had said that the variation was, in fact, less in the full-load trials, and if that were so, it could have had little to do with the errors in the I.H.P., which the report clearly showed to be considerable even in full-load trials. These must have been mainly due to the defects of the indicator. The card in Fig. 24 was taken from the smallest engine (5 H.P. only); it was mainly interesting as showing the impossibility of indicating such an engine with the ordinary indicator, on account of the sharpness of the explosion and consequent oscillation.

Professor Dalby emphasised the fact that in the Committee's trials the engines had been adjusted for maximum economy, and rather suggested that in his (the speaker's) trials that condition had not been kept in view, and that this might have been the cause of the better diagrams. That was not the case. The cards exhibited were taken with a mixture of nine parts of air to one of gas (as drawn in) and corresponded to a brake efficiency of 31½ per cent. at full-load. A rather weaker mixture (9½ to 1) gave a slightly higher efficiency (32 per cent.), but equally regular cards at full load. At half-load or running light this weaker

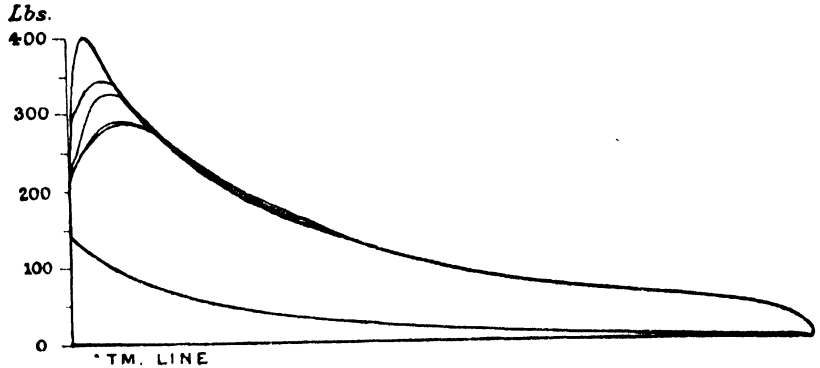


Fig. 23. Trial 17. Time 3.12 p.m. Casartelli indicator. Large engine. Spring $\frac{1}{250}$. Correct.

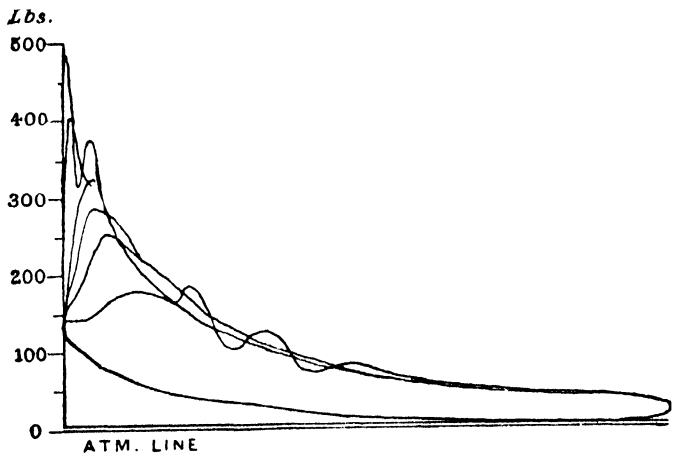


Fig. 24. Trial 3. Time 2.53 p.m. 5 H.P. engine. Half-load. Spring $\frac{1}{250}$. 6·6 % strong.

mixture tended to give irregular ignition after scavenging strokes, with diagram variations of the type shown in Professor Dalby's Fig. 22. Under half-load conditions a 9-to-1 mixture gave better results, and it was probable that this was about the right proportion for ordinary purposes. In the Committee's published trials the mixture was never weaker than 9½ to 1, and the average was 9 to 1, the best efficiency being recorded as 29·9 per cent.

The second conclusion in the paper having a bearing on the Committee's trials was that the mechanical losses (including pumping) at full-load were rather less than when running light. The difference was shown to be almost wholly accounted for by a change in the work of pumping, due partly to a

difference in the bottom loops and partly to the work done in compressing and expanding a charge of air. The inference was that the mechanical friction was nearly constant; but it should be observed that a change in the friction of half a horse-power was not excluded by the measurements, and that while this was only $1\frac{1}{4}$ per cent. of the indicated power, and was therefore in a sense negligible, it amounted to 15 per cent. of the friction. Dealing with the change in suction loop as between full and light load, Professor Callendar had stated that in the Committee's trials the difference was negligible, and had suggested that it was not usually of importance. That was entirely a question of the particular engine tested. It depended largely on the arrangement of exhaust-pipe, being much less with an exhaust-gas calorimeter, such as was used in his trials and in the Committee's trials, than with a long exhaust-pipe coupled direct to the engine. In the latter case, which was the common case, there was (as was well known) a considerable depression of the exhaust line after an explosion, which might in some cases be so marked as to cause actual suction during the exhaust stroke, and so produce a scavenging action. The speaker had examined several engines with respect to this particular point, and felt sure that in the majority of practical cases this effect was of importance. In one of the engines in his laboratory— a 10 H.P. Forward, with an exhaust-pipe about 25 feet long—the suction loop after an explosion was sometimes almost obliterated from this cause.

Professor Callendar had also expressed the opinion that in the Committee's trials there was no material difference between the compression and expansion strokes in an idle cycle. Through the kindness of Mr Dugald Clerk, he (the speaker) had been able to measure up one of the diagrams taken from the largest engine tested by the Committee. In that engine the compression pressure reached on a day when the barometer was 30·2 inches was 144 lbs. per square inch. With adiabatic compression, assuming no loss of volume, the pressure reached would be 155 lbs. per square inch. The figures given by the Committee showed that the loss of volume was very slight in this engine, and it appeared therefore that there must have been some loss of heat and some difference, probably amounting to between 1 and 2 lbs. per square inch, between the compression and expansion strokes. The loss, however, was not so great as in the Crossley engine, for which the corresponding figures were 175 and 200 respectively, a fact which was no doubt due to the higher compression. This effect depended, of course, upon the size of the engine—being greater in small than in large engines. In the Daimler motor-engine referred to in the paper the mean difference was about 3 lbs. per square inch at a speed of 750 revolutions per minute. It was usually only of importance in basing estimates of mechanical friction upon light load indicator diagrams, when neglect of it might cause an error of 20 per cent. or more in the amount of friction. The difference between the compression and expansion lines was almost impossible to detect with the pencil indicator on account of friction and back-lash, and it was not surprising that Professor Callendar should not have noticed it, as his attention was not specially directed to the point.

As regards the change in mechanical loss as between light-load and full-load, he would remark generally that while in the Committee's trials it may have been unimportant, there could be no question that in his own it amounted to more than 5 per cent. of the indicated power, and that wherever measurements of that order of accuracy were desired, its amount would have to be investigated if the Committee's method of computation were adopted. The items going to make the difference were severally small, but they were all in the same sense, and together they would often amount to a substantial total.

Professor Dalby made mention of the second method adopted by the Committee for obtaining mechanical efficiency, namely, that based upon the measurement of the gas consumption at half-load and full-load. He (Professor Hopkinson) had not overlooked that method, but did not consider it in the paper, as he was of opinion that it was inferior in accuracy to the other given by the Committee. It was sufficient to remark with regard to it that the work done in a cycle following a scavenging stroke might be as much as 7 per cent. greater than in a cycle following an explosion, the charge of gas being the same in the two cases. Thus, in the tests at half-load and full-load described by Professor Dalby, if the gas taken per suction were the same, the gas per I.H.P.-hour would be materially different; for in the one case the engine would be working practically as a scavenging engine, in the other not. Yet the mode of calculation proposed did not distinguish between the two efficiencies. It was obvious that it could throw no light on one of the points which the speaker had investigated, namely, the effect of scavenging upon thermal efficiency.

Several speakers had criticized his definition of indicated power, which was based on the positive loop of the diagram only, and there was some discussion as to what definition had really been adopted in the Report of the Institution of Civil Engineers' Committee. This question of fact had been settled by Mr Hayward's communication, from which it appeared that the Committee had in fact adopted his (the speaker's) definition both for full-load and light-load indicated power. They took no account of the bottom loops in either case. It was therefore not necessary to discuss this matter further. But he (the speaker) would like to point out that if, for any reason, it were desired to adopt the other method so that indicated power should represent brake-load plus mechanical friction only, then it was necessary to deduct all the bottom loops, and not merely to run over the bottom loop of a single diagram with the planimeter. The latter method would give a correct result in the extreme case when the engine was exploding every time, but in the other extreme case, when there was no brake-load and the engine was firing, say one time in six, the areas of six idle cycles must be deducted from the one positive loop in order to get the equivalent of the mechanical friction of the engine. It was obvious that in such a case the bottom loops must be separately determined with a light spring, as an error in measuring the bottom loop area was multiplied six times in the final result. If, for example, the mean positive pressure of the firing cycle were 100 lbs. per square inch, and the mean pressure in each idle cycle 4 lbs. per square inch, the mean pressure equivalent to mechanical friction would be 100 – 24, or 76 lbs. per square inch, that is, 76 multiplied by the number of explosions per minute, and by the appropriate constant would be the horse-power absorbed in friction. This instance showed the absurdity of this method of defining indicated power, if the term were to be used in any thermodynamic reasoning about a hit-and-miss engine. It was obviously wrong to speak of the mean pressure in a case such as this as 76 lbs. per square inch. Of course, for practical purposes it did not matter very much which definition was taken, for the pumping work with the engine firing every time was, anyhow, only 2 or 3 per cent. of the whole indicated power. But where any inference was to be drawn from light-load diagrams as to the magnitude of mechanical friction, it obviously became important that clear ideas should be held on the subject.

Professor Robinson had referred to the results given in Appendix II (page 244) as to the quantity of gas and air drawn in, stating that these results showed that the charge was less with a hot piston and cylinder, and he said that these results

contradicted the theory stated earlier in the Appendix, according to which less gas should be taken after a scavenging stroke. That, however, was not so. The results on page 247 referred to steady temperature conditions, such as could only be established after many hundreds of cycles. A *single* scavenging stroke in a series of explosions hardly affected the temperature of the inner surface of the cylinder at all. This he (the speaker) had been able to show by means of thermo-couples fixed in the piston and exhaust-valve. For such an isolated scavenging stroke the assumption in the theory which had been criticized by Professor Robinson—"the cylinder and piston temperatures being the same"—was practically true.

The statement in the paper that the injection of water did not affect the diagram, of course, referred only to the gas engine, and, moreover, only to the particular means of water injection which had been employed. As Professor Robinson and Dr Hele-Shaw had said, water injection profoundly influenced the combustion of oil; and it was quite possible that the injection of water in a very fine spray or as a cloud might affect the gas engine also. It was not, however, to be expected that the comparatively coarse drops injected in these experiments could have any influence, for there was not time for any appreciable evaporation.

Professor Hopkinson wrote, in reply to the written communications, that he had examined the indicators exhibited by Messrs Dobbie, McInnes at the meeting, and, though he had not had actual experience in using these instruments, he could see that admirable precautions had been taken to eliminate as far as possible the essential errors of the pencil instrument. He did not think, however, that even the most careful design could reduce these errors materially below the amounts which he had suggested in the paper. Inertia and the possible deformations of springs limited the mean heights of the diagrams from a modern gas engine to not more than 0·3 inch. That was a fact dependent upon the properties of materials, and could not be overcome by design. The mere measurement of such a diagram as it stood on the paper could not be carried out nearer than 1/100 in., equal to 3 per cent. of its mean height, and when to that were added the various errors due to back-lash, friction, etc., he felt justified in saying that the error of the number obtained, as representing the mean pressure in the engine, would frequently amount to 5 per cent., even with the most perfect design, manufacture, and operation.

As regards the use of the exhaust gases for steam-raising, to which Mr Thomas Turner had referred, Professor Hopkinson's opinion was that it would rarely, if ever, pay to do this if the steam were to be used for driving steam engines. It was quite true that, judged by the standards of steam engine economy, a considerable amount of heat was thrown away in the gas engine exhaust. But the actual percentage addition to the power of the gas engine which could be obtained by using its exhaust to raise steam, and thereby working a steam engine, was small, amounting to not more than 10 per cent., and would never pay the charges on the additional plant required. A better way of utilizing a portion of the rejected heat would be to increase the expansion of the gases in the gas engine and so to reduce their temperature. Of course these remarks only applied to the raising of steam for power purposes; in many cases where large gas engines were used, steam was needed for other purposes than power, and where this was the case it could frequently be raised with advantage from the exhaust gases.

19

THE EFFECT OF MIXTURE STRENGTH AND SCAVENGING UPON THERMAL EFFICIENCY OF GAS ENGINES.

["Proc. Institute of Mechanical Engineers," 1908.]

In October 1907 the author presented to the Institution an account of experiments On the Indicated-Power and Mechanical Efficiency of a 40-B.H.P. Crossley gas engine*, the chief points of which were the use of a new type of optical indicator and a full investigation of the mechanical losses under various conditions. It was found that this instrument gave results for the indicated power which were more consistent than those obtained with the pencil indicator; and these results agreed closely with those got by adding to the brake-power the power absorbed at light load under the same conditions of lubrication and jacket temperature, a proper deduction being made for the difference in the pumping work at full-load and light-load. It is probable that the full-load indicated power of this engine can be determined either by the new indicator, or from the brake-power and mechanical losses, correct to within 2 per cent. For the indicated power at light-load reliance has to be placed on the indicator only, but the agreement between the two methods in the other case shows that it can be trusted to within 2 per cent.

In the same Paper a method of measuring gas-consumption was described, which consisted in observing the fall of a small standard gas-holder during some fifty suctions of the engine. The general consistency of the results showed that the gas-consumption could be determined in this way to within 1 part in 200.

As it appeared probable that both measurements, that is, gas-supply and indicated power, were capable of considerable accuracy, it seemed worth while to make some tests on thermal efficiency. The points chosen for investigation were the effect of strength of mixture and of scavenging. The method used for measuring the gas was especially advantageous for this purpose, for it gave the actual volume of gas used in the series of 40 or 50 explosions from which the indicator diagrams were taken, and the materials for a complete measurement could thus be obtained in a few minutes. Diagrams with three or four different gas-consumptions could

* *Proceedings* (1907), Part 4, p. 863. Page 226 of this volume.

be got within an hour, during which time the calorific value of the gas would remain constant, so that the effect of changing the strength of mixture or of scavenging by running without load could be very accurately determined*.

Effect of Strength of Mixture. The following table shows the results of a series of tests which were all taken within two or three hours; the measurement of gas-supply being made as described in the last paragraph simultaneously with the photographing of the diagram.

In measuring the indicated power, two diagrams, each covering about a dozen explosions, were photographed in each test. These photographs were integrated by planimeter direct from the negatives by two independent observers. In no case did the mean pressures so determined differ by more than $1\frac{1}{2}$ lb. per square inch or 1·6 per cent., and the average difference was about 1 per cent. Thus the mean pressures given, which are the means of the two observations, may be taken as correct in every case to within 1 per cent.

Table I.

Gas		M.E.P.	Efficiency per cent.	
Cub. ft. per suction	Percentage of cylinder contents†			
0·1275	11·0	102·2	32·5	Full-load
0·1147	10·0	98·4	34·7	,,
0·1005	8·65	90·2	36·5	,,
0·1275	9·5	108·4	34·5	Light-load
0·1140	8·5	101·6	36·1	,,

The relation between gas-consumption and mean effective pressure is shown in Fig. 1.

All the observations here shown were taken within two or three hours and it may be assumed that the calorific value of the gas remained the same during that time.

The calorific value was not taken at the time of the test, and the efficiencies are calculated on an assumed lower calorific value of 600 B.TH.U. per cubic foot at standard temperature and pressure—a value which accords with measurements made a day or two after the test. The relation between the efficiencies with different gas-consumptions, which it was the main object of this experiment to determine, is unaffected by any error in the calorific value or in the calibration of the indicator, and is undoubtedly shown with great exactness in the above series of figures.

* Fig. 8, Plate I, is a facsimile reproduction of a sample diagram at full-load.

† Calculated on the assumption that the full-load suction temperature is 100° C. and the light-load 50° C.

That the absolute values are also fairly close was shown, however, by tests made at other times in which the calorific value was measured at the time of the test. In some trials the indicated power was calculated from the brake-power and the indicated power at no load, as in the Institution of Civil Engineers' trials, but with a proper deduction for the difference in

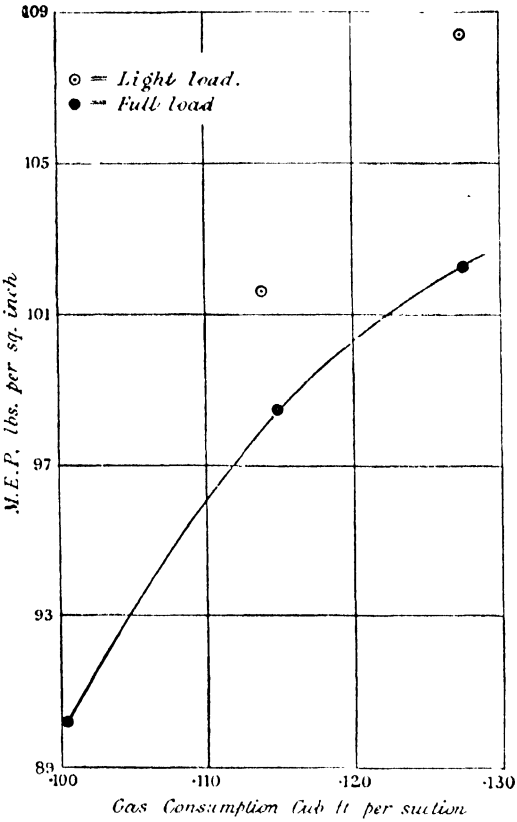


Fig. 1. Relation between gas-consumption and mean effective pressure.

pumping work as between light- and full-load*. The following were the results of one such test:

Full-load.

Gas used 0·1006 cubic foot per suction at 52° F. and barometer 30·46 ins.
Calorific value (measured at time), 570 B.TH.U. per standard cubic foot.

$\frac{\text{Gas}}{\text{Total charge}} = 0{\cdot}0865.$

$\frac{\text{Explosions}}{\text{Cycles}} = 0{\cdot}896.$

B.H.P. = 34·4. Efficiency on B.H.P. = 32·2 per cent.
Jacket temperature = 190° F.

* See the author's paper, "On the Indicated-Power and Mechanical Efficiency of the Gas Engine," *Proceedings* (1907), Part 4, p. 863. Page 226 of this volume. In the experiments there described the negative indicated work when the engine is fully loaded is shown to be 2·3 H.P. less than that indicated in idle cycles in which a charge of air is compressed and expanded.

Light-load (taken as soon as the brakes were off).

Gas, 0·1129. Mean pressure calculated from gas-consumption, 100 lbs.

$\frac{\text{Explosions}}{\text{Cycles}} = 0{\cdot}138$. I.H.P. = 6·9.

*Extra pumping work, 0·896 × 2·3 = 2·0.

∴ Mechanical losses = 4·9
Indicated horse-power = 39·3
Mechanical efficiency = 87·5%
Thermal efficiency * = 36·8%
} at full-load.

The thermal efficiency at full-load is plotted in terms of strength of mixture in Fig. 2. The straight line which most nearly represents the

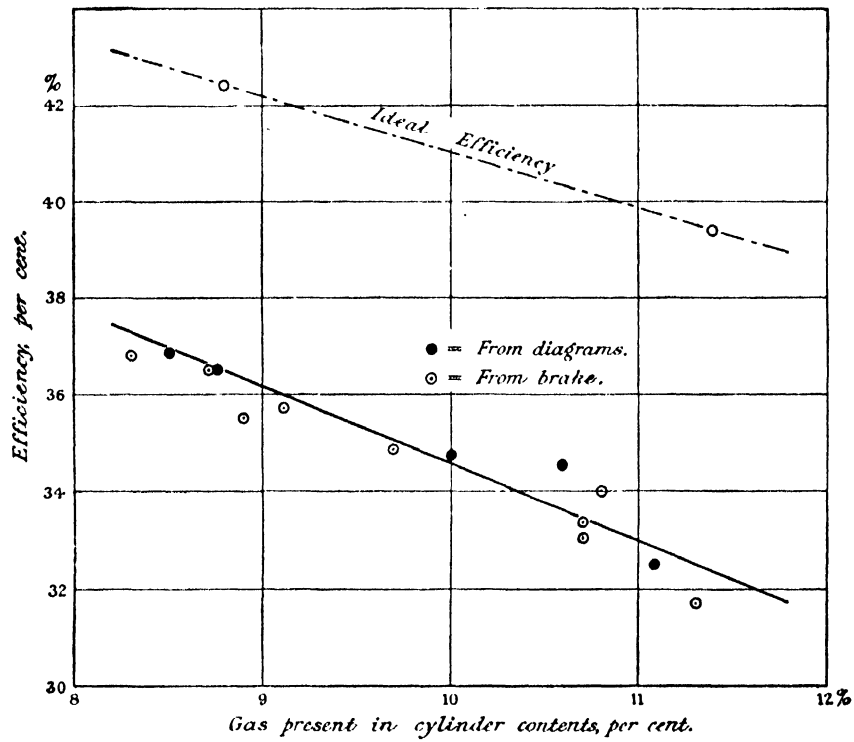


Fig. 2. Thermal efficiency.

mean efficiency is drawn through the points, about equal weight being given to diagrams and brake tests. A number of other tests are shown as well as those cited above. The results are tabulated in Appendix V, Table VIII (page 286).

The strength of mixture is calculated on the assumption that the suction temperature with full-load is 100° C., and with light-load (scavenged charges) 50° C.† There is some uncertainty about these temperatures, and a corresponding uncertainty in the absolute value of the pro-

* See note on page 265.

† See Appendix II (page 278).

portion of coal gas in the mixture. But as the total weight of charge is practically independent of the strength of mixture (if the engine be kept fully loaded and the jacket temperature constant) the *relative* values of the proportions under full-load conditions are unaffected by this uncertainty. The only effect of an error in the suction temperature is to alter the horizontal scale of the diagram, Fig. 1. The scavenged charges are dealt with later. The weakest mixture used in these tests contained about 8·65 per cent. of coal gas when in the engine, the proportion of air to gas drawn in

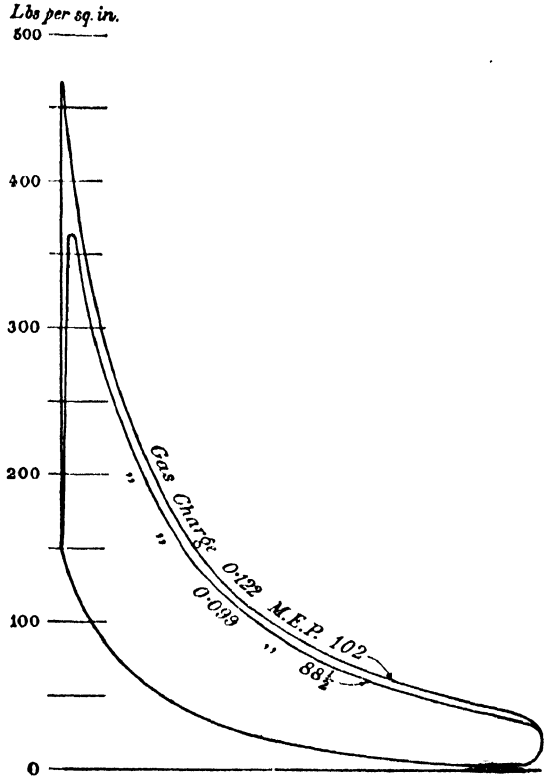


Fig. 3. Typical diagrams.

being about 9½ to 1. The diagram was quite normal, the explosion line being nearly vertical, Fig. 3. Weaker mixtures than this, however, would not ignite regularly. At the other end of the range the proportion of air to gas was about 7½ to 1, the excess of air being about 1½ times the volume of gas; slightly heavier charges than this could be used, but it is possible that the combustion would not be complete and the pressures in the engine would become dangerously high. The range of mixtures tested therefore covers all which could be practically used. Within that range the efficiency diminishes steadily as the strength of mixture increases, the

difference between the weakest and strongest charge amounting to 4½ per cent. in efficiency, or 12 per cent. on the work done*.

Causes of higher Efficiency with weaker Mixtures. That the efficiency will increase as the strength of mixture is reduced, so long as the combustion is substantially complete, is to be expected from the now well-established fact that the specific heat of the working substance increases with the temperature. The work done in the gas engine cycle is mainly determined by the rise of pressure which occurs on explosion; and in the same engine the area of the diagram with different mixtures is about proportional to this rise, when corrected for the change of volume during combustion. If the specific heat of the working substance were constant, as is assumed in the air-cycle, the rise of temperature and therefore of pressure at the explosion end of the diagram would be proportional to the heat supply, and the efficiency would therefore be constant. But the specific heat being in fact greater at high temperatures, the rise of temperature or of pressure on explosion increases in a less ratio than the heat supply and the efficiency therefore diminishes as the supply of heat is increased.

The ideal efficiency of a gas engine, by which is meant the efficiency which would be attained if all heat losses to the walls were suppressed and if combustion were complete and instantaneous at the in-centre, is easily calculated if the internal energy of the working fluid is known as a function of its temperature. It cannot be said that we yet possess this knowledge in any high degree of accuracy, but enough is known to enable an estimate to be formed of the effect of strength of mixture on efficiency. Fig. 4 shows the internal energy curves corresponding to the weakest and strongest mixtures used in these experiments. The ordinate of the curve is the quantity of heat in foot-pounds required to heat a standard cubic foot of the burnt products, at constant volume, from 100° C. up to the temperature represented by the abscissa. These curves are calculated from the figures given by Langen for the specific heats of CO_2, H_2O and air, between 1500° C. and 1900° C., and from the results of Holborn and Austin and Holborn and Henning, at lower temperatures. The values given by Clerk for a mixture of intermediate composition are also shown. The ideal engine efficiencies for the two mixtures can be calculated from these curves by the method given by the author in the discussion on Mr Dugald Clerk's Paper before the Institution of Civil Engineers†. Details of the calculation are given in Appendix III (page 280). The ideal efficiencies corresponding to mixtures containing respectively 8·8 per cent. and 11·4 per cent. of coal gas, calculated by this method, are 42·4 and 39·4 per cent. respectively. For

* That, within limits, weaker mixtures give higher efficiency was one of the results obtained by Professor Burstall in his recent experiments for the Gas Engine Research Committee. See also Nägel, *Zeitschrift des Vereines Deutscher Ingenieure*, 1907. Other investigators have doubtless noticed the same thing.

† "On the Limits of Thermal Efficiency in Internal-Combustion Motors," *Proceedings of the Institution of Civil Engineers*, vol. 169, p. 157.

mixtures of other compositions the efficiency will follow a straight-line law sufficiently near for present purposes, and this straight line is shown dotted in Fig. 2 (page 266). In Fig. 5 the efficiency results, both calculated and observed, are exhibited on a different scale in order to admit of the two lines in Fig. 2 (page 266) being prolonged backwards to the line

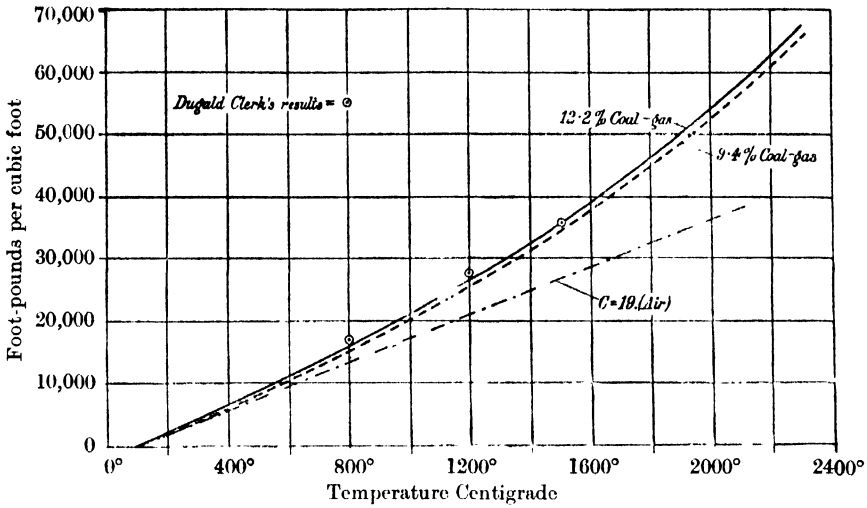


Fig. 4. Internal energy curves.

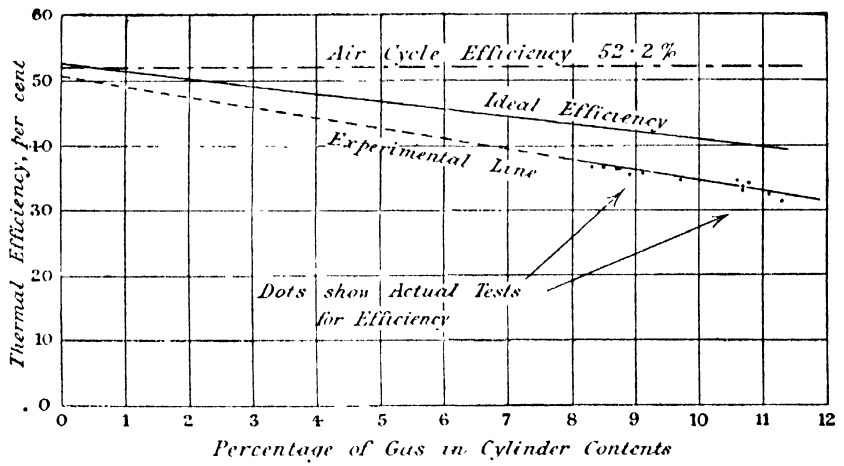


Fig. 5.

of no heat supply. The line of constant efficiency (52·2 per cent.) given by the air-cycle, in which the thermal capacity of the gas is supposed constant, is also shown on the same figure. It is worth noting that these three lines meet the line of no heat supply at approximately the same point. In other words, if it were possible to burn weaker mixtures—say, by using stratification—and if the actual and ideal efficiencies continued to bear a

linear relation to the gas-consumption, these efficiencies would tend to become equal to one another and to the air-cycle efficiency with a very small gas-consumption. The ideal efficiency ought, of course, to approximate to the air-cycle efficiency when the charge is greatly reduced; the close agreement in the other case is no doubt, to some extent, accidental, but something of the kind is to be expected.

Without laying too much stress on the absolute values* of the real and ideal efficiencies shown in Fig. 2, it is apparent, from the ratios that they bear to one another, that, while much of the superiority of the weaker mixtures is to be ascribed to increase of specific heat, that cause is not sufficient to account for the whole of the effect. Comparing the actual with the ideal efficiency, it will be seen that for a mixture containing 8·5 per cent. of coal gas the ratio—usually called the efficiency ratio—is 0·87, but when the proportion of coal gas is increased to 11·0 per cent. it is only 0·83; the weaker mixtures, in addition to giving a higher ideal efficiency, come nearer in practice to realizing that ideal. This is due to the fact that the percentage of heat lost to the walls during expansion is less with small gas charges than with large. The difference is sufficient to counterbalance an influence tending the other way, viz., the more rapid combustion of the stronger mixtures. This has been established by a series of experiments directed to that end.

Heat loss in Expansion. As pointed out by Mr Clerk, the ordinary method of determining wall-loss by the amount and rise of temperature of the cooling water does not give an accurate notion of the loss of heat occurring in the expansion stroke. Much of the heat in the cooling water passes into the walls after release and should therefore in a proper heat-balance be credited to exhaust. In a true heat-balance the measured items must be the work done and the energy contained in the gases at the end of expansion, the heat loss during expansion being obtained by difference. Such a heat-balance has been formed for the weakest and strongest mixtures used in these experiments.

The energy at the end of expansion is in part thermal, and in part the chemical energy represented by unburnt gas. For the calculation of the first item the data required are:

(1) Temperature of gas at end of expansion;

(2) Quantity of gas present;

(3) Its internal energy as a function of its temperature.

The quantity of gas present is known from the suction temperature and suction pressure; it may be taken as 1·06 standard cubic feet per explosion in full-load running with a medium jacket. The internal energy

* The *absolute* values of the efficiencies are affected by any errors in the calorific value of the gas or in the indicator calibration; and may all be wrong in any experiment by as much as 1·0. But the relations between the efficiencies with different strengths of mixture will be unaffected by these errors, since they are based upon measurements with the same indicator and the same gas.

is given by the curve, Fig. 4. The temperature at the end of expansion can be inferred from the pressure of the gases at release. For measuring this the indicator was fitted with a large piston giving an open scale. A series of consecutive tests were made, the gas charges being alternately about 0·1 and 0·13 cubic foot per explosion. In each test the gas charge was measured by gas-holder as described above, and the release-pressure was determined simultaneously either by photographing the diagram* or by reading it off in the telescope used with the indicator. The calorific value was also determined during the course of the experiments. Table II gives the mean of the results obtained in a series of such tests which show very good agreement.

Table II.

—	*A* (weak mixture)	*B* (strong mixture)
Gas per explosion as measured by holder	0·1007	0·1294
Gas used per explosion (standard cubic foot) ...	0·095	0·122
Percentage of coal gas present in cylinder contents	8·5	11·0
Pressure at release (lbs. per sq. in. absolute) ...	52	57
Pressure at end of expansion	45	49·5
Temperature at end of expansion (absolute C.) ...	1180	1290

The "pressure at release" is the pressure observed in the indicator at the moment of opening the exhaust-valve which occurs 45° before the out-centre. Between this point and the end of the stroke the volume increases in the ratio 1·11. The "pressure at the end of expansion" is that which would have obtained if the expansion had continued to the end of the stroke, and is calculated by multiplying the release-pressure by 0·87, a factor based on the assumption that the expansion curve if continued would have had the form $pv^{1\cdot35}$ = constant. The temperature at the same point is calculated from the pressure, taking the suction temperature to be 100° C., and allowing for a contraction of 3 per cent. in each case. The atmospheric pressure (which is also very nearly the pressure at the end of the suction stroke) was 14·7 lbs. per square inch.

From these figures, using the internal energy curves in Fig. 4 (page 269), and taking the quantity of stuff present as 1·06 standard cubic feet, the energy present as heat in the charge at the end of expansion (assuming such expansion to have been completed) is found to be 24·0 and 29·0 B.TH.U. respectively in the two cases. The calorific value of the gas (measured at the time) was 604 B.TH.U. per standard cubic foot. Thus 57·5 thermal units are supplied to the engine per explosion with the smaller charge and 74 with the larger. The quantities of heat present at the end

* Fig. 7, Plate I, is a facsimile reproduction of one of these photographed diagrams.

of expansion are therefore 42 per cent. and 39 per cent. respectively of the whole supply.

The other part of the energy of the exhaust gases, viz., the amount of unburnt fuel present, was next investigated. The engine was run exploding every time, and samples of the exhaust gases were collected from the exhaust-gas calorimeter and analysed by combustion over hot copper-oxide. The jets of the calorimeter were kept working while the samples were being taken, so that combustion was checked instantaneously at release, no burning in the exhaust-pipe being possible. The analyses therefore represent fairly closely the state of the cylinder contents at the opening of the exhaust-valve. In five analyses at full-load—two with a charge of 0·1 cubic foot and three with a charge of 0·13—the percentage of fuel discharged unburnt varied from 0·2 to 1·5, with a possible error in each analysis of ± 0·5 (one-half of 1 per cent. of the coal gas originally present). In full-load running it is very improbable that more than 1 per cent. of the fuel is ever unburnt at the end of the expansion stroke; and there was no indication in the analyses that the percentage unburnt varies in any definite way with strength of mixture or jacket temperature*.

In forming the heat balance for full-load running, the unburnt gas may be neglected altogether, as its amount is uncertain, but can never be such as to affect materially the result of a comparison between weak and strong mixtures. The heat-balance is accordingly as follows:

Table III.

	A (weak mixture)	*B* (strong mixture)
Indicated work (from curve, Fig. 2, p. 266.) ...	37	33
Heat in exhaust (from release-pressure)	42	39
Heat loss in expansion (by difference)	21	28
	100	100

In *A* 0·095 standard cubic foot of gas is taken per explosion, equivalent to a supply of 57·5 thermal units. The percentage of gas present in the cylinder contents is 8·5. In *B* the gas charge is 0·1220, equivalent to 74 thermal units, and to 11 per cent. of the cylinder contents†.

* Details of the analyses are given in Appendix IV (page 283).

† For convenience of description, a single representative test is here described, all the observations given above having been taken on one day, but it should be said that the results were confirmed in a number of other tests of similar character. The absolute amounts of the release-pressure and of the quantity of heat present in exhaust of course vary with the calorific value of the gas and with the height of the barometer; but the percentages depend only on the strength of mixture, and are probably always within 0·5 of the values given. An error of that amount would correspond to an error of one pound per square inch, or 2½ per cent. in the release-pressure.

The efficiency ratio of the weaker mixture is 0·83 and that of the other 0·87. Since the unburnt gas, if any, is sensibly the same in the two cases, the difference between these two figures must be mainly due to the greater heat loss which occurs in *B*.

To put the stronger mixture on a level with the weaker in respect of loss in expansion, 7 per cent. of the heat-supply must be saved from the wall loss. This might conceivably be done by burning this mixture in a larger engine having smaller heat-losses. The resulting gain in efficiency would depend upon the place in the expansion stroke where the saving was effected. If the heat were all saved at the very beginning of the stroke, the engine would turn about one-third of it into work, equivalent to 2·3 per cent. of the total heat-supply. The efficiency in *B* on this supposition would be increased to 35·3 per cent., and the efficiency ratio to 0·89, which is slightly greater than that of *A*—0·87. There is good reason for supposing that the greater part of the difference in heat-loss as between the weak and strong mixture is due to the greater vigour of the explosion of the latter, and is therefore to be referred to the explosion end of the diagram. But some of the saving must, in fact, take place in the later parts of the stroke, when it will have less effect upon efficiency, and if this were taken into account it would somewhat reduce the figure found for the relative efficiency of *B* when corrected as above to make it comparable with *A*. Any difference in the rate of burning of the two mixtures would have the same effect. Having regard to this, the agreement between the figures is remarkably close and affords good ground for the statement that, of the 12 per cent. additional work done per cubic foot of gas with the weaker mixture, about half is to be ascribed to lower mean specific heat, and half to a smaller heat-loss in explosion and expansion.

Heat-loss after Release. It is interesting to compare the true heat-loss in expansion, determined as above, with the quantity of heat carried away by the jacket-water. A large number of measurements of the jacket loss were made in the ordinary way with gas-charges of about 0·1 and 0·13 cubic foot per explosion*, and it was found that the percentages were 27 and 33, equivalent to 15·5 and 24·5 thermal units per cycle respectively with a jacket temperature of 70° C. at exit. The loss by radiation should be added to these figures in order to get the whole heat passing into the engine during a complete cycle. The amount of this is uncertain, but it is probable that it is between 3 and 4 per cent., or say 2·5 thermal units, and it must be nearly the same in absolute amount in the two cases, if the jacket temperature be the same. The heat-loss in compression and expansion is 21 per cent., or 12 thermal units for the weaker mixture and 28 per cent., or 21 thermal units for the other case. Thus the loss occurring in the rush of gas past the exhaust-valve after release and during the exhaust stroke is 3·5 thermal units in each case plus the unknown

* See Table IX, Appendix VI (page 288), for details of some of these measurements.

radiation-loss, which is nearly the same for both. These losses ought to be rather less in absolute amount for the weaker mixture, because the gases are cooler; but no very great difference is to be expected, for the quantity of gas discharged and its state of motion are precisely the same in the two cases, and the difference of temperature is not very large. The estimate of these losses is very rough, as it is the difference of large quantities; that they come out about the same is, however, evidence of the general correctness of the experiments and deductions here given.

Heat Balances. In the course of this work about twenty-five tests were made for heat-balance with the exhaust-gas calorimeter. The results of all these tests are tabulated in Table IX, Appendix VI (page 288). In fifteen of the tests at full-load the balance was correct within ± 2 per cent., in four within 4 per cent., and one test was 7 per cent. out. On the average of these twenty tests at full-load the balance of heat unaccounted for in the engine was rather less than 1 per cent. The items on the credit side of the balance were:—Brake horse-power (measured or estimated from gas-consumption and efficiency curves), jacket, exhaust-gas calorimeter, and the (estimated) heat carried away by the exhaust gases after leaving the calorimeter. The balance unaccounted for covers radiation, conduction, and errors of observation. No satisfactory method has yet been suggested of separately determining the radiation, but some notion of its magnitude may be obtained by comparing the jacket loss with the same gas-charge with a hot and cold jacket. A number of tests agreed in showing that when the temperature of the jacket-water at exit is 70° C. the heat taken away by the water is less than at 40° C. by between 100 and 150 thermal units per minute, the gas-charge and all other circumstances being the same. When the engine is fully loaded this is equivalent to between 2 and 3 per cent. of the whole supply. As the indicator diagram is not affected to any perceptible degree by the jacket temperature, the heat actually received by the engine must be nearly the same in the two cases, and the difference must be mainly due to the higher radiation at the higher temperature. Since there is still some radiation of heat at 40° C. it seems probable that the total radiation at 70° C. is at least 3 per cent. The average balance shown in the tests at this temperature is only 1 per cent., so that there must be systematic errors in one or more of the items going to form the balance sheet*. On the other hand, it is hardly possible

* It is probable that the error is mainly in the calorific value of the gas. The wet meter used with the calorimeter was tested against the gas-holder at the beginning of the series of trials and found to be 2½ per cent. slow at the speed at which it was to be used, and this correction was applied to its indications in every case. About six months later, after all the heat-balance tests had been completed, it was again checked and found to be nearly correct. The presumption is, therefore, that the calorific values, while correct at the beginning of the series, were under-estimated by perhaps 2½ per cent. at the end, and that on the average they were under-estimated by between 1 and 2 per cent. The absolute values of the efficiencies would be affected by the same small error, and are probably too high on the average by about 0·5.

that the aggregate of these errors can amount to so much as 3 per cent., or that the radiation at 70° C. can be more than 4 per cent.

Effect of Scavenging. When the engine is running light or partially loaded, so that each explosion stroke is followed by one or more scavenging strokes, the suction temperature is about 50° C. as against 100° C. when running fully loaded. With a given charge of gas, therefore, the mixture will be weaker under these conditions than when fully loaded, and the efficiency should be correspondingly higher. For example, if the engine is taking 0·11 cubic foot of gas per suction, the percentage of coal gas in the charge will be about 9·6 when running fully loaded, but it will be only 8·2 when the engine is scavenging. Referring to Fig. 2 (page 266), it will be seen that the corresponding efficiencies are about 37½ per cent. and 35 per cent. respectively. There is some uncertainty about the suction temperatures on which this calculation is based (taken to be 100° C. and 50° C. respectively), but making full allowance for that, it may be said that the mean pressure realized with the same gas-charge should be at least 5 per cent. greater when the engine is scavenging than when it is running fully loaded—assuming, of course, that the strength in each case is such as to give regular and normal ignition.

A number of experiments were made with the object of testing this conclusion. Diagrams have been taken with the engine running light on half-load; and have been compared with full-load diagrams taken at the same time, the gas-consumption being measured in each case. The results of one such test have been given in Table I (page 264). Referring to that table it will be seen that a gas-charge of 0·1275 cubic foot, gave a mean pressure of 108·4 on light load, as against 102·2 when fully loaded. Further, a charge of 0·100 cubic foot at full-load, and a charge of 0·114 at light-load, corresponding in each case to a mixture strength of about 8·5 per cent. give approximately the same efficiency of 37 per cent.

These results were confirmed generally by other diagrams taken at light-load, and also by running the engine at half-load, so that most of the explosion strokes were followed by one or more scavenging strokes. The results, however, were not so consistent as in the full-load tests, the mean pressure sometimes falling short by as much as 6 per cent. of that which was anticipated from the gas-consumption. In the case of the full-load trials the mean pressure can be predicted from the gas-consumption within 2 per cent. This want of regularity is due in part to variation in the suction temperature, which was always assumed to be 50° C. after a scavenging stroke and 100° C. after an explosion. As a matter of fact, both temperatures vary to some extent with the number of explosions per minute, and possibly also a little with the gas-charge; there will be corresponding differences between the actual mixture strength and that calculated. But a more important cause of irregularity is the fact that the combustion of a scavenged charge is generally incomplete, the fuel discharged unburnt

sometimes amounting to 4 or 5 per cent. In all, four analyses were made of the exhaust when the engine was missing about every other stroke. The quantities of unburnt gas found were respectively 4·2, 3·2, 5·4, and 4·5 per cent.—average 4½ per cent. These analyses are not so accurate as those at full-load because of the dilution of the exhaust with air, and there seems to be some selective combustion as the quantities of steam and CO_2 formed in the combustion tube are usually not in the proportion obtained by the complete burning of the coal gas. But there is no question that a good deal of unburnt gas is sometimes discharged when the engine is missing explosions. The effect is quite apparent in the heat-balances at half-load, which all show a bigger deficiency than can be accounted for by radiation. Five trials at half-load showed balances unaccounted for ranging from 297 to 433 thermal units per minute, the average being 350, or about 10 per cent. on the heat-supply (higher value). Six trials at full-load with the same jacket temperature (75° C.), and taken with the same appliances, showed deficiencies ranging from − 58 to + 189 thermal units, average + 25 thermal units. The systematic errors referred to above were probably the same in all these trials. The radiation is a little greater in the full-load trials because the piston is hotter, but the difference in this respect cannot be very large. Thus, after allowing for radiation, the heat unaccounted for in the half-load trials is some 300 B.TH.U. per minute more than at full-load, and this must mainly be due to a greater proportion of unburnt gas. In the last of these trials the thermal efficiency obtained from the brake-load by addition of the mechanical losses (separately measured at the same time by observing the light-load indicated power) was 32·7 per cent. About three-fourths of the explosions were followed by scavenging strokes, and the gas-charge was 0·1285 cubic foot as measured in the holder. The average strength of mixture, calculated on the above-mentioned assumptions as to the suction temperature, was 10·0 per cent., and the corresponding efficiency 34·5 per cent. Thus the mean pressure was 5 per cent. less than that calculated. In this case 4½ per cent. of unburnt gas was found in the exhaust, and the deficiency on heat-balance was 433 thermal units per minute out of a total supply of 3740.

It is not possible to say how far the combustion is incomplete when the engine is running quite light, but it seems likely, from the high mean pressures sometimes realized under these conditions, that it may under some circumstances be more nearly complete than in the half-load tests. From a study of the latter it would appear that when allowance is made for the gas discharged unburnt, the efficiency is not much affected by scavenging provided the strength of mixture is kept the same, which implies an increase of about 15 per cent. in the gas-charge with, of course, a corresponding increase of mean pressure.

The work described in this Paper was rendered possible by the generosity of Mr W. J. Crossley, who lent the author the engine on which the experi-

ments were made. To him and to Messrs Mather and Platt (who lent the dynamo by which the engine was loaded) the author must express his gratitude. He also wishes to acknowledge the assistance which he has received from Messrs A. L. Bird and A. R. Welsh, and later from Messrs H. B. Jenkins and L. A. Fullagar, students and demonstrators in the Cambridge University Engineering Department. These gentlemen carried out practically all the experiments and reduced the results.

APPENDIX I.

Particulars of Engine, etc.

The engine works the ordinary Otto cycle with "hit-and-miss" governing, and is rated to give 40 H.P. on the brake at a speed of 180 revolutions. The ignition is by magneto.

Cylinder diameter, 11½ inches.
Stroke, 21 inches.
Compression space, 407 cubic inches.
Compression ratio, 6·37.

The engine was belted to a dynamo by Messrs Mather and Platt. When an accurate measure of brake power was required, all round-rope brakes were used on the fly-wheels. As the brake tests only lasted a few minutes no water cooling was necessary. This is a great convenience, and is an incidental advantage of the method of gas-measurement.

Cambridge coal-gas was used throughout as fuel. The following Table gives its average composition:

Table IV.

—	Percentage by volume	O required for combustion	Steam produced	CO_2 produced
H	47·2	23·6	47·2	
CH_4	35·2	70·4	70·4	35·2
Heavy hydrocarbons	4·8	22·6	16·0	14·4
CO	7·15	3·6	—	7·15
N	5·4	—	—	—
Other gases ...	0·25	—	—	—
	100·00	120·2	133·6	56·75

The higher calorific value varies between 630 and 680 B.TH.U. per standard cubic foot, the lower value between 570 and 620.

APPENDIX II.

Estimate of Suction Temperature.

From anemometer experiments it is found that, with a medium jacket temperature and with the engine exploding every time, the volume of mixed gas and air taken in is 0·85 times the stroke volume*. If we assume a normal barometer and an outside temperature of 15° C. the *quantity* taken in (reckoned in standard cubic feet) is $0{\cdot}85 \times \frac{273}{288} = 0{\cdot}805$ times the stroke volume. This is mixed with the contents of the compression space and possibly also with some exhaust products which have backed in from the exhaust pipe. The volume of the compression space is 0·187 of the stroke volume; the pressure of the gases is atmospheric, and their temperature may be taken as that resulting from the nearly adiabatic expansion which took place at release. The release-pressure is between 50 and 55 lbs. per square inch absolute according to strength of mixture. We may assume 52 lbs. The volume at release is 0·90 times the total cylinder volume; assuming that the suction temperature was 100° C., this gives a temperature just before release of about 1190° absolute. The expansion down to atmospheric pressure, which occurs very rapidly after release, will reduce this in the ratio $\left(\frac{14{\cdot}7}{52}\right)^{0{\cdot}26}$ if γ be taken as 1·35. The result is that the temperature in the cylinder just after release is about 860° abs., or, say, 600° C., and this probably does not vary more than 50° C. either way, under different conditions. Assuming that this temperature does not change materially during the exhaust stroke†, it follows that the contents of the compression space amount to $\frac{273}{860} \times 0{\cdot}187 = 0{\cdot}06$ of the stroke volume, reckoned in standard cubic feet. The total cylinder contents at the end of the suction stroke would therefore, at standard temperature and pressure, occupy 0·865 of the stroke volume, and the volume they actually occupy is 1·187 times the stroke volume. Thus the mean temperature is $\frac{1{\cdot}187}{0{\cdot}865} \times 273 = 375°$ abs., or 102° C.

The chief error in this calculation lies in the assumption that the products of combustion mixed with the incoming charge are only those left in the clearance space. This would be strictly true if the exhaust-valve shut before the inlet-valve opened. But as a matter of fact the inlet-valve begins to open slightly before, and the exhaust-valve is not completely closed until slightly after the in-centre. For about 50° of crank angle both valves are open together, though not fully open. During the last stages of the exhaust

* See the author's paper on "The Indicated Power and Mechanical Efficiency of the Gas Engine," *Proceedings*, 1907, Part 4, page 863. Page 226 of this volume; and also "On the Measurement of Gas Engine Temperatures," *Philosophical Magazine*, Jan. 1907. Page 214 of this volume.

† The temperature will of course fall to some extent, but the gas which is ultimately left in the clearance space is throughout the exhaust stroke in contact with the piston which has a temperature of 300°–400° C. This portion of the gas probably does not lose heat very rapidly,

stroke, therefore, the engine may pass some exhaust-gas into the inlet-pipe, which gas will subsequently be drawn into the cylinder along with the incoming charge; and in the early stages of the suction it may draw some exhaust-gas from the exhaust-pipe. If this happens, the quantity of products of combustion mixed with the incoming charge is greater than the contents of the clearance space, and the total cylinder contents are underestimated in the above calculation, leading to too high an estimate of the suction temperature. On the other hand, it is possible that during the overlap of the two valves there is (owing to the inertia of the gases in the exhaust-pipe) a current constantly flowing into the exhaust-pipe and continuing even after the centre has been passed. Such an action would reduce the quantity of exhaust gases in the charge, and if it occurs the suction temperature is higher than is given by the above calculation. With the arrangements used by the author the inertia of the exhaust gases cannot have much effect, because the exhaust-gas calorimeter, a vessel of considerable volume, is placed close up to the engine, and constitutes an enlargement on the exhaust-pipe which would tend to reduce any inertia effects. It is probable that exhaust gases back in to some extent, and that the suction temperature is rather less than 100° C.* Fortunately an error in the estimate of this temperature only affects absolute values of temperature and of strength of mixture. For example, in the calculation of the energy of the gases at release, while an increase in the suction temperature would give rise to a proportionate increase in the temperature at the end of expansion, it would also be accompanied by a proportionate decrease in the estimate of the quantity of gas present. The energy in the gas would remain the same except for the small change in specific heat consequent on the change of temperature.

When the engine is running light the suction temperature will be nearly the same as it is when the engine is motored round without firing. The temperature under these conditions can be measured by platinum thermometers, a method not available when the engine is firing because the wire melts†. The author has made such measurements on the Crossley engine, and finds that with a cold jacket and an external temperature of 17° C. the temperature at the end of the suction stroke is within 2 or 3 degrees of 40° C.‡ With the hotter jacket used in these experiments, and the higher piston temperature which exists even when the engine only fires once in

* The cooling of the gases left in the cylinder after release also contributes to lowering the temperature somewhat.

† Since the above was written, Messrs Callendar and Dalby have devised an ingenious method of overcoming this difficulty, and have measured the temperature of the gases during the suction stroke at a point near the inlet-valve. They found that in the engine which they tested the suction temperature at full-load varied from 95° C. to 125° C. with strength of mixture. The average temperature is rather higher than that calculated above; this is no doubt due to the larger clearance space, which was 1/3·68 of the stroke volume against 1/5·37 in the engine tested by the author. See *Proceedings of the Royal Society*, A, vol. 80 (1907), p. 57.

‡ *Philosophical Magazine*, Jan. 1907. Page 214 of this volume.

six or seven times, the temperature will be a little greater. 50° C. must be very close.

In considering the calculations of mixture strength, it is to be remembered that the proportion of gas to air in the mixture drawn in is accurately known from anemometer experiments. The total volume taken per cycle in full load running varies between 0·825 and 0·875 times the stroke volume, according to the piston and jacket temperature. Under medium conditions it is 1·07 cubic feet reckoned at the external temperature and pressure. The fraction of coal gas in the mixture taken in is therefore equal to the volume taken per cycle, as measured by the gas-holder, divided by 1·07. Working light, the volume of mixture taken is 1·135 cubic feet. The proportion of fuel in the mixture as it is in the engine depends upon the dilution of the air and coal gas drawn in by products of combustion or air, and, as already pointed out, the extent of this dilution is rather uncertain. Analysis of the exhaust gases of course gives no information about it.

APPENDIX III.

Calculation of Ideal Efficiency.

It is assumed that the gas has a calorific value of 600 B.TH.U. per standard cubic foot, the products of combustion being cooled to a temperature of 100° C. At the end of the suction stroke the valves are all closed and the cylinder is then full of the mixture of coal gas and air (assumed to be dry) which has been drawn in *plus* products of the previous explosion amounting to 7 per cent. of the whole, the temperature being 100° C. and the pressure 14·7 lbs. per square inch. The mixture is compressed adiabatically, and is fired at the in-centre, the combustion being complete and instantaneous. The products of the combustion are then expanded without loss of heat to the out-centre, when the exhaust-valve is opened.

The efficiency is calculated for two mixtures, of which the following are particulars:

	A	B
Volume of coal gas taken per suction (cubic foot at external temperature and pressure)	0·1	0·13
Percentage of coal gas in mixture drawn in	9·4	12·2
Percentage of coal gas in mixture in engine	8·8	11·4

Products of combustion of 1 cubic foot of mixture:

	A	B
Steam	0·125	0·163
CO_2	0·053	0·069
N and O	0·793	0·732
Total	0·971	0·964

The analysis of the products of combustion is calculated from the average composition of the coal gas.

The internal-energy curves, Fig. 4 (page 269), have been calculated from these compositions, using the following values for the specific heats:

Table V.

Temperature ...	800°	1400°	1900°
Air	19·9	22·0	23·5
H_2O	26·0	31·0	39·6
CO_2	35·2	41·4	46·1

The figures are the mean values of the specific heats at constant volume up to the temperature in question, expressed in foot-pounds per standard cubic foot of the gas. Those at 800° and 1400° are the results of Holborn and Austin* and of Holborn and Henning†, obtained by external heating at constant pressure. These are probably correct within 3 per cent. The figures at 1900° are from Langen's explosion experiments‡; they are probably rather too high because of incomplete combustion and loss of heat, but they are the best available. The values given by Clerk§ are for mixed gases; they are rather higher than those calculated from the above figures at 800° and 1400°.

It is most convenient to follow what happens to a standard cubic foot of the mixture in passing through the engine. The mixture contains $\frac{0{\cdot}805}{0{\cdot}865} = 0{\cdot}93$ of its volume of gas and air, the rest being products of combustion. Starting at 100° C., or 373° absolute, it is compressed adiabatically 6·37 times. The temperature rises to $373 \times (6{\cdot}37)^{0{\cdot}4} = 780°$ absolute. The rise of temperature is 407° C. and the work done is $19 \times 407 = 7700$ foot-pounds, since the thermal capacity is constant and equal to 19 foot-pounds per cubic foot nearly. This is the internal energy at the end of compression with either mixture.

The pressure at the end of compression is:

$$14{\cdot}7 \times (6{\cdot}37)^{1{\cdot}4} = 196 \text{ lbs. per sq. in.}$$

(I) *Strong Mixture.* In this case 12·2 per cent. of the mixture drawn in is coal gas. In the mixture as it exists in the engine (after mixing with the products) the percentage of coal gas is $12{\cdot}2 \times 0{\cdot}93 = 11{\cdot}4$. The heating value of the gas in 1 standard cubic foot is therefore:

$0{\cdot}114 \times 600 \times 778 = 53{,}000$ foot-pounds
Add to this the work of compression = 7,700 ,,
Internal energy after explosion = 60,700 ,,

* *Researches of the Reichsanstalt*, vol. 4, 1905.

† *Annalen der Physik*, vol. 23 (1907).

‡ *Zeitschrift des Vereines Deutscher Ingenieure*, vol. 47 (1903).

§ "On the Limits of Thermal Efficiency in Internal Combustion Motors," *Proceedings of the Institution of Civil Engineers*, vol. 169, p. 121.

After explosion the standard cubic foot of mixture becomes 0·964 cubic foot of products. Thus, the internal energy of the products after explosion is $\frac{60{,}700}{0{\cdot}964} = 63{,}000$ foot-pounds per cubic foot, reckoning from 100° C. From the curve, Fig. 4 (page 269), the corresponding temperature is 2210° C., or 2480° absolute. The pressure is

$$0{\cdot}965 \times \frac{2480}{780} \times 196 = 600 \text{ lbs. absolute.}$$

The expansion curve is computed by trial and error. We assume an expansion curve of the form pv^n = constant. The true adiabatic will not be of this form, because the specific heat is not constant. But if n be so chosen that no heat is lost on the whole during expansion, the loss in the first portion being balanced by an equal gain in the second, we shall have a sufficiently close approximation to the real adiabatic. If we take $n = 1{\cdot}20$, the temperature at the end of expansion is $\frac{2480}{(6{\cdot}37)^{0{\cdot}20}} = 1713°$ absolute, or 1440° C. From the curve the energy at this temperature is read off to be 33,700 foot-pounds and the loss of energy in expansion is 63,000 — 33,700 = 29,300 foot-pounds. The work area under this curve is most simply computed by noting that it is the adiabatic of a gas for which γ is constant and equal to 1·20, and for which the specific heat is therefore 38·7 foot-pounds. The fall of temperature in expansion is 767° C., and the work done in expansion is therefore 38·7 × 767 = 29,700 foot-pounds per standard cubic foot of products, which is slightly more than the loss of energy, showing that along this expansion line there must be some gain of heat on the whole. If the index 1·21 be tried, corresponding to an average specific heat of 36·9 foot-pounds, it will be found that the loss of energy in expansion is 30,500 foot-pounds and the work done 29,500, corresponding to a slight loss of heat in expansion. We may take the index 1·20 as sufficiently near. Since there is only 0·965 cubic foot of products for every cubic foot of original mixture, the work done in expansion per cubic foot of mixture is 29,700 × 0·965 = 28,600 foot-pounds. The nett work performed in the cycle, after deducting the work of compression (7700) is 20,900 foot-pounds. Since the heating value of the gas is 53,000 the efficiency is 39·4 per cent.

This is the efficiency of an ideal engine using the actual working substance with adiabatic compression and combustion, and in which the expansion line follows the course $pv^{1{\cdot}20}$ = constant. As already pointed out, this is not an adiabatic expansion line, but possesses the property that no heat is lost in the course of it, the loss of energy in the early parts being balanced by an equal gain in the latter parts. The true adiabatic for which n is an increasing quantity, at first greater and afterwards less than 0·20, will at first be above the assumed line, will then cross it, and will finally be below it. The final temperature after adiabatic expansion will

therefore be less than after the assumed expansion. Since no heat is lost to the walls (on the whole) in either case, it follows that the work area under the adiabatic line must be slightly greater than under the line $pv^{1\cdot20}$ = constant. Thus the efficiency calculated above is a little lower than that of an engine using real adiabatic expansion, but it is easy to prove that the difference is inappreciable.

(II) *Weak Mixture.* It is unnecessary to go through all the steps of the calculation with the weaker mixture. The following are the figures:

Compression work (as before) ...	= 7,700	per cubic foot of mixture from 100° C.
Gas heat 0·094 × 0·93 × 600 × 778	= 40,700	
Internal energy after explosion ...	= 48,400	
Energy per cubic foot of products...	$= \frac{48,400}{0\cdot973} = 49,800$	
Corresponding temperature from curve	= 1940° C. = 2210° abs.	

Assuming the expansion curve $pv^{1\cdot24}$ = constant, the final temperature is 1418° absolute, or 1145° C. The energy is then 24,000 from the curve.

Loss of energy	= 25,800
Work area under expansion curve 32·3 × 792	= 25,600
Work of expansion per standard cubic foot of mixture 25,600 × 0·975	= 24,950
Net work " " " " "	= 17,250
Heat supply	= 40,700
Efficiency	= 42·4%

In working out the heat supply and work done *per cycle* in the engine, it is only necessary to multiply the figures by 1·09 the number of standard cubic feet of mixture present in the cylinder.

The difference between the efficiencies with the two mixtures is mainly due to the fact that a greater rise of temperature and therefore of pressure (in proportion to the fuel used), is obtained when exploding the weak than when exploding the strong mixture. The temperature rises are 1700° and 1430° respectively and have a ratio 1·19, but the amounts of fuel supplied have a ratio 1·30. The pressure falls rather more rapidly in the adiabatic expansion of the weaker mixture, but the difference in this respect is not very material. The determining factor is the initial pressure produced by the explosion.

APPENDIX IV.

Analysis of Exhaust Gases.

The gas was dried by bubbling through concentrated sulphuric acid and by passing through a large calcium chloride tube, and the CO_2 was removed by passing over soda lime in a large U tube. The combustion-tube was of the usual type, and was filled with copper-oxide made from pure copper wire. After passing the combustion-tube the gas was led through a calcium chloride tube, then through two potash bulbs, and finally through another calcium chloride tube in order to absorb any

traces of moisture removed from the potash bulbs. The calcium chloride tubes and potash bulbs were separately weighed before and after the experiment, and a sealed potash bulb and a sealed calcium chloride tube, which were kept in the neighbourhood of the absorption bulbs, were also weighed at the same time in order to make allowance for any changes in weight due to condensation or evaporation of moisture on the glass surfaces. The gain in weight of the last calcium chloride tube was credited entirely to CO_2. It was found that practically the whole of the CO_2 was absorbed in the first potash bulb; changes in weight in the second were quite slight.

The following table (p. 285) gives the result of all the analyses which were made after the apparatus had been got into satisfactory working order, with two exceptions, which were rejected as obviously wrong. The quantity of gas used (stated in the first column of figures) is the actual volume as measured in the containing vessel. For purposes of calculation this is taken to be the same as the volume under standard conditions, the correction for temperature being sufficiently nearly compensated by that for pressure, since a head of about 2 feet of water was required to force the gas through the apparatus. Column 2 gives the nett increase in weight of the first calcium chloride tube after the gas has passed the combustion tube. Column 3 gives the percentage ratio which the steam found bears to the amount of steam which would have been produced by the complete combustion of the coal gas known to have been originally present in the mixture before it was burnt in the engine. In the 4th and 6th columns corresponding figures are given for the CO_2; the weight in the 4th column being the nett increase in weight of the two potash bulbs and calcium chloride tubes combined after deducting any increase in the sealed bulbs. The final column is the mean of the percentages in columns 3 and 5.

The percentages in columns 3 and 5 are estimated in a manner which will be clear from the following example: In test No. 3 the measured gas-consumption is 0·120 cubic foot per explosion. From anemometer experiments it is known that this corresponds to 11·25 per cent. of coal gas in the mixture drawn in (*see* Appendix II, p. 278). Thus, in this case, 1 volume of coal gas is burnt in 7·9 volumes of air, and if completely burnt would take 1·2 volumes of oxygen, and would yield in the exhaust (*see* Analysis of coal gas, page 285)

Steam	...	1·33 volumes
CO_2	...	0·57
N and O		6·70
Total	...	8·60 volumes of exhaust gas

In the exhaust gas as collected from the calorimeter the steam is condensed, and about half of the CO_2 is dissolved out by the calorimeter water. Thus, there are about 7·0 volumes of exhaust gas for every volume of coal gas burnt. In the analysis 1·5 litres are taken, corresponding to 0·21 litre of coal gas, which if completely burnt would give 0·28 litre, or 0·266 gramme of steam and 0·12 litre or 0·236 gramme of CO_2. In fact,

Table VI.

Test No.	Nature of gas analysed	1	2	3	4	5	6
		Quantity of gas used	Steam		CO_2		Percentage of unburnt coal gas
		litres	mg.	per cent.	mg.	per cent.	
1	Exhaust from Boys' Calorimeter $\frac{air}{gas}=7{\cdot}1$	1·53	0·8	0·3	− 0·4	− 0·1	0·1
2	Exhaust from Junker Calorimeter $\frac{air}{gas}=9{\cdot}6$	1·74	3·0	1·5	1·2	0·5	1·0
3	Engine exhaust. Full-load. Gas 0·1204 (11·25 per cent.)	1·5	4·0	1·8	2·8	1·2	1·5
4	Engine exhaust. Full-load. Gas 0·1228 (11·46 per cent.)	1·94	2·3	0·7	0·7	0·2	0·4
5	Engine exhaust. Full-load. Gas 0·1002 (9·37 per cent.)	1·63	1·4	0·8	2·5	1·3	1·1
6	Engine exhaust. Full-load. Gas 0·1320 (12·3 per cent.)	1·5	...	...	1·8	0·6	0·6
7	Engine exhaust. Half-load. Gas 0·1212 (11·3 per cent.)	1·5	2·8	2·7	6·3	5·7	4·2
8	Engine exhaust. Half-load. Gas 0·1199 (11·2 per cent.)	1·5	2·8	2·7	3·5	3·2	3·0
9	Engine exhaust. Full-load. Gas 0·13 (12·2 per cent.)	1·0	1·3	0·7	0·3	0·1	0·4
10	Same samples as 9	1·72	1·5	0·5	− 0·1	...	0·2
11	Engine exhaust. Full-load. Gas 0·1 (9·4 per cent.)	2·0	1·2	0·5	1·5	0·7	0·6
12	Engine exhaust. Half-load. Gas 0·106 (10·0 per cent.)	1·56	3·3	4·2	5·6	6·8	5·5
13	Engine exhaust. Half-load. Gas 0·1285 (12 per cent.)	1·9	6·8	6·7	2·9	2·3	4·5

4 milligrammes, or 1·8 per cent. of the steam, and 2·8 milligrammes, or 1·2 per cent. of the CO_2, are formed in the combustion tube.

In order to check the accuracy of the analyses, tests were made before and after the above series on an artificial mixture in the proportion of 1 part of coal gas, 300 of air. The results were as follows:

Table VII.

No.	Quantity of gas used	Steam	CO_2	Observed percentage of coal gas	
				From steam	From CO_2
	litres	mg.	mg.		
(1)	3·19	14·3	11·7	0·41	0·33
(2)	2·56	7·9	9·4	0·28	0·32

These two tests show that the CO_2 gives a more accurate measure of the coal gas than the steam, but it is probably better to take the mean of the two in the analysis of the engine-exhaust, as it is uncertain whether there is not some selective combustion.

Turning to the table of analyses above, it will be seen that the combustion in both calorimeters is very nearly complete. In the tests at half-load (Nos. 7 and 8), in which the engine was firing every other time on the average, there is no doubt that between 3 and 4 per cent. of the gas was discharged unburnt. In test No. 5 the jacket temperature at exit was only 26° C., in all the others it was about 70° C.; this makes no difference to the amount of unburnt gas.

APPENDIX V.

Table VIII.

Efficiency Tests.

No. of trial	Lower calorific value	Gas charge: Cubic foot per suction	Gas charge: Explosion-Cycles	Gas charge: Per cent. of mixture	Gas charge: Per cent. corrected	Thermal efficiency	Efficiency calculated from Fig. 1	Remarks
1 *a*	600	0·1275	1·0	11·1	11·1	32·5	32·8	Diagrams only
1 *b*	,,	0·1147	,,	10·0	10·0	34·7	34·6	
1 *c*	,,	0·1005	,,	8·75	8·75	36·5	36·6	
2	582	0·1180	0·8	10·0	9·7	34·8	35·1	Brake and diagrams
3	575	0·1006	0·9	8·6	8·3	36·8	37·3	Brake
4 *a*	600	0·1304	0·8	11·0	10·8	34·0	33·0	Brake
4 *b*	,,	0·1066	0·9	9·1	9·1	35·7	36·0	
5 *a*	600	0·122	1·0	10·6	10·6	34·5	33·8	Diagrams
5 *b*	,,	0·099	,,	8·5	8·5	36·8	37·0	
6	610	0·131	0·8	11·1	11·3	31·7	32·5	Brake
7	600	0·1151	1·0	10·0	10·0	34·7	34·6	Diagrams
8 *a*	605	0·1012	0·9	8·6	8·7	36·5	36·6	Brake
8 *b*	,,	0·128	0·8	10·6	10·7	33·4	33·5	
9 *a*	605	0·103	0·9	8·8	8·9	35·5	36·4	
9 *b*	,,	0·127	0·9	10·6	10·7	33·0	33·3	

Trials under the same numerals are comparative tests, made at the same time. In trials (1), (4) and (5) the calorific value was assumed to be 600 thermal units per standard cubic foot, which was the average value at the time that the tests were made; in the others the calorific value was measured at the time. The gas-consumption given is the volume as measured in the gas-holder. "Per cent. of mixture" is the fraction of coal gas in the mixture as it is in the engine after dilution with the contents of the

clearance space. Where (as in trial No. 8) the engine was missing an appreciable number of explosions, allowance is made in calculating the percentage composition for the fact that the dilution is greater after a scavenging stroke, and the average percentage is given. "Per cent. of mixture corrected" is the equivalent percentage of coal gas having a calorific value of 600 B.TH.U. It is proportional to the heat-supply per standard cubic foot of cylinder contents.

"Thermal Efficiency" is the ratio of indicated power to heat-supply (lower value), indicated power being taken from the positive loop of diagram only, without any deduction for pumping.

There is a possible error of about 2 per cent. in the indicated power, whether from diagrams or brake, and of about the same amount in the heat-supply. An error of 4 per cent. in the ratio of these quantities may therefore occasionally occur, equivalent to about 1·3 in the efficiency. All the observations of efficiency agree with the straight line in Fig. 2 (page 266) within that limit, and the probable error in calculating the mean pressure from that straight line does not exceed 2 per cent., assuming the heat-supply to have been accurately determined.

APPENDIX VI.

Note to Table IX (*page* 288).

B. Temperature of jacket-water at exit in degrees Centigrade.

C. Standard cubic feet.

D. Higher value per standard cubic foot, obtained at time with Junker or Boys' calorimeter, occasionally with both. This is probably between 1 and 2 per cent. too low on the average, owing to meter errors. (*See* note on page 274.)

E. British thermal units per minute.

F. Usually calculated from the thermal efficiency (taken from curve, Fig. 1, page 265) and from the known mechanical losses. In tests 47 and 48 at half load the brake-power was measured.

H. British thermal units per minute.

J. The calorimeter was of the spray type and consisted of a flanged cylinder about 3 feet by 1 foot by 1 foot 6 inches internal diameter. The water was led first into the space surrounding the cylinder, which served as a jacket, and was then taken to two bats-wing gas-burners fitted into the exhaust-pipe close to the exhaust-valve, where it formed two flat sheets athwart the bore of the exhaust-pipe. The exhaust-pipe was carried straight through one end of the calorimeter cylinder, which was placed slightly sloping, to near the other end. The gases thus encountered a sheet of cold water, and were efficiently cooled, immediately on leaving the engine. The water was drawn off at the bottom and its amount and rise of temperature measured. The gases, after leaving the exhaust-pipe within the cylinder, pass backward up the latter, and escape by a pipe fixed to the upper cover. Baffle plates are fitted across the calorimeter inside to assist the gases in shedding the suspended water as they pass back up the calorimeter.

L. This represents the heat which is evolved by the exhaust gases after leaving the calorimeter, during the process of cooling to temperature of 15° C. The quantity of gas discharged per minute is known from anemometer measurements, probably within 3 or 4 per cent. This is saturated with water vapour when it leaves the calorimeter, and, its temperature and pressure being known, its internal energy per cubic foot can readily be calculated from steam

Table IX.

Heat Balances.

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O
Test No.	Explosions / Cycles	Temperature of jacket °C.	Gas per suction	Higher calorific value	Heat-supply	Brake horse-power		Jackets		Exhaust calorimeter		Exhaust gases		Unaccounted for	
						B.TH.U.	Per cent.	B.TH.U.	Per cent.	B.TH.U.	Per cent.	B.TH.U.	Per cent.	B.TH.U.	Per cent.
1	0·93	78	0·093	650	4,835	1,406	29	1,190	25	1,932	40	357	7	− 50	− 1
2	0·91	21	0·097	670	5,115	1,419	28	1,263	25	1,886	37	367	7	180	3
4	0·98	76	0·103	674	5,875	1,713	29	1,576	27	1,680	29	480	8	426	7
6	0·97	81	0·113	657	6,280	1,759	28	1,818	29	2,048	33	670	11	− 15	0
7	1·0	47	0·094	657	5,335	1,526	29	1,470	28	1,832	34	558	10	− 51	− 1
8	0·94	48	0·103	678	5,640	1,683	30	1,558	28	1,758	31	624	11	17	0
9	0·97	49	0·113	662	6,235	1,716	28	1,820	29	1,872	30	763	12	64	1
10	0·99	49	0·122	662	6,750	1,758	26	2,108	31	1,990	30	852	13	42	0
11	0·95	76	0·122	661	6,640	1,784	27	1,942	29	2,048	31	784	12	82	1
12	0·95	47	0·122	661	6,610	1,739	26	2,088	32	2,090	32	754	11	− 61	− 1
13	0·99	31	0·094	645	5,335	1,546	29	1,502	28	1,819	34	636	12	−168	− 3
14	0·96	31	0·122	674	6,785	1,753	26	2,035	30	2,032	30	758	11	217	3
15	0·98	29	0·107	658	5,900	1,655	28	1,712	29	1,802	31	696	12	35	0
33	0·93	75	0·093	643	4,830	1,405	29	1,186	25	1,698	35	546	11	− 5	0
34	0·92	75	0·108	643	5,460	1,534	28	1,478	27	1,840	34	616	11	− 8	0
35	0·91	75	0·117	643	5,800	1,558	27	1,698	29	1,940	34	662	11	− 58	− 1
36	0·88	40	0·119	638	5,730	1,461	25	1,876	33	1,902	33	604	11	−113	− 2
37	0·89	75	0·120	638	5,735	1,505	26	1,720	30	1.850	32	686	12	− 26	0
39	0·87	75	0·118	661	5,880	1,563	27	1,672	29	1,878	32	717	12	+ 50	0
41	0·92	75	0·105	669	5,420	1,522	28	1,460	27	1,827	34	422	8	+189	3
38	0·47	75	0·119	661	3,335	781	23	792	24	1,138	34	303	9	321	10
40	0·49	75	0·117	669	3,440	820	24	872	25	1,228	36	223	6	297	9
46	0·50	75	0·111	671	3,215	784	24	802	25	1,068	33	246	8	315	10
47	0·47	75	0·100	686	2,950	670	23	630	21	1,032	35	223	8	395	13
48	0·50	75	0·121	686	3,740	854	23	965	26	1,068	29	420	11	433	11

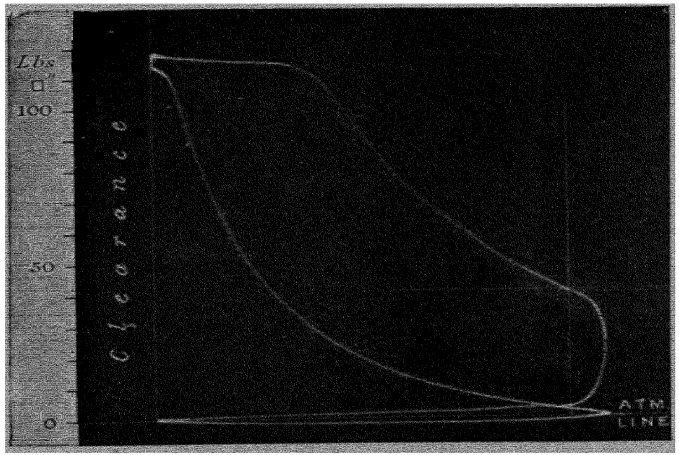


Fig. 7. Diagram for Release-Pressure. 20 Explosions. Gas 0·1278.

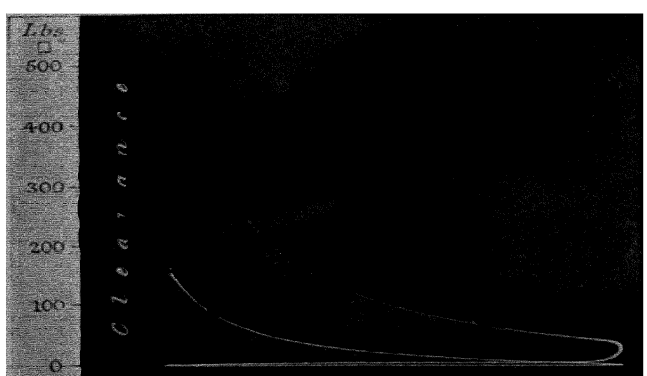


Fig. 8. 20 Explosions. Gas about 0·11.

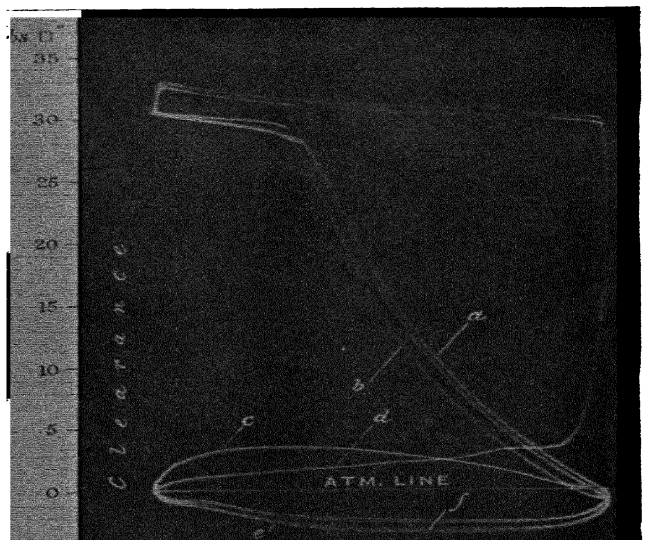


Fig. 6. Light Spring Diagram (Crossley Engine).

a. Compression Lines.
b. Expansion Lines, idle strokes.
c. Exhaust after idle stroke.
d. Exhaust after Explosion.
e. Suction, taking Gas.
f. Suction, taking Air only.

tables. The internal energy at 15° C. can be calculated in the same way The difference of these energies, plus the work done by the atmospheric pressure in the contraction of the gas as it cools, is equal to the heat evolved per standard cubic foot. The temperature of the gases after leaving the calorimeter varied from 34·1° to 56·2° C.

N. Includes radiation losses, unburnt gas, and errors of observation.

Professor Hopkinson [in introducing the paper] gave a short abstract of his paper, in the course of which he exhibited upon the screen the diagrams which are reproduced in facsimile in Figs. 6, 7, and Fig. 8, Plate I. The first of these was shown as evidence of the accuracy of the author's indicator, which the observations of pressure referred to in the paper were made. It was a light-spring diagram on the scale of about 13 lbs. per square inch to the inch. The very dead-beat character of the instrument was shown by the exhaust line *d* following an explosion. There was no trace of oscillation on this line though the scale of the diagram was so open. The absence of friction was shown by the separation of the various compression and expansion lines *a*, *b*, and by the separation of the two suction lines *e*, *f*. Fig. 7 was exhibited as a sample of the diagrams from which the release pressure measurements were made, and Fig. 8 was a facsimile of an ordinary diagram such as those upon which the measurements of efficiency were based. In the case of the last diagram the author drew attention to the separation of the exhaust and suction lines as further evidence of the accuracy of the instrument and of the absence of back-lash. The pressure difference between these lines could be measured by a microscope, and was found to be the same within $\frac{1}{2}$ lb. per square inch as that given by the diagrams with a more open scale, such as Figs. 6 and 7.

Professor HOPKINSON [in replying to the discussion], said he would very much like to carry out tests upon producer-gas, but he had chosen coal-gas deliberately for the experiments described in the Paper, because it was possible to be sure that for an hour or two its calorific value would remain constant, because its volume was comparatively easy to measure—there was not so very much of it—and because it was possible to get a wide range of mixture without any difficulty. It gave a sharp and clear ignition as the diagrams showed. Of course, for practical application one ought thoroughly to experiment with producer-gas, though there did not appear to be any reason why the effect of strength of mixture should differ materially from that found with coal-gas. As to the effect of water vapour on the calorific value of the gas, he had taken the gas for the calorimeter out of the same mains as he took the gas for the engine, and he measured them under the same conditions, and presumed that he was getting the true value for the stuff he was using, whether gas, or partly gas and partly water vapour.

Mr Davis had referred to the possibility that by retarding the ignition with the stronger charges slightly better efficiencies might have been obtained. That was quite possible. He did not experiment on the effect of the time of ignition on efficiency. The real point of the Paper was to find the effect of varying one condition, namely, mixture strength, the other conditions remaining the same, and to refer it to its causes. One of the causes, of course, was heat loss, and possibly he might have altered the difference in the heat loss as between the two mixtures by altering the time of ignition, but that would not have affected the point.

Professor Hopkinson [after the discussion], in reply to Mr Robinson's remarks,

wrote that the increase in economy with weak mixtures was, of course, obtained at the expense of power, and it was moreover discounted to a considerable extent by the reduction in mechanical efficiency. If the only matters to be considered were power and economy, it was probable that it would in many cases pay to use stronger mixtures than were generally used in practice. These, however, were not the only factors affecting the question; the cost of repair and certainty of running were probably more important than either. There could be no doubt that wear-and-tear on the engine and danger of pre-ignition were both greatly increased by the use of heavy charges. The higher pressures and sharper explosions conspired with the higher temperatures to produce this result. The temperature of such parts of the engine as the exhaust-valve and piston increased to a remarkable extent as the strength of mixture was increased; these temperatures, in fact, rose much more than in proportion to the heat supply. The result was greatly increased stresses in the unequally heated parts of the engine together with an increased danger of pre-ignition in large engines. For these reasons he (Professor Hopkinson) was of opinion that it would be found to pay in practice to run engines of the size of that discussed in the paper or of larger size with as weak a mixture as would give certain ignition.

With reference to Mr Royds' remarks the author was at first inclined to draw from Mr Royds' inference that the observations in the paper indicated an approach to maximum efficiency as the strength of mixture was reduced. His further experiments, however, had convinced him that no such inference could be drawn from the experiments, which only pointed to a steady and apparently constant increase in efficiency as the strength of mixture was reduced. There was clearly no justification for selecting special observations, as Mr Royds had done in his figure, and drawing a line through them.

It was, of course, true that a fall in efficiency was usually observed when the gas-consumption was reduced beyond a certain point. But that was due to slow or imperfect ignition, and proved nothing as to what might happen if the diagram could be kept of the normal form by stratification or otherwise. In the observations described in the paper the explosion line was always nearly vertical, and they gave strong support to the inference, based on theoretical grounds, that so long as that condition held, the efficiency would increase as the heat supply was reduced, tending to a value but little below the air-cycle value for a very small heat supply.

With regard to the incomplete combustion which was found after a cut-out cycle, the author was of opinion that Mr Royds' suggestion as to the trapping of gas between the gas and air-valves might possibly account for it, at any rate in part. The gas-valve closed before the air-valve but after the out-centre, and the space in question between the gas and air-valves was large enough to contain the unburnt gas which had been found in the exhaust. There were, however, one or two difficulties in the way of accepting this as a complete explanation, notably that the full mean pressure corresponding to the gas-charge was occasionally realized when the engine was cutting out explosions. As he had stated in the paper, the phenomenon was a puzzling one and, whatever the actual cause, it was quite likely that it was peculiar to the engine under test. He did not in the paper draw any general inference from it, merely stating that the unburnt gas accounted for the otherwise inexplicable fact that the mean pressures realized at half-load were not such as would have been expected from the measured gas-consumption and strength of mixture, and that conclusion was of course independent of the cause of the incomplete combustion.

20

ON HEAT-FLOW AND TEMPERATURE-DISTRIBUTION IN THE GAS ENGINE.

["PROC. INSTITUTION OF CIVIL ENGINEERS," 1909.]

IN the course of the expansion and exhaust-strokes of the gas engine from one-quarter to half of the heat developed in the explosion is discharged into the cylinder-walls and piston. Recent researches have shown that from a thermodynamic point of view this great loss of heat is not very important except in quite small engines; the investigations of Mr Dugald Clerk* and others having proved that even if it were completely suppressed the work done by an engine developing 60 H.P. would only be increased by one-eighth part. In its effect upon the mechanical design and upon the practical working of the motor, however, the heat-flow into the walls and piston of the engine is of the greatest possible importance. It is necessary in long-continued working to remove the heat from outside as fast as it is put into these parts from within; and this necessitates first the provision of a water-jacket or other artificial means of removing the heat from the outer surface, and second, the existence of a sufficient temperature-gradient in the metal to cause the heat to flow from the places where it is generated to the places where it is dissipated. If the distance through which the heat has to travel is great, as for example from the centre of an uncooled piston to the water-jacket, the fall of temperature necessary to maintain the flow is correspondingly great. Thus the uncooled parts get very hot, their temperature increasing with the size of the engine. These high local temperatures give rise to practical difficulties of two kinds: first, there is the mechanical problem of designing a structure in which the temperatures at places separated by a few inches may differ by 300° or 400° C.; and secondly, there is the danger of pre-ignition due to the fact that a piece of metal whose temperature exceeds 500° C. may in various ways cause spontaneous ignition of the charge. It is probable that unequal expansion and pre-ignition between them account for a large proportion of the failures which are still frequent in the working of large gas engines.

The present paper is concerned chiefly with an experimental investigation into the temperatures reached by the hotter parts of a gas engine of considerable size, the conditions which determine these temperatures,

* "On the Limits of Thermal Efficiency in Internal Combustion Motors," *Minutes of Proceedings Inst. C. E.*, vol. 169, p. 143.

and the circumstances in which pre-ignition may be caused by hot metal. The means for the investigation, which was conducted at the Engineering Laboratory, Cambridge, were provided for the most part by Mr F. W. Crossley, who in the summer of 1906 placed at the author's disposal one of the well-known engines built by his firm. The experiments were carried out and the results were reduced by research students at the Laboratory, among whom must be particularly mentioned Messrs A. L. Bird, A. R. Welsh, H. B. Jenkins and L. A. Fullagar. To the enthusiasm and perseverance of these gentlemen is in large measure due any value which the work here described may be found to possess.

Before describing the experiments and discussing the results it will be well to consider the temperature-distribution in an engine in a general way, in order to get an idea of the order of magnitude of the quantities involved and of the points that require investigation. Under any given conditions of working heat is at each instant passing into the cylinder-walls and piston from the hot gases at a certain rate per square foot, which fluctuates throughout the cycle, being greatest just after the explosion and falling to zero, or perhaps reaching a small negative value in the suction-stroke. If this rate of heat-flow were known for every instant of time, and for every point of the inner surface, it would be possible by a sufficiently elaborate mathematical analysis to determine the temperature at every point of the metal; the other conditions of the problem being that certain portions of the outer surface are maintained by the cooling water at a known temperature, while over the remaining (uncooled) portions the rate of heat-loss is either negligible or can be calculated from the temperature according to the laws governing the loss from hot surfaces in air. It is obvious, however, that apart from the complication of this mathematical problem, some of the data required for its solution are wanting. In particular, the rate at which heat flows into the metal at each instant of the cycle is not known with any approach to accuracy. It is possible, however, roughly to estimate, from the heat carried away by the jacket-water, what is the *mean* rate of flow, and corresponding with this mean rate there will be a mean distribution of temperature which is such that the temperature-gradient at every point just suffices to convey heat past that point at the correct rate, so that there is no accumulation of heat. Superposed on the mean distribution there will be fluctuations of temperature at every point, both above and below the mean, corresponding with the similar fluctuations in heat-supply which occur during the cycle.

The mean rate of heat-flow depends upon many circumstances, such as gas-supply and degree of compression; and it is not worth while to attempt to draw any conclusions, even as to the mean temperature-distribution, except such as can be based on the order of magnitude of the quantities involved. Taking the engine on which the experiments here described have been made, it has been found that the quantity of heat carried

away by the jacket-water in full-load running may be anything from 1400 to 2000 B.TH.U. per minute according to the gas-supply. Under average working conditions it may be taken as 1600 B.TH.U. per minute. This supply of heat is distributed over the inner surface, but by no means equally over every part of it; the surface of the compression-chamber, being always in contact with hot gases, gets more than its share, and the outer portions of the cylinder-liner get less. The surface exposed to the hot gases at the in-centre is about 2½ square feet; at the other end of the stroke it is about 8 square feet. If all the heat were given to the surface of piston and compression-chamber, the rate of heat-flow over these portions would be 640 B.TH.U. per square foot per minute. This of course is a superior limit. Taking the average exposed surface, namely 5 square feet, the rate of heat-flow into the metal, averaged both as regards time and over the surface, is in round numbers 300 B.TH.U. per square foot per minute.

As an indication of the meaning of these figures it may be noted that a bare steam-pipe at a temperature of 200° C. loses heat by radiation and conduction at the rate of about 10 B.TH.U. per square foot per minute*. A "black" surface, having a temperature of 600° C., radiates at the rate of 160 B.TH.U. per square foot per minute. The outer surface of the engine has to get rid of heat as fast as the inner surface takes it in; if the outer surface is 10 square feet it must lose 160 B.TH.U. per square foot per minute, and must consequently have a temperature of the order of 600° C. These figures are a demonstration, if any be needed, of the necessity of some means (such as the cooling water), of removing the heat from the engine other than natural radiation and convection.

The effect of the water-jacket is to keep at a temperature not exceeding 100° C. those parts of the external surface which are in contact with the water. The heat supplied from within the engine is then removed almost wholly from the water-cooled parts; those portions of the external surface which are not water-cooled losing but little heat in comparison with that taken away by the water. In order to reach the water from the inner surface the heat has to flow by conduction through the metal of the walls, and for this a certain temperature-gradient is necessary.

Consider first the cylinder-liner. The metal separating the inner surface from the water is here 1 inch thick on the average, and it is hardly possible that the average rate of flow of heat through this metal can anywhere exceed 600 B.TH.U. per square foot per minute; in most parts it must be less. To sustain this flow, a temperature gradient of about 60° C. per inch is necessary—according to the experiments of Messrs Callendar and Nicolson on the thermal conductivity of cast-iron†. Thus the inner surface of the cylinder-liner can nowhere be more than 60° C. hotter than the jacket-water.

* G. M. Brill, "Pipe Covering Tests," *Transactions of the American Society of Mechanical Engineers*, vol. 16 (1895), p. 827.

† *Minutes of Proceedings Inst. C. E.*, vol. 131, p. 159.

At some places on the surface of the compression space it is possible, though not likely, that the flow of heat exceeds 600 B.TH.U. per square foot per minute, and at such places the temperature will be a little higher; but it is clear that the mean surface-temperature of those parts of the cylinder which are water-cooled can never rise above quite a moderate value. Probably about 200° C., or say 400° F., with boiling jacket-water, is an upper limit to the mean temperature reached at any point within 2 inches of the water, and at most places the temperature will be much less. Thus the temperature is no higher than in the walls of a high-pressure steam-engine.

It is otherwise with the piston. The heat given to the piston-face has to be conducted radially to the edge, and it has then to cross the film of oil between the piston and liner and so reach the jacket-water. The difference of temperature between the centre and edge of a thin disk of radius a and thickness t, which is receiving heat at rate h per unit area, is $\frac{ha^2}{4kt}$, where k is the thermal conductivity of the material of which it is made*. In the engine which is the subject of this paper, a is 5·75, t is 1·5, and k (according to Messrs Callendar and Nicolson) is 0·064 at 180° C., the units being the inch, the minute, the British thermal unit, and the Centigrade degree. If the heat-flow into the piston-face is taken as 300 B.TH.U. per square foot per minute, h in the same units will be 2·1. Taking these figures it is found that the temperature at the centre of the piston will exceed that at the edge by 180° C. The actual fall will be rather greater because the piston is not a thin disk, and the full thickness is not effective for conducting the heat. Added to this there must be the possibly considerable fall of temperature necessary to conduct the heat from the edge of the piston-face into the cylinder-liner. It is apparent that temperatures of 300° C. and more may easily be reached in the centre of the piston of an engine of this size; and it appears, moreover, that the temperature will increase more or less as the square of the diameter if the thickness remains constant, or directly as the diameter if the thickness is increased in proportion thereto. Similar considerations apply to the valves; the heat given to the valve-face is mainly removed by conduction along the spindle and through the seating, and a sufficient fall of temperature must exist to cause this conduction. The exhaust-valve may be expected to get hotter than the inlet-valve because it receives considerable heat

* See Appendix II for a general discussion of the temperature distribution in a disk heated uniformly over one face. It is there shown that the difference of temperature between the centre and edge of a thick disk lies between $\frac{ha^2}{4kt}$ and $\frac{h(a^2+2t^2)}{4kt}$. When the disk is very thin these expressions become equal, so that a mean between the two, namely $\frac{h(a^2+t^2)}{4kt}$, is probably a very close approximation for the case here considered, and would give about 193° C. instead of 180° C. The difference is not important for the present purpose.

from the rush of gas past it, whereas the inlet-valve is cooled during the suction-stroke.

These estimates of possible temperatures inside a gas engine refer to the clean metal surface. The temperature reached by a deposit of carbon, tar, or other relatively non-conducting material may be much higher. Such a deposit will receive heat nearly as fast as the clean metal, even though its temperature may be three or four hundred degrees higher; and in order that it may get rid of the heat received, a far greater temperature-gradient must exist than is necessary in the case of the metal. Thus, such a surface may reach a high temperature, even though it is on a water-cooled portion of the engine.

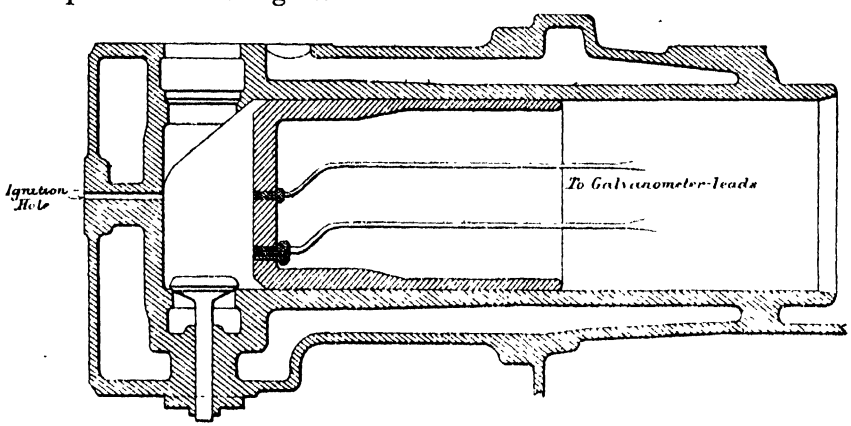


Fig. 1. Section of the Experimental Engine.

Measurements of Temperature and Heat-Loss.

The first object of the experiments to be described was an examination of the actual distribution of temperature in the engine, and of the conditions determining the temperatures reached at various points. The foregoing theoretical discussion, imperfect though it is, indicates pretty clearly that the temperatures of the water-jacketed parts must always be moderate, and cannot have any serious effect on the design or working of the engine unless it be of very large size*. Attention was therefore directed to the uncooled portions, namely, the piston, the exhaust-valve and the inlet-valve.

The Crossley engine is intended to give a maximum of 40 B.H.P. at 180 revolutions per minute. The compression pressure is higher than usual —about 160 lbs. above atmosphere. The diameter of the cylinder is 11½ inches and the stroke is 21 inches, the ignition being by magneto and the governing by hit-and-miss. Fig. 1 gives those details of the cylinder and

* The experiment was tried of sticking patches of tin-foil on the surface of the compression-space. These patches were not melted even when the heaviest charges of gas were used, proving that the temperature of this surface never exceeded 230° C. See also some experiments by Professor Coker, *Engineering*, vol. 86 (1908), p. 497.

piston which are important in the present connection. The engine has been very fully tested for efficiency, etc., and Cambridge coal-gas was used as the fuel throughout.

For the measurement of the temperatures a number of thermo-couples were used, each consisting of a nickel wire brazed into a wrought-iron bolt. The nickel wire passed through a hole drilled along the axis of the

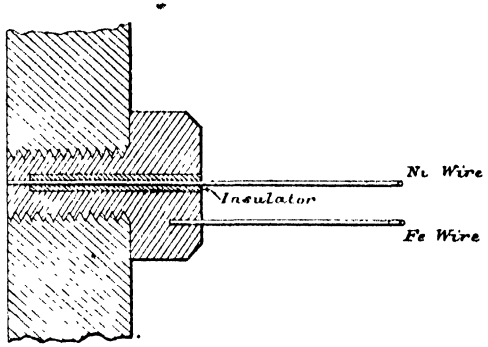


Fig. 2. Section of thermo-couple used in piston.

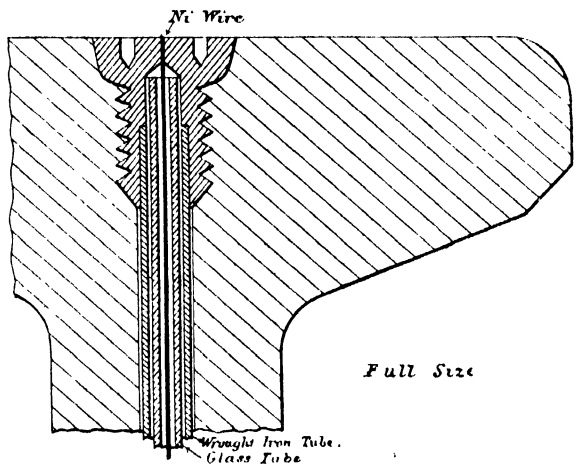


Fig. 3. Section of thermo-couple used in exhaust-valve.

bolt, from which it was insulated by means of a glass tube set in plaster of Paris; it was then brought through a small hole at the end of the bolt, where it was brazed to the iron, the end of the nickel wire being cut off flush with the end of the bolt. A wrought-iron wire was brazed into the head of the bolt. Copper leads, sweated to the nickel and iron wires respectively at a distance of 3 feet or more from the bolt, connected the thermo-couple through an adjustable resistance to a reflecting galvanometer. Details of one of the thermo-couples used in the piston are shown in Fig. 2, and that used in the exhaust-valve is shown in Fig. 3.

Each thermo-junction was calibrated by placing it in an electric furnace along with a Callendar pyrometer. The same leads, resistances and galvanometer were used in the calibration as in the engine, the bolt containing the couple being simply transferred from its place in the engine to the furnace; thus a direct connection was established between the galvanometer-deflection and the temperature. The thermo-couple registers, with sufficient accuracy for the purpose, the excess of its temperature over that of the junctions between the iron and nickel wires and the copper leads. These junctions were sufficiently far from the engine to have the temperature of the surrounding air, which was taken with a thermometer*.

The temperature of the bolt, as shown by the thermo-couple, will not be quite the same as that of the solid metal which it replaces; after the insertion of the bolt there is in fact a slightly different engine. But the difference as regards temperature-distribution is certainly no greater than will often be found between two engines of the same make and size, and in all other respects it is of course inappreciable. Moreover the subject of the investigation is not so much absolute values as the relations between temperature and conditions of running, and these will be just the same whether the piston is solid or not, provided only that the bolt is tight-fitting and screwed up hard. The effect of the screw-thread will be to make the temperature of the bolt exceed that of the solid metal by an amount proportional to the heat-flow into the bolt. The difference is certainly small; in the piston it probably does not exceed 20° C., and in the valves (where the coned head of the bolt was accurately fitted into the hole and then screwed up hard) it must be quite negligible.

The temperature at the metal surface, of course, fluctuates to some extent; and these fluctuations resulted in a slight oscillation of the galvanometer in synchronism with the engine-cycles. The actual amount of the fluctuation was determined by means of a rotating contact-maker, the potential of the thermo-couple being measured at definite epochs in the cycle by means of a potentiometer method. It was found that the variation of temperature in the course of a cycle was in no case more than 5° C. above and below the mean. It is to be observed, however, that the brazed contact between the wire and the iron bolt extends to a depth of about $\frac{3}{16}$ inch, and the temperature measured is really the mean over this length. Since the cyclical variations of temperature in the metal diminish very rapidly with the depth, the fluctuations registered by the couple will be much less than the surface-variation. The mean temperature, however, is the same within 3° or 4° C. at the surface and at a point $\frac{1}{8}$ inch below it, and will be correctly registered by the thermo-couples.

* For details of calibrations see Appendix I. The relation between the electro-motive force of the thermo-couple and the temperature-difference varies slightly with the temperature of the cold-junction. This variation is hardly sufficient to affect the measurements for the present purpose, but it has been allowed for.

An important point in the investigation was a comparison between the temperatures of the various parts and the total heat received by the engine and removed by the jacket-water and by conduction and radiation. The excess of the temperature of any portion of the engine over the mean temperature of the jacket-water, multiplied by the average thermal-conductivity of the metal along the line of flow of the heat from that point to the jacket-water, should be proportional to the total heat carried away, except in so far as disturbing causes affect the manner in which the heat-flow is shared by different parts. A study of the ratio between the heat-loss and temperature of any part under various conditions of running, therefore, throws light on the manner in which the distribution of heat-loss is affected by such conditions. For the purpose of such a comparison an accurate measure of the total heat-loss is necessary, this total being equal to the heat carried away by the jacket-water plus the heat lost by radiation and conduction. The jacket-water heat was measured in the ordinary way, though it was found that very considerable care was requisite in order to obtain accurate measurements. A separate experiment, of which it is unnecessary to give details, proved that the capacity for heat of the engine was for this purpose equivalent to over 1000 lbs. of water. That is to say, an increase of 1° C. in the mean temperature of the jacket-water during the course of the tests, or of 2° C. in the temperature at exit, was associated with an absorption of heat into the engine of about 2000 B.TH.U., which is considerably more than the whole quantity of heat discharged in the course of a minute's run, under full-load conditions. It was necessary, therefore, to run the engine for a considerable time with the jacket-water temperature perfectly steady, in order to be sure that the heat absorbed by the metal of the engine and by the weight of water in the jacket should be a sufficiently small proportion of the whole. In most of the tests to which reference is made in this Paper, the whole of the water passed through the jackets in the course of 1 hour was weighed and the mean temperature was taken; and the arrangements were such that the temperature at exit did not at any time differ from the mean by more than 1° C., thus ensuring that the heat absorbed or given out by the engine was not more than 1 per cent. of the whole amount removed by the jacket-water.

The heat removed by conduction and radiation was estimated with sufficient accuracy for the purpose by comparing trials in which the conditions were in all respects the same, except that the temperature of the jacket-water was varied by changing the rate of flow. Several of these comparisons were made; the two trials in each case closely succeeding one another on the same day. It was found that when the mean temperature of the jacket exceeded that of the air by 28° C. the heat removed by the water was about 100 B.TH.U. less than when the difference was 9° C. Assuming that the rate of loss is proportional to the temperature-difference, it follows that the total heat lost by radiation and conduction in the former

case is about 150 B.TH.U., or roughly 10 per cent. of the total heat passing into the walls of the engine. In most of the tests, except those made for the purpose of determining conduction and radiation, the temperature of the jacket-water at exit was 70° C., and the mean jacket-temperature was about 42° C., which would be approximately 28° C. in excess of the air-temperature. The loss by radiation under these conditions, therefore, would be practically the same for all the tests, and equal to about 150 B.TH.U. per minute. As the results are mostly of a comparative kind and do not depend upon absolute values, a considerable error in this determination will not seriously affect any of the conclusions.

The heat removed by the jacket-water and by conduction and radiation in ordinary full-load running varied from 29 per cent. of the whole heat-supply (lower calorific value), with a charge containing 8·5 per cent. of coal gas, up to 34 per cent. with 11 per cent. of coal gas. Between these limits, which represent the extreme mixtures used in the experiments, the percentage could be sufficiently nearly expressed as a linear function of the mixture-strength. These percentages are averages based on about twenty trials; occasionally in individual trials the percentage found differed by as much as 2 from that calculated on the basis of the average figures, though in nine cases out of ten the difference was not more than 1½. Considering the large number of measurements which enter into the determination of this percentage, these casual errors in it are not surprising.

It is unnecessary to give full details of the other appliances used for testing the engine, as they have been very fully described in other Papers by the author*. The gas was measured by observing the fall of a small standard gas-holder of about 10 cubic feet capacity with the main gas-supply shut off. The actual quantity of gas taken in about fifty explosions could be estimated in this way with certainty to 1 part in 250. The calorific value was measured with a Boys or a Junker calorimeter. It was found difficult to get accurate measurements of this quantity, mainly on account of errors in the wet-meter used with the instrument, and in consequence of this it was usually arranged that all tests which depended in any way on the total heat supplied to the engine should be done in pairs on the same day, so that accurate comparison should be possible—independent of measurements of calorific value. The engine has been very fully tested for efficiency, and it is certain that, given an accurate measurement of the heat-supply, either the brake work or the indicated work can be predicted correctly to within 2 per cent. An exhaust-gas calorimeter of the spray type was fitted, and the total heat delivered to the engine was occasionally measured, as a check upon the calorific value measurements. Details of a large number of these heat-balances are given in a previous Paper by the author†, and to these may be added a set of three obtained in June,

* *Proceedings Inst. Mech. E.*, 1907, p. 863. Page 226 of this volume.

† *Proceedings Inst. Mech. E.*, 1908, p. 417. Page 263 of this volume.

1908—at the end of the series of trials described in the present Paper. In two of these tests, the heat unaccounted for, after allowing for radiation (estimated as just described), was within 1 per cent. of the total heat supplied to the engine, as determined by calorimeter and gas-holder; and in the third the quantity of heat unaccounted for was 2 per cent. of the whole. The total heat given to the engine can certainly be determined by means of the exhaust-gas calorimeter with an accuracy which is equal, if not superior, to that of the measurements of the calorific value of the gas.

Piston-Temperatures under Normal Conditions.

Systematic observations of the piston-temperatures were commenced in October, 1907, and were continued to June, 1908. The thermo-couple

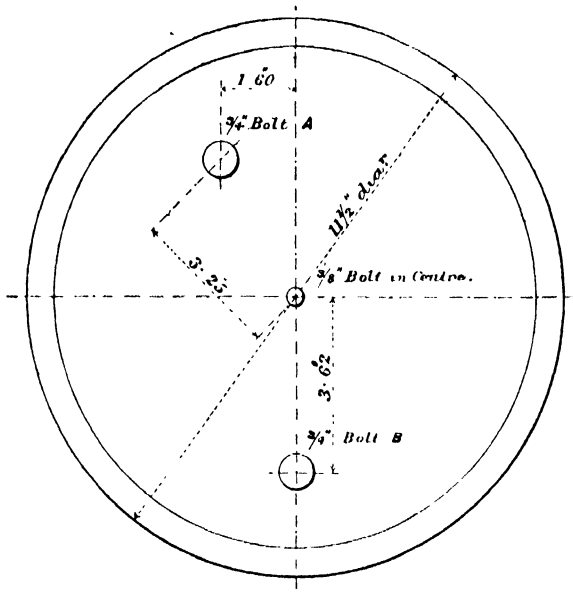


Fig. 4.

used for this purpose, having previously been calibrated, was first fixed in the position marked A in Fig. 4, distant $3\frac{1}{4}$ inches from the centre. In the series of observations set out in Table I the engine was firing nearly every time and the conditions of running were in every respect normal, except that the gas-charge was varied over a rather wider range than is usual in practice, so that a large variation in the heat given to the engine could be produced. The quantity shown in the third column, H, is the total heat removed from the engine by the jacket and radiation, determined as already described; θ_A, shown in the fourth column, is the excess of the piston temperature at A over the mean jacket-temperature, which in all these trials was about 42° C. In the fifth column the ratio θ_A/H is shown.

Table I.

Test No.	Date	H	θ_A	$\frac{\theta_A}{H}$	Test No	Date	H	θ_A	$\frac{\theta_A}{H}$
	1907					1907			
23	Oct. 17	1945	384	0·197	34	Nov. 13	1628	312	0·192
24	,, 18	2033	388	0·190	35	,, 13	1848	349	0·189
25	,, 19	1695	328	0·193	36	,, 20	1931	358	0·185
26	,, 19	1335	255	0·191	37	,, 20	1870	355	0·190
28	,, 22	1944	379	0·195	39	,, 22	1823	347	0·190
29	,, 22	1365	256	0·193	41	,, 27	1610	312	0·194
33	Nov. 13	1366	249	0·186					

The trials were resumed in February, 1908, the thermo-couple having been in the meantime removed, recalibrated, and replaced in the position A. Another thermo-couple, similar in construction to the first but fixed in a bolt of smaller size, was also inserted at the centre of the piston-face, so that simultaneous readings of the temperature could be taken at the centre and at the point A. The engine was working almost daily in the interval with the exception of a short period at Christmas.

The temperature at the centre is denoted by θ_C in the following table:

Table II.

Test No.	Date	H	θ_C	θ_A	$\frac{\theta_C}{H}$	$\frac{\theta_A}{H}$
	1908					
49	February 27	1333	270	224	0·202	0·168
50	,, 27	1968	390	323	0·198	0·164
51	,, 28	1426	294	241	0·206	0·169
52	,, 28	1447	300	254	0·207	0·175
55	March 6	1322	276	227	0·209	0·172
56	,, 6	1980	419	350	0·212	0·177

At this point the piston was drawn and magnesia lagging was packed in at the back of it to a depth of about 2 inches, with the object of ascertaining whether its temperature was materially affected by thus checking the radiation of heat. The trials were resumed within a day or two with the following results:

Table III.

Test No.	Date	H	θ_C	θ_A	$\frac{\theta_C}{H}$	$\frac{\theta_A}{H}$
	1908					
59	March 11	1397	295	246	0·211	0·176
60	,, 11	1945	423	346	0·218	0·178

The lagging was now removed and both bolts were taken out, the centre bolt was replaced in its old position, but the other was shifted to the position marked *B* in Fig. 4, which is 3·62 inches from the centre and vertically below it. The subsequent observations are shown in the following table:

Table IV.

Test No	Date	H	θ_C	θ_B	$\theta_C - \theta_B$	θ_A (calculated)	$\frac{\theta_C}{H}$	$\frac{\theta_A}{H}$
	1908							
61	March 16	1327	285	224	61	235	0·215	0·177
62	,, 16	1990	425	331	94	345	0·214	0·173
63	,, 16	1392	290	232	58	239	0·209	0·171
64	,, 17	2103	437	340	97	355	0·208	0·169
70	,, 26	2030	428	339	89	355	0·211	0·175
75	April 28	2110	440	344	96	361	0·208	0·171
79	June 5	1912	383	289	89	299	0·200	0·160
81	,, 12	1750	356	264	92	276	0·203	0·158
83	,, 15	1692	352	261	91	272	0·208	0·161
85	,, 16	1810	...	272	...	284	...	0·157

At the completion of the series of tests both the thermo-couples were removed and again calibrated.

A study of these tables at once shows that in tests made at about the same time (*e.g.* within the same month) the ratio θ/H is the same, certainly within 5 per cent., and probably within 2 per cent. whether the heat-loss H be 1300 or 2000 B.TH.U. per minute. This proves that within these limits the proportion of heat received by the piston is substantially unaffected by the strength of the mixture*. There are, however, considerable differences in the ratio as given by tests widely separated in point of time. The mean value of the ratio θ_1/H in the observations given in Table I is 0·193, whereas in the tests of February and March it is 0·171, corresponding with a reduction of temperature in the bolt of 35° C. when $H = 1600$. This is greater than can be accounted for by errors in temperature-measurement, and is probably due, in part at any rate, to the fact that the bolt was removed and replaced between the two sets of observations, and that the relation between its temperature and that of the surrounding metal was altered. A similar reduction, however, occurred in the temperature at the

* The heat received by the piston is, strictly speaking, proportional to the product $k\theta$, where k is the average conductivity of the metal: k decreases as the temperature rises; hence, if θ/H is constant, the proportion of heat received by the piston must be rather greater at the lower temperatures. The experiments of Messrs Callendar and Nicolson showed that k is about 10 per cent. less at 180° C. than at 40° C. Extrapolation from these results leads to the conclusion that k must be taken as between 5 and 10 per cent. greater when H is 1300 B.TH.U. per minute than when it is 2000 B.TH.U. per minute. There is, therefore, some evidence that the piston receives a rather greater share of the total heat-loss when the mixture is weak; but the difference is hardly more than can be accounted for by errors of observation, and is of no practical importance.

point B between the months of March and June (see Table IV). This change can hardly have been due to any alteration in the seating of the bolt, which was screwed in very tight at the beginning of March, and was not taken out until July. It would appear probable that in course of time the temperature did for some reason change slightly outside the bolt and independently of it.

On the assumption that the heat-flow over the piston-face is uniform and that the heat is removed equally all round the edge, the distribution of temperature over the piston-face should be such that the lines of equal temperature are circles, the fall of temperature from the centre to any point being proportional to the square of the distance from the centre. This was found to be nearly the case. In the trials enumerated in Table II —all of which were taken at the end of February or in the first half of March—the temperature at the point A was, on the average, $0{\cdot}171\ H$ and at the centre $0{\cdot}206\ H$. The mean difference of temperature between the centre and A was $0{\cdot}035\ H$, corresponding to about 56° C. with a medium heat-loss of 1600 B.TH.U. per minute. Between March 11 and 16 the thermo-couple was moved from the point A to the point B, and in the subsequent trials, made in the same month (shown in Table IV), the mean temperature at B was found to be $0{\cdot}166\ H$ and at the centre $0{\cdot}211\ H$, the difference being $0{\cdot}045\ H$, or 72° C., with a heat-loss of 1600 B.TH.U. per minute. The ratio of the fall of temperature from the centre to B to the fall from the centre to A is therefore 1·30. According to the theory the ratio should be $\left\{\frac{3{\cdot}62}{3{\cdot}25}\right\}^2$, or 1·23. The agreement is as close as can be expected having regard to the general accuracy of the measurements, and furnishes satisfactory evidence that the thermo-couples are really giving a close approximation to the temperature of the piston in their immediate neighbourhood, and further that the radial heat-flow in the piston is substantially the same in all directions.

Assuming that the temperature continues to fall away as the square of the distance, the difference between the edge and the centre of the piston will be $\left\{\frac{5{\cdot}75}{3{\cdot}62}\right\}^2 \times 0{\cdot}045\,H = 0{\cdot}114\,H$, giving a value of 182° C. when the total heat-loss is 1600 B.TH.U. per minute. Roughly speaking, this is half of the whole difference of temperature between the centre of the piston and the jacket-water, the other half being the fall necessary to conduct the heat from the edge of the piston into the liner and across the liner into the water. The actual temperature at the centre of the piston, with a jacket-water temperature at exit of 70° C. and with the medium total loss of 1600 B.TH.U. per minute, is about 370° C. The highest recorded temperature at the centre of the piston was 480° C. in the tests given, but it was probably rather higher on some occasions in the earlier trials when the temperature at the point A alone was measured.

Expansion and Stresses in the Piston.

The metal temperatures in the engine are of practical importance in two ways. There is first the possibility that the hot metal may cause pre-ignition of the charge, but from experiments described in a later portion of this Paper the author is of opinion that pre-ignitions are not likely to originate in the piston of an engine of this size, or indeed of much larger size, except when deposits are formed of carbon or other badly-conducting material. A more important aspect of the piston-temperature is that of mechanical design, which must be such as to provide for expansion, and for the internal stresses set up by unequal heating.

The stresses in a disk whose temperature falls from the centre to the circumference in the same way as in the piston-face (that is, as the square of the distance) are identical with the stresses produced in the same disk by rapid rotation; the angular velocity would be such that its square was proportional to the temperature-difference between centre and circumference, and it would be combined with an inward radial pressure, such as might be produced by winding the edge of the disk with wire under tension. Calculation shows that the contraction in diameter of the disk (assuming the temperature of the centre to remain constant) will be half that corresponding with the peripheral fall of temperature*. The tensile stress at the edge will, of course, be that corresponding with the difference between the contraction which actually occurs, and that which would occur if the contraction were unresisted by the compression of the inner portions. The piston-face is $11\frac{1}{2}$ inches in diameter, the temperature of the centre under normal working conditions may be taken as 340° C., and that at the periphery as 170° C. above the mean jacket-temperature. The linear expansion of iron may be taken as 10^{-5} per degree Centigrade. If the whole piston-face were heated through 340° C. it would expand by 0·0034 of its diameter. Superposed on this expansion, however, is the contraction caused by the cooling of the edge to 170° C. below the centre. This contraction amounts to 0·00085 of the diameter, leaving a net expansion of 0·0026, say 30 thousandths of an inch. At the same time a tensile stress corresponding with an extension of 0·00085 is set up. If Young's modulus for cast iron is taken as 15,000,000 lbs. per square inch, the stress will amount to about $5\frac{1}{2}$ tons per square inch. These figures apply to working with normal charges; with the heaviest charges the temperature rises at the centre to 440° C. and at the edge to 220° C. above the mean jacket-temperature. The resulting net expansion in this case is 38 thousandths of an inch and the tension is $7\frac{1}{2}$ tons per square inch.

The actual clearance between piston and liner in this engine when cold is about 45 thousandths of an inch, which is sufficient for all ordinary conditions of working as the liner will also expand to a small extent. Under pre-ignition conditions the piston gets considerably hotter, and

* See Appendix III.

continuous early ignition combined with excessive gas-charges might bring it dangerously near to seizing. A stress of 7 tons per square inch is rather high for cast-iron, but it is to be remembered that the calculation of stress applies to a plain disk of uniform small thickness. The piston-face is not plane over the whole area, being bevelled off at the edge, and it is strongly reinforced at the edge by the back part of the piston, which is here considerably thickened. Both these features tend to reduce the temperature stresses below the calculated values; the bevelling keeps up the temperature of the edge, and the thickening distributes the stress over a larger area. But if the piston were much increased in size the stress at the edge and the expansion would become serious problems. If for example the piston were of double the diameter and its thickness were also doubled (the compression and working mixture remaining the same) the temperature at the centre under normal conditions would probably exceed 600° C., the expansion would be about 0·0045 of the diameter (or more than $\frac{1}{10}$ inch) and the tension in the edge would be of the order of 10 tons. Such a piston might run all right so far as pre-ignition is concerned, but it could hardly be considered satisfactory mechanically. It would be likely to fail by a radial crack at the edge, and the resulting sudden expansion would probably cause it to seize and tear out the liner. The stress might be reduced by using a domed piston, the bending of which at the centre would relieve the tension at the edge. Practically it would be necessary to water-cool such a piston.

From the temperature-gradient in the piston a rough estimate can be formed of the rate at which it is receiving heat per unit area. The relation between the heat-flow h into a disk and the difference of temperature θ between its centre and circumference has already been given (p. 294); it is $\theta = \frac{ha^2}{4kt}$. For the disk bounded by the circle through the point B in the piston-face (Fig. 4), $a = 3{\cdot}62$ inches, $t = 1{\cdot}5$ inches, and k (at a temperature of 300° C.) = 0·056 (by extrapolation from Messrs Callendar and Nicolson's results). Under normal working-conditions, when the total flow into the engine is 1600 B.TH.U. per minute, θ will be 72° C. It follows that the rate at which heat is flowing into this portion of the piston-face is 1·85 B.TH.U. per square inch per minute. This is the difference between the heat given by the gas to the metal and that lost by the metal by convection and radiation from the back of the piston. Rough calculation showed that the latter loss should not amount to much, but in order to settle the point the experiment was tried of lagging the piston with magnesia and comparing the temperatures under identical conditions of running with and without lagging.

The results of this test have been shown in Table III. Comparing the temperatures in that table with those immediately preceding (Table II) and those following (Table IV), it will be seen that the temperature at the

centre of the piston is not more than about 10° C. higher with the lagging than without it, which shows that not more than 2 or 3 per cent. of the whole heat received by the piston is lost from the back.

The area of the piston-face is 104 square inches; the total heat received by it is therefore 190 B.TH.U. per minute, or about 12 per cent. of the whole heat-flow into the walls. It is of interest to compare this result with direct determinations of the heat given to a water-jacketed piston. Those made by Professor Burstall, the results of which are given in the Third Report of the Gas Engine Research Committee*, may be taken as an example. In trials *D* and *F* of the series described in the report, the compression-ratio was nearly the same as in the engine used by the author—being 6·79 in the one case and 6·37 in the other. The area of the piston at the out-centre was one-ninth of the whole exposed area, and at the in-centre about three-tenths—ratios which are very nearly the same as in the engine experimented with by the author. Taking the average of the eight trials classed as *D* and *F*, the author has found that the piston of Professor Burstall's engine took in 16 per cent. of the heat carried away by the cooling-water. If to the heat removed by the water were added that taken away by radiation and conduction, this proportion would be rather reduced—probably to 14½ or 15 per cent. This fraction is rather greater than the author has found—a result which may be accounted for mainly by the fact that the water-jacketed piston is some 200° C. the cooler. If the temperature of the gas in contact with the piston is taken as 1200° C. during the period of rapid heat-flow, this would show that Professor Burstall's piston took in heat about 20 per cent. faster than the author's.

The proportion of heat received by the piston is considerably smaller than at first sight would be expected. At the in-centre, when the density and temperature of the gas is highest, and when the heat-flow is therefore most rapid, the piston-area amounts to 30 per cent. of the whole exposed surface. It remains in contact with the hot gas during the whole stroke, and even on the out-centre it constitutes 11 per cent. of the whole surface. The piston would be expected to receive at least one-fifth, and probably a quarter of the total heat lost in the engine; that it only receives about one-eighth is only partly due to its own high temperature, for the excess of the temperature of the piston over that of the other parts of the engine, though considerable, is still a small fraction of the difference between the mean temperature of the gas and that of the metal. The fact that in Professor Burstall's trials, in which the piston was water-cooled, the amount of heat received by it was only 15 per cent. of the total, also shows that the temperature of the piston itself is not sufficient to account for the small heat-flow into it. A number of other causes contribute to this result. A considerable proportion of the total heat-loss *H*, probably amounting to about 300 B.TH.U. per minute, occurs during the rush of the hot gases past

* *Proceedings Inst. Mech. E.*, 1908, p. 5.

the exhaust-valve at the moment of release, and practically the whole of this goes to the valve and passages, and none to the piston; thus the latter gets less than its full share of the whole. This, however, is not sufficient to account for the discrepancy; and it is probable that, from various causes, the gases in contact with the piston after combustion is complete are cooler than anywhere else in the cylinder. In the first place, the mixture there must be rather weaker than the average, because the piston no doubt takes with it on the suction-stroke a large part of the gases left in the clearance-space, which will dilute the mixture near the piston more than elsewhere in the cylinder. In the second place, the gas near the piston is the last to be ignited, the flame starting from the ignition-hole at the opposite side of the compression-space; in consequence, as explained in greater detail later on in this paper, the gas near the piston after ignition will be much cooler than that near the point of ignition. It would be interesting to try the effect upon the piston-temperatures of firing the charge from a point near the piston. The author was unable, in the time at his disposal, to try this experiment, but he believes that the piston would get much hotter —probably by more than 100° C. at the centre.

The Temperature of the Valves.

Observations of the temperature at the centre of the exhaust-valve and inlet-valve were taken in March, and again in June, 1908. The engine was run firing every time, the strength of mixture only being varied, as in the observations of piston-temperature which have been described in the last section; indeed, the valve-temperatures were generally taken at the same time as those of the piston. It is unnecessary to do more than summarize the results of these observations of valve-temperatures. As before θ represents the excess of the temperature at the centre of the valve over the mean jacket-temperature (which was about 40° C. in nearly all cases), and H is the total heat-loss in B.TH.U. per minute (jacket-water plus radiation):

Exhaust-Valve.

(*a*) Seven trials with heavy charges, giving H between 1900 and 2100. Mean value of $\frac{\theta}{H}$ was 0·217, maximum 0·226 and minimum 0·208.

(*b*) Seven trials with light charges, giving H between 1350 and 1450. Mean value of $\frac{\theta}{H}$ was 0·240, maximum 0·250 and minimum 0·233.

Inlet-Valves.

(*a*) Six trials with heavy charges. Mean value of $\frac{\theta}{H}$ was 0·127, maximum 0·133 and minimum 0·123.

(*b*) Seven trials with light charges. Mean value of $\frac{\theta}{H}$ was 0·134, maximum 0·137 and minimum 0·132.

Under ordinary running conditions, with a medium gas-charge and with a loss H of 1600 B.TH.U. per minute, the temperature at the centre of the exhaust-valve was about 400° C., and at the centre of the inlet-valve about 250° C.

The most noticeable feature of these trials is the decided decrease, amounting to about 10 per cent. in the ratio θ/H for the exhaust-valve when the strength of mixture is increased; it shows that the proportion of the total heat-loss received by the exhaust-valve is considerably less when the charges are heavy than when they are light. The difference must amount to at least 10 per cent., and, having regard to the fact that the conductivity of the metal is less at the higher temperatures, it is probably nearer 15 per cent. The explanation is probably to be found in the fact that the heat-loss which occurs during the out-flow of the exhaust-gas just after release goes mainly to the valve, and that this part of the heat received by the valve is not much less in absolute amount with a weak than with a strong mixture. The quantity of gas and its state of motion are the same in both cases, and the temperature, though less with a weak mixture, does not diminish in proportion to the heat-supply, because there is a smaller percentage of heat lost during explosion and expansion with the weaker mixtures, leaving more to be discharged to exhaust. Thus, the heat received by the exhaust-valve in this way forms a greater proportion of the whole loss when the charge of gas is small*. This view is confirmed by the fact that, when the total heat-loss H is reduced by diminishing the load on the engine so that it misses ignitions, the charge of gas remaining the same, the value of θ/H for the exhaust-valve is unaltered—showing that under these circumstances the proportion of heat received by the exhaust-valve is practically unchanged.

The temperature of the exhaust-valve is mainly important in connection with pre-ignition. In this engine under ordinary conditions it does not exceed about 400° C., which is too low to cause pre-ignition. But with very heavy charges it may reach nearly 500° C., and a drop of oil falling near its centre will then cause pre-ignition. Of course it is quite possible for this to happen, and it is probable that the occasional pre-ignitions which occur in this engine, when working in the ordinary way but with abnormally heavy charges, are traceable to this cause.

The temperature of the inlet-valve is only of practical importance in so far as it affects that of the incoming charge. That this effect is not very great is shown by the fact that the quantity of gas and air drawn in is

* See the author's paper on "The Effect of Mixture Strength upon Thermal Efficiency" (*Proceedings Inst. Mech. E.*, 1908, p. 417). Page 263 of this volume. From the results there given it appears that the ratio

$$\frac{\text{heat present in gases at release}}{\text{total heat removed by jacket and conduction}}$$

was about one-sixth part greater when the gas-charge was 0·10 cubic foot per suction than when it was 0·12 cubic foot.

only altered to a small extent by the load on the engine and by the strength of the charge*.

THE CONDITIONS DETERMINING THE TEMPERATURE OF THE METAL.

The temperature of the metal at any point is chiefly determined by two factors, namely, the rate at which heat passes into the surface per unit area in the neighbourhood of that point, and the distance that the heat has to travel to the place where it is removed. Of these factors the first is to a great extent independent of the size or mechanical design of the particular engine, but is determined by such things as degree of compression, strength of mixture, and time of ignition. The second factor is a question of size and of the arrangement of the metal in relation to the cooling water. The experiments described in the last section give the temperature-distribution in one particular engine under normal conditions of working. In a geometrically similar engine of different size working under the same conditions, the distribution of temperature will be similar, but the temperatures at corresponding points will be approximately in proportion to the linear dimensions. It is easy to see what will be the general effect of any given alteration in the arrangement of metal. For example, if the thickness of the piston-face were halved, the fall of temperature from centre to edge would be approximately doubled, and pre-ignition would probably occur with heavy charges.

The experiments to be now described deal with the effect upon the heat-flow—and thereby upon the metal temperatures—of a change in the conditions of working. Such a change may affect the heat-flow at any point in two ways; it may alter the total heat passing into the engine and removed by the water-jacket, and it may change the manner in which that heat is distributed over the metal surface. The most important factors in heat-flow are the following:

(1) Strength of mixture;
(2) Time of ignition;
(3) Degree of compression;

which will be considered in this order.

(1) *Strength of mixture.* The observations of temperature described in the last section were made with varying mixture-strengths, and sufficiently indicate the influence of this factor. The following table is based on these observations, and shows the temperatures reached at various points with a very weak and a very strong charge respectively. The engine was running at 180 revolutions per minute and firing every time; and the jacket-water had a temperature of about 75° C. at exit. The lower calorific

* The air was measured by an anemometer. See the author's paper "On the Indicated Power and Mechanical Efficiency of the Gas Engine," *Proceedings Inst. Mech. E.*, 1907, p. 863. Page 226 of this volume,.

value at the external temperature and pressure is taken as 580 B.TH.U. per cubic foot:

Gas-charge (volume per suction at external temperature and pressure)	0·1	0·13 cubic foot
Percentage of gas in cylinder contents ...	8·5	11·0 per cent.
Total heat-loss per minute	1510	2300 B.TH.U.
Total heat-loss as percentage of total heat-supply	29	34 per cent.
Temperature of piston (point *A*)	300° C.	430° C.
„ exhaust-valve	400° C.	540° C.
„ inlet-valve	240° C.	355° C.

The heat-loss, and with it the temperatures, increase in a much greater proportion than the gas-charge is increased. This is probably due in part to the more vigorous explosion, which results in a more violent motion of the gas, and in part to more rapid combustion. With the weaker mixture, the volume increases to a sensible extent during the combustion, thus lowering the temperature reached by the gas.

As already indicated, strength of mixture does not much affect the distribution of heat-loss so far as the piston is concerned. But the share of heat received by the exhaust-valve is greater with the weaker mixtures, and the strength of mixture thus has less effect upon the temperature of the exhaust-valve than upon that of other parts of the engine. It is probable that this will be more marked and that the exhaust-valve will be relatively hotter in engines using low compression, since the heat received by the valve after release is then greater because of the higher temperature of the exhaust-gases.

(2) *Time of ignition.* Normally the ignition in this engine commences about 5° before the in-centre, and is completed from 10° to 15° of crank-angle after the centre according to strength of mixture. The tripping of the magneto takes place at − 23°. It was found that retarding the period of ignition reduced the total heat-loss in a marked degree and also altered its distribution, so that the piston and inlet-valve received a less share of heat and the exhaust-valve received more. Comparative measurements of temperature and heat-loss were made on the same day, first with normal ignition and then with the ignition retarded by 20° of crank-angle—the gas-charge being precisely the same in the two cases. The mean jacket-temperature was as usual about 40° C. The following are the results of one such pair of trials:

	Trial No. 84 (Retarded ignition)	Trial No. 85 (Normal ignition)
Gas-charge, per suction	0·1224	0·1218 cubic foot
Total heat-loss, *H* per minute ...	1619	1805 B.TH.U.
Total heat-loss as percentage of total heat supply on lower calorific value	28·1	31·3 per cent.
Piston-temperature (point *B*) ...	258° C.	317° C.
$\frac{\theta}{H}$ for piston	0·132	0·151
Inlet-valve temperature	207° C.	255° C.
$\frac{\theta}{H}$ for inlet-valve	0·10	0·118

The exhaust-valve temperature was not taken in this pair of trials; but other tests showed that retarding the ignition was practically without effect upon the temperature of the exhaust-valve, the value of θ/H being increased in proportion as H was diminished. Thus it appears that when

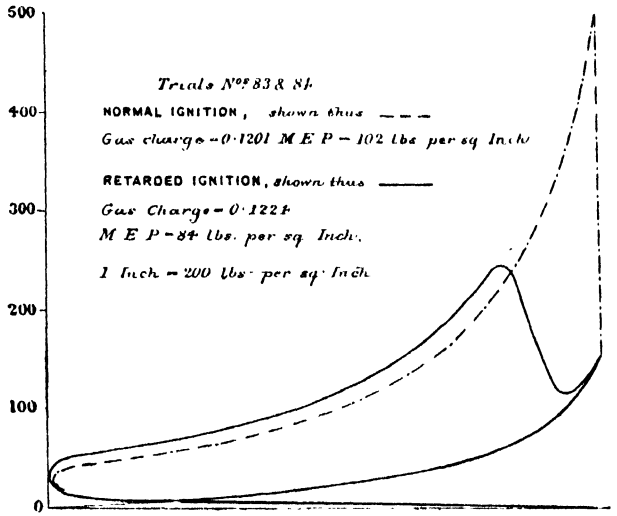


Fig. 5.

the ignition is retarded the exhaust-valve gets a larger share of the heat, and the piston and inlet-valve a smaller share than with normal ignition. Both effects are to be expected, the one because the exhaust-gases are

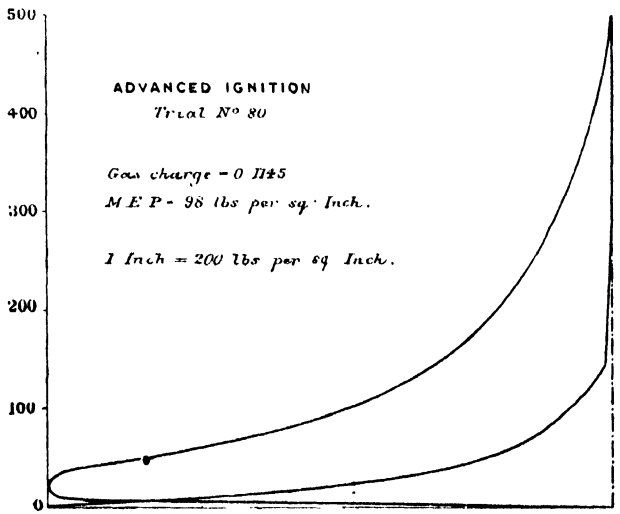


Fig. 6.

hotter and therefore the exhaust-valve gets more heat after release; the other because a larger surface is exposed to the flame and the piston constitutes a smaller proportion of that surface.

Tracings of the diagrams taken with normal and retarded ignition and nearly the same gas-charge are shown in Fig. 5. The efficiencies are 34½ per cent. and 28½ per cent. respectively. The temperature at the moment of release is 1000° C. with normal ignition, against 1200° C. with retarded ignition. The effect of retarding the ignition is, of course, very similar to that of reducing the compression by shortening the stroke, and keeping the volume at out-centre the same. The total heat-loss is reduced and the engine keeps cooler except in the exhaust-valve. This advantage is secured, however, at the expense of a substantial loss of efficiency, and the exhaust-valve temperature is not reduced.

Advancing the ignition by 10° of crank-angle caused a marked increase in heat-loss and temperature, as shown in the following pair of trials taken on the same day:

	Trial No. 80	Trial No. 81
Ignition point (time of tripping magneto; actual ignition commences 18° later)	− 33°	− 23° (normal)
Gas-charge	0·1145	0·1151 cubic foot
Total heat-loss, H per minute ...	1920	1750 B.TH.U.
Total heat-loss, as percentage of total heat-supply on lower calorific value	34·8	31·5 per cent.
Piston-temperature (point B) ...	358° C.	308° C.
„ „ „ $\frac{\theta}{H}$ ...	0·165	0·151
Piston-temperature (centre)	451° C.	400° C.
„ „ „ $\frac{\theta}{H}$ ...	0·212	0·204
Exhaust-valve temperature	479° C.	460° C.
„ „ „ $\frac{\theta}{H}$ ...	0·227	0·239
Inlet-valve temperature	292° C.	221° C.
„ „ „ $\frac{\theta}{H}$ ...	0·130	0·103

It will be seen that when the ignition is advanced the piston and inlet-valve get a distinctly larger share of the heat, and the exhaust-valve a smaller share, but the difference is not very large. The diagram for Trial 80 is shown in Fig. 6; it is hardly distinguishable from the normal diagram with the same gas-charge, the difference of area being less than 2 per cent. The increase in metal-temperatures caused by the more advanced ignition, however, might be quite sufficient to stop the running of the engine if it were near the point of pre-ignition, or if the allowance for piston-expansion was cut rather fine.

(3) *Degree of compression.* The effect of changing the compression upon metal-temperatures is of special interest, because upon it depends in a large measure the practical limit to increase of compression. It was studied in the present instance by keeping the half-compression cam, which is provided for starting purposes, in action when the engine was running. This cam holds the exhaust-valve open during the first part of the compression stroke, so that a part of the charge is expelled unburnt. The valve

closes when the volume enclosed by the piston is 41 per cent. of the total cylinder-volume at out-centre; the pressure is then about 10 lbs. per square inch above atmosphere. The proportion of the whole charge which is retained can be calculated from these figures on the assumption that up to the closing of the valve the gas in the cylinder has been compressed without loss of heat; worked out in this way it comes to 59 per cent. The proportion retained can also be measured directly by determining the total heat supplied to the engine with the aid of the exhaust-gas calorimeter. Three measurements of this kind gave respectively 56, 57 and 58 per cent., which are a little less than the results of calculation; and giving some weight to the calculations, the proportion may be taken as 57·5 per cent., in which the probable error is not more than about 1.

Comparative trials were made in pairs on the same day; one with the engine working normally, the other with the half-compression cam in action. The gas taken into the engine per suction was as nearly as possible the same. The mixture, therefore, is the same in the two trials; the temperatures on the expansion stroke are also very nearly the same, and the extent of surface exposed to the action of the hot gases is identical. The only thing which is varied is the density of the charge. The results of two pairs of trials are given in the Table below.

Date	5th June, 1908		15th June, 1908	
Trial No.	78	79	82	83
Compression	Reduced	Normal	Reduced	Normal
Gas-charge cu. ft.	0·1199	0·1207	0·1187	0·1201
Heat supplied to engine per minute*. B.TH.U.	3100	5815	3140	5420
Total heat-loss H	1100	1900	1100	1710
Total heat-loss H, as percentage of total heat-supply. per cent.	35·5	32·6	35·0	31·6
Piston-temperature (centre) ...	271° C.	425° C.	278° C.	393° C.
,, ,, ,, $\frac{\theta}{H}$ ...	0·211	0·200	0·218	0·206
Piston-temperature (point B) ...	213° C.	335° C.	217° C.	302° C.
,, ,, ,, $\frac{\theta}{H}$ ...	0·160	0·153	0·162	0·152
Exhaust-valve temperature ...	267° C.	458° C	...	...
,, ,, ,, $\frac{\theta}{H}$ ...	0·201	0·220	...	...
Inlet-valve temperature ...	167° C.	276° C.	167° C.	244° C.
,, ,, ,, $\frac{\theta}{H}$ ...	0·112	0·123	0·112	0·118

(Engine missing about 1 explosion in 10, in every case.)

* Calculated for the reduced-compression trials on the assumption that 57½ per cent. of the charge is retained.

It will be seen that the total heat-loss H is very much less with the reduced compression, but that it is not diminished quite in proportion to the density of the charge. It must be observed, however, that the piston-friction, amounting to about 100 B.TH.U. per minute in each case (2·5 H.P.), is included in the total heat-loss, and should be deducted in order to obtain the heat actually given up by the gas. The percentage losses then become (mean of the two pairs of trials):—with reduced compression 32·2 per cent. and with normal compression 30·2 per cent. To put it in another way, when the density (δ) of the charge is reduced in the ratio of 0·575, the heat-loss from the gas is reduced in the ratio 0·61, or in proportion to $\delta^{0\cdot9}$, the temperature of the gas (or strength of mixture) and of the exposed metal-surface being identical in the two cases. The matter is further complicated by the heat given up after release, which will probably follow a law rather different from that giving the loss during explosion and expansion.

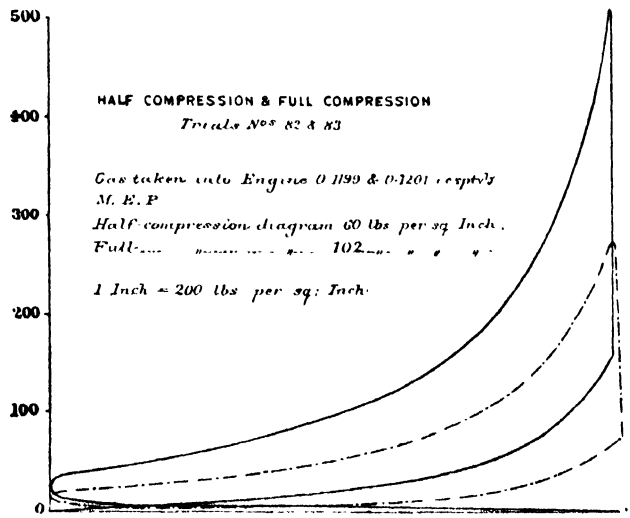


Fig. 7.

An estimate of the heat actually given up by the gas during explosion and expansion, and excluding that lost after release, can be formed from the temperature-fall in the piston (centre to point B). This is nearly proportional to the heat received by the piston. In trials 78 and 79 the ratio of this fall in the two cases is 58 : 90, or 0·64; and in trials 82 and 83 it is 61 : 91, or 0·67. The mean of the two ratios is 0·665, which is equal to $(0\cdot575)^{0\cdot75}$. This estimate is free from the complication of the heat-loss after release, but, on the other hand, the measurement is not very accurate. Substantially, however, it is clear that the heat-loss in the gas-engine increases nearly, but not quite, in proportion to the density of the gas.

In applying this result to the practical purpose of predicting the effect of an increase of compression made by reducing the clearance without other alteration, it is to be remembered that the greater part of the

heat-loss occurs during and soon after the explosion. Thus, if the clearance is halved, the density during the time that really matters will be doubled, and the heat-loss per unit area of exposed surface will be nearly doubled also. The temperature of the exposed metal, which depends upon the heat loss per unit area, may be expected to rise almost in proportion to the compression ratio. The exhaust-valve temperature, however, will not increase so much, because it is determined largely by the temperature of the exhaust-gases, and these will be cooler when the compression is greater. The total heat removed by the jacket may be roughly expressed as

$$H = Ah + E + F,$$

where A denotes the area of metal exposed when the piston is at a point somewhere near the in-centre,

h denotes the heat-loss from the gas per unit area,
E denotes the heat lost after release, and
F denotes the heat developed by friction.

As the clearance is reduced, the stroke remaining the same, h tends to increase rather less than in inverse proportion to the clearance volume, A diminishes, but not in proportion to the clearance volume, E diminishes, and F remains nearly constant. While the compression-ratio is moderate—say not more than 3—E and F form a large proportion of the whole, and A may be diminished materially by reducing the clearance. Thus, a moderate increase of compression may not cause any increase in total heat-loss, the diminution in A and E counterbalancing the increase in h. But as the compression increases (with ordinary ratios of stroke to diameter), the surface A tends to a constant value—namely, something rather more than twice the area of the piston-face. At the same time E diminishes, until with high compression ratios of 6 or more, the term Ah becomes dominant, and the total heat-loss increases rapidly with the compression ratio, though not quite in proportion thereto. When that point has been reached, further increase of compression may be accompanied by great reduction in the efficiency-ratio (*i.e.* the ratio of the actual to the theoretical efficiency for the cycle used) and the efficiency may actually diminish. Some approach to this state of things was observed by Professor Burstall in his recent trials. The really important effect of increasing compression, however, is that the temperature of the exposed surfaces inside the engine rises roughly in proportion to the compression ratio, and this fact (among others) sets a practical limit to compression which is some way short of that at which the efficiency begins to fall off.

The Effect of Missing Ignitions.

When the load on the engine is reduced without altering the gas taken per suction the engine misses ignitions. The heat given to the metal by the hot gases per explosion remains much the same as before, and the heat given to the engine per minute therefore (speed remaining the same)

will be reduced approximately in proportion to the number of explosions. Some part of the heat so given to the metal, however, will be taken away by the charges of cold air which now pass through the engine in the idle strokes. The magnitude of this effect is of some importance in connection with the use of scavenging, and some experiments were therefore made with the object of determining it. Several pairs of trials were made on the same day, in the first of which the engine was run in the ordinary way firing every time; while in the second the load was reduced so that it missed ignitions every other cycle, the gas-cock being adjusted so as to make the charge of gas taken per suction as nearly as possible the same in the two cases. In one such pair the volume of gas taken per suction was 0·128 cubic foot, and the total heat removed by the jacket and radiation (temperature of the engine being precisely the same in the two cases) was 2030 B.TH.U. per minute at full load, and 1103 B.TH.U. per minute at half-load. The heat removed per explosion cycle was 25·2 B.TH.U. in the full-load trial, and 23·7 B.TH.U. in the half-load trial; about 1 B.TH.U. is to be deducted in each case for piston-friction, which is the same for both, leaving 24·2 B.TH.U. and 22·7 B.TH.U. respectively as the heat received from the gas and removed by the jacket-water and by radiation. It appears that rather less heat goes into the jackets per explosion when the engine is partially loaded, but this is accounted for entirely by the fact that with the scavenging charges the strength of the mixture is less because it is diluted with a greater weight of air left in the clearance-space; and also because in this engine about 4 per cent. of the gas taken into the cylinder under these conditions is discharged unburnt. In consequence of the weakening of the mixture a smaller percentage of the heat developed at each explosion is lost to the cylinder-walls and the difference is such as to completely account for the reduced heat-loss to the jackets. The inference is that the air passing through the engine in the idle strokes at half-load removes only an insignificant percentage of the whole heat received by the metal from the hot gases. This inference was confirmed by the following experiment: The engine having been run for some time at full load until the inner surfaces were thoroughly hot, the load was suddenly thrown off and the engine was motored round with the gas cut off so that it took in and expelled charges of cold air at every cycle. As soon as the load had been thrown off a thermometer was inserted in the exhaust-pipe in order to ascertain the rise of temperature of the air passing through the engine. It was found that the air in the exhaust-pipe under these circumstances was about 45° C. hotter than that in the inlet pipe. About 30° C. of this is to be ascribed to the work done upon the air in passing through the engine, leaving about 15° C. only to represent the heat taken in by the air from the metal surfaces. The volume of air dealt with in this test was about 1 cubic foot per suction, and the heat required to warm this quantity of air through 15° C. at constant pressure is approximately $\frac{1}{2}$ B.TH.U. This is, therefore, about the amount of heat removed

from the engine by the air passing through it in a scavenging stroke, and it amounts to not more than 5 per cent. of the heat received from the hot gases in an explosion-stroke. It should be observed that observation was kept on the temperatures of the piston, exhaust-valve and inlet-valve during the time that the engine was being motored round, and that these changed by only an insignificant amount in the time necessary to get the temperature of the air in the exhaust-pipe. A close approximation to this temperature was obtained in a short time by making a series of tests, in some of which the thermometer was slightly below the air-temperature before it was inserted into the pipe, while in others it was slightly above; in the first case the temperature shown by the thermometer after it had been in the pipe for 4 or 5 seconds was a little below, and in the second case a little above, the true temperature of the air. Limits were thus obtained between which the two temperatures must lie, and after a few trials, these limits were brought so close that the real temperature was known within 1° or 2° C.

The temperatures of the exhaust-valve and inlet-valve when running at half-load were closely in proportion to the total heat-loss H. In the case of the piston, the ratio θ/H was about 10 per cent. greater at half-load than at full-load corresponding with a difference in temperature of about 25° C. Friction of the piston, practically the same in both tests, is sufficient to account for the relatively higher temperature when the engine is missing ignitions. These facts give further confirmation of the inference that the air passed through the engine in an idle stroke has no practical effect in removing heat.

Pre-Ignition.

When the metal in the interior of a gas engine exceeds a certain temperature at any point it causes spontaneous ignition of the charge and so spoils the running of the engine. It is this fact more than anything else that sets a limit to the possible size of engines in which there are parts not water-jacketed. It was decided, therefore, to investigate this limiting temperature, and thus to make more complete the results described in this paper.

Under ordinary conditions of full-load running, the engine runs quite satisfactorily without any sign of pre-ignition. In fact under no conditions of running has it been found possible to raise the temperature of any part so far as to cause ignition of the charge. In order to produce pre-ignition, therefore, it was necessary to introduce a special piece of metal which would be heated up by the explosions. For this purpose an iron bolt, about 4 inches long, was screwed into the exhaust-valve cover so that it projected into the centre of the compression-space. The bolt carried at the inner end a thermo-junction, formed by passing a nickel wire down a hole drilled along the axis of the bolt and brazing it to the iron at the projecting end (Fig. 8, page 318). This thermo-couple was exactly similar in

construction to that used for measuring the temperature of the piston and was calibrated in the same way. It was found that the end of the bolt became sufficiently hot to cause ignition of the charge if the proportion of gas was raised rather above the normal value. The ignition doubtless originated at the hottest point on the surface of the bolt, which would be near to, though not exactly coincident with, the thermo-junction. The temperature recorded by the junction, therefore, would probably be a little lower than the actual temperature of the metal which caused the ignition, but the difference could not have been very great, and some idea of its possible magnitude was attained by varying the shape of the bolt in the neighbourhood of the thermo-junction.

It was found that, if the engine were started with one of these bolts in position and run firing every time, the temperature of the bolt would rise rapidly at first, but would in the course of a few minutes reach a steady value, at which, if not high enough to cause ignition of the charge, it would remain. The bolt could in this way be raised to a temperature of about 700° C., and kept continuously at that temperature without any

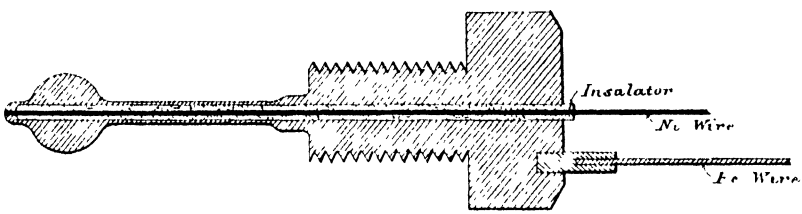


Fig. 8. Section of bolt used for obtaining pre-ignitions.

apparent effect upon the running of the engine. A slight increase in the gas-charge, however, would cause the temperature of the bolt to rise still further, with the result that ignitions from it would commence. Such ignitions could easily be distinguished by the sound, and also by the fact that they were accompanied by an abnormal rise in the temperature of the bolt. In consequence of this increase of temperature the ignitions tended to become more and more frequent and earlier, the bolt-temperature at the same time rising until, after a few explosions, the charge would fire as soon as it entered the cylinder and the engine would pull up.

It was found that the best way of exhibiting these phenomena was to plot the temperature of the bolt as a function of the number of explosions in the engine. The observer watched the galvanometer and at the same time counted the explosions, plotting down the temperature at every twenty explosions, or more frequently if it was varying rapidly. Fig. 9 shows samples of curves taken in this way. Taking No. 1 as an example, it will be seen that the temperature, which at the commencement of observation was about 640° C., rises steadily to about 710° C. in the course of 280 explosions, or at the rate of about 0·25° C. per explosion. During this period

the ignition was normal, except for an occasional ignition from the bolt in the latter part, giving rise to slight irregularity in the curve. From 280 to 290, however, every ignition was from the bolt, with the result that its temperature rises in that period by 25° C., or about ten times as fast as with normal ignitions. The temperature then remains nearly steady for twenty explosions; during this period most of the ignitions were normal. Pre-ignitions commence again, however, at a temperature of about 740° C., and continue without intermission until the engine pulls up. A number of tests of this kind were made with similar results.

In the tests described in the last paragraph the charge of gas was about 0·133 cubic foot per suction, and with this charge the bolt soon reaches the ignition-temperature. If the charge is reduced the rate of rise of temperature becomes less, until finally the steady temperature reached by the bolt is below the critical value, and ignition from the bolt never occurs. It was found that with a gas-charge of 0·1265 cubic foot

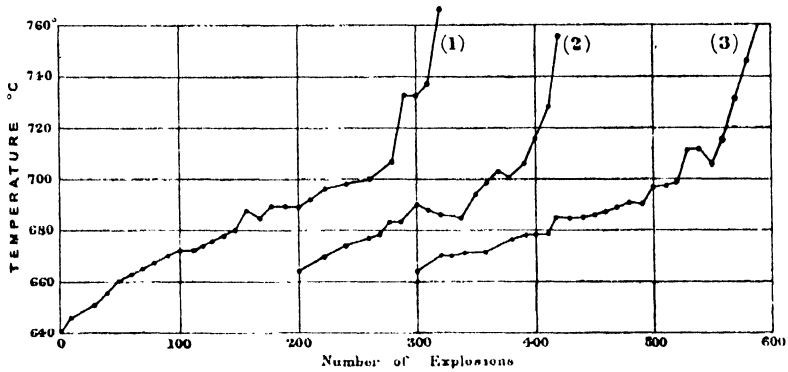


Fig. 9. Pre-ignition curves.

per suction the bolt reached a steady temperature of 670° C., and the engine ran continuously and perfectly satisfactorily under these conditions. When, however, the gas-charge was increased to 0·1282—or about 1 per cent.—the temperature rose to 690° C., at which point ignitions from the bolt commenced. At first they only occurred occasionally—perhaps one in every ten explosions—but each one caused the temperature of the bolt to rise by a few degrees, with the result that they became more and more frequent, until at a temperature of 725° C. they were continuous, and the engine pulled up after about twenty explosions. This experiment, which was repeated with the same result, illustrates in a striking way the very narrow limits which divide safe conditions of running from those which are impossible on account of pre-ignition.

The instability, which is the cause of this narrow dividing line, is perhaps from a practical point of view the most important point in these experiments. It is probably due to the fact, pointed out some years ago

by the author and established by experiments on explosion in a closed vessel*, that whenever an explosive mixture is ignited from one point, the gas at the point of ignition is, just after the explosion is completed, very much hotter than the gas at a distance. The reason is that the gas at the point of ignition is first ignited almost at constant pressure, and is then compressed during the progress of the flame through the vessel. On the other hand, the gas at those points which are last reached by the flame is compressed by the oncoming flame before it is ignited, and its pressure and temperature rise but little after its combustion is complete. The result is that when the flame has spread completely through the space, the temperature at the point of ignition, apart from the cooling action of the walls, is generally several hundred degrees higher than the temperature at a distance. Thus, when the ignition takes place from the bolt, the very fact that it has taken place there increases the temperature of the gas round about the bolt, and so raises the temperature of the bolt itself. Thus the ignitions from the bolt tend to become more and more frequent and earlier, on a compound-interest principle, giving rise to the instability to which reference has been made.

The experiments so far recorded show that with the particular bolt figured in Fig. 8, ignition never occurs so long as the temperature at the tip is below 690° C., and it occurs every time if the temperature exceeds 740° C. As already pointed out, however, the thermo-couple may not be quite the hottest part of the bolt, and it is probable that the true temperature of ignition is nearer to the upper than to the lower of these limits. A small piece of scale, for example, on the surface of the bolt, might be 20° or 30° hotter than the sound metal just below the surface, and would cause ignition at an apparently lower temperature. In order to test this matter further, experiments were made with a bolt of different shape, consisting of a cylindrical rod of iron about 6 inches long and $\frac{3}{4}$ inch in diameter, with a screw-thread cut along the whole of its length to give greater surface. The thermo-couple was in the centre of the projecting end which was flat. In this case ignition never occurred at a lower temperature than 730° C., and it always occurred at 750° C.; the ignition point was thus decidedly more definite and slightly higher than in the other case.

It should be noted that the metal-temperature is measured by the thermo-couple, not the gas-temperature. It is probable that the hottest place in the gas is at a little distance from the bolt, and that the temperature is there rather higher than the metal-temperature. At the commencement of compression the gas a few millimetres from the bolt will have a temperature but little below that of the metal, and this will be raised above the metal-temperature by the compression, which at such a point will be more or less adiabatic. This consideration prevents any definite

* *Proceedings of the Royal Society*, A, vol. 77 (1906), p. 387, and page 367 of this volume. See also *Engineering*, vol. 81 (1906), p. 777.

inference as to the true ignition-temperature of the gas-mixture, as to which it can only be asserted that it is above 740° C. But the practically important point is that a clean metal-surface will ignite the gas when its temperature is a little above 700° C., and will not ignite it if it is below that figure.

The progress of the ignition was studied by taking indicator diagrams with the eccentric driving the indicator turned through a right-angle. The pressure in the neighbourhood of the ignition point is then shown approximately on a time basis. Tracings of such diagrams under normal conditions are shown in Fig. 10; the tripping of the magneto occurs at *A*,

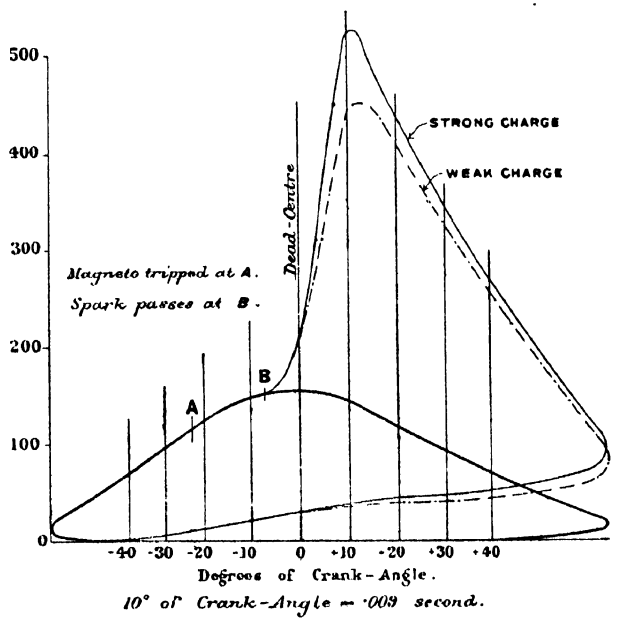


Fig. 10

and the spark passes at *B* slightly before the in-centre. The intervening time—about $\frac{1}{60}$ second—is that required for the operation of the igniter mechanism. Maximum pressure is reached from 0·014 to 0·02 second after the passage of the spark according to the strength of mixture.

Figs. 11 and 12, p. 322, are reproductions of diagrams taken after ignition from the bolt has commenced. In Fig. 11 the shutter of the camera was held open and every explosion is shown. In Fig. 12 the shutter was opened for one or two explosions and then closed for five explosions. Some normal ignitions, taken before the bolt was hot enough to fire the charge, are also shown in this diagram. The rapid advance of the ignition as the bolt heats up appears clearly in both diagrams.

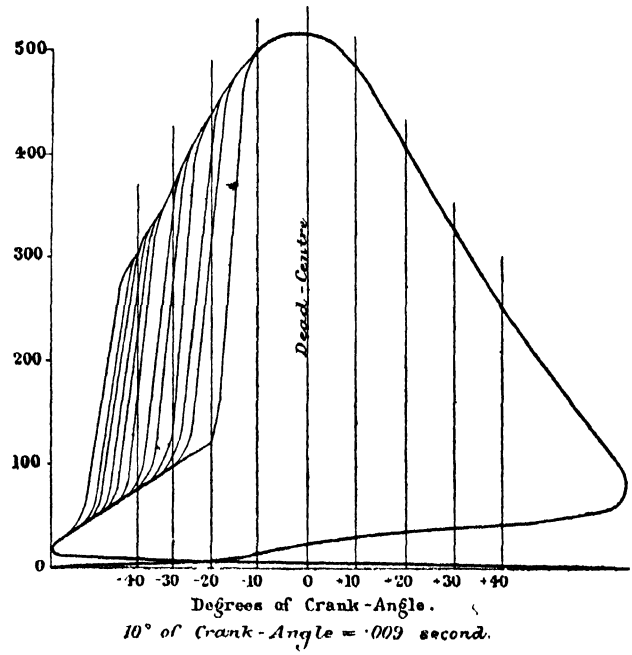

500
400
300
200
100
0
Dead-Centre
-40 -30 -20 -10 0 +10 +20 +30 +40
Degrees of Crank-Angle.
10° of Crank-Angle = ·009 second.

Fig. 11.

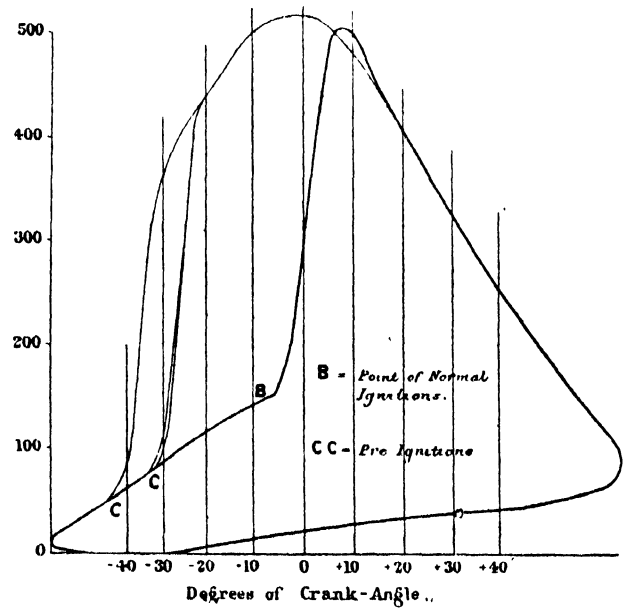

500
400
300
200
100
0
B
C
C
C
B = Point of Normal Ignitions.
C C = Pre Ignitions
-40 -30 -20 -10 0 +10 +20 +30 +40
Degrees of Crank-Angle.

Fig. 12.

Fig. 13 is a diagram of the ordinary kind showing pre-ignitions in an advanced stage. Photographs of the normal diagram were also taken on the same plate for comparison. It will be seen that the maximum pressure reached after the early ignitions is not materially higher than that following normal ignition. This is due to the very rapid loss of heat which occurs just after explosion while the flame is being compressed. The flow of heat is so fierce along the line *CD* that the mean temperature of the gas is practically constant in spite of the work that is being done upon it. Something like 12 B.TH.U. per explosion are lost between the points *C* and *D*, and the total loss in a cycle must be increased by 50 per cent. above its normal value.

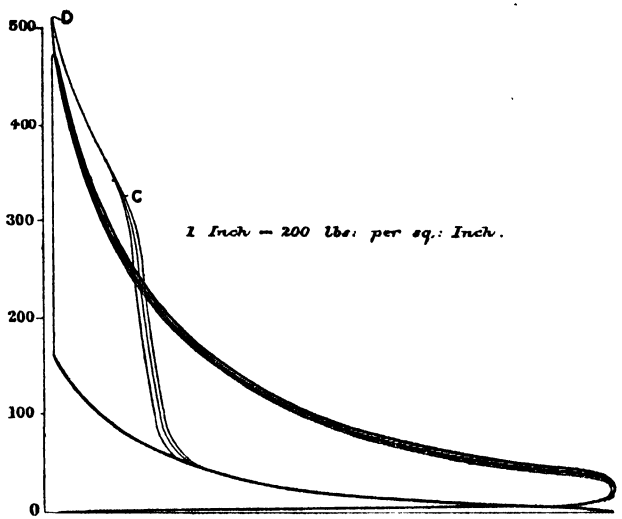


Fig. 13. Pre-ignitions.

The fact that, as the temperature of the bolt rises, ignition occurs at a lower compression is of some interest. It may be explained on the supposition that the temperature of the gas a little way from the bolt is raised by compression above the metal-temperature, as already explained; or it may be held to show that the temperature of ignition gets lower as the density of the mixture increases. That increased density does lower the temperature of ignition to some extent is quite certain. Professor Dixon stated at the Dublin meeting of the British Association that he had found this to be the case; and the author has also found it in experiments of his own by quite a different method. It was found that when the bolt reached a temperature of about 800° C., the charge ignited as soon as it came into the engine, that is at atmospheric pressure; but this is necessarily a very rough estimate.

A large number of tests were made with different strengths of mixture, and it was found that within fairly wide limits the temperature of ignition

was independent of the strength of mixture. Injection of water into the engine, through the suction-valve provided for that purpose, reduced the rate of rise of temperature of the bolt, and sometimes prevented it from reaching the critical temperature. Even in considerable quantities, however, it did not prevent pre-ignition if the bolt got hot enough, nor did it alter the temperature at which pre-ignition occurred. The water appears simply to exercise a cooling effect, and if it stops pre-ignition does so simply by preventing the metal-surfaces from reaching the critical temperature.

So far reference has been made only to the action of clean metal surfaces, or surfaces covered with a thin coating of oxide, in causing pre-ignitions. The nature of the metal does not appear to make any difference, for it was found that a copper bolt caused ignition at the same temperature, certainly within 20° C., as an iron bolt. In this case the temperature was measured by means of a "constantan" wire, and the bolt was cylindrical and screwed along its length as in the case of the iron bolt last described. This observation makes it probable that the ignitions originate in the gas and are not produced by any catalytic action. It is possible, however, that other substances (*e.g.* carbon) deposited on the metal surfaces may exert such an action, which of course must be in the direction of lowering the ignition-temperature.

Experiments were made on the effect of oil in producing ignition. An arrangement was fitted vertically above the exhaust-valve by means of which oil could be dropped on to the centre of the valve, and it was found that in this way pre-ignitions of considerable violence could be produced when the surface upon which the oil fell had a temperature as low as 450° C. The oil was evidently split up by contact with the hot surfaces, as a good deal of carbon was discharged into the exhaust, and it seems probable that (as has been several times suggested) the ignition is due to the evolution of hydrogen which occurs when the oil becomes decomposed. It was found that a moderate injection of water had the effect of completely stopping pre-ignitions of this kind. Ignitions due to casual drops of oil can obviously occur quite readily without any part of the engine being over-heated, and it is probable that they do occur not infrequently in practice. But so long as the supply of oil which causes them is not continuous they are not likely to cause much trouble, since they do not tend to propagate themselves as do pre-ignitions caused by hot surfaces, and moreover, as already pointed out, they can be stopped by the injection of a small quantity of water.

APPENDIX I.

Nickel-Iron Thermo-Couples.

The nickel wire was obtained from Messrs Johnson, Matthey and Co., being described as "best commercially pure*"; it was of 0·015 inch diameter, and the same batch of wire was used for all the thermo-junctions. The bolts were of ordinary wrought-iron, and the second connection was made with common wrought-iron wire. There was no material thermo-electric force between the iron wire and the bolt.

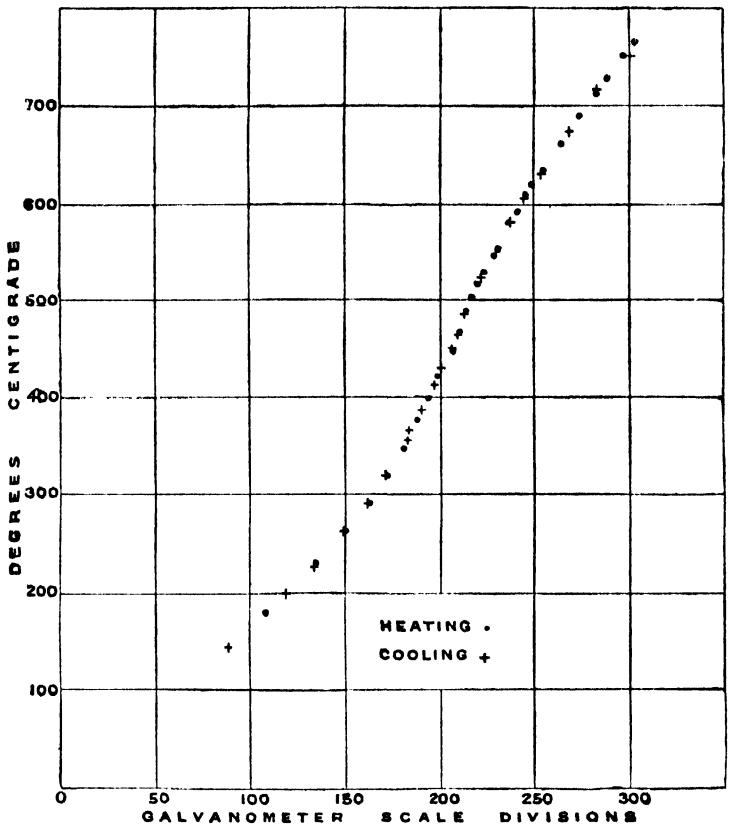


Fig. 14. Calibration-Curve of Ni-Fe Thermo-Couple No. III (Inlet-Valve).

The thermo-couples were calibrated in an electric furnace against a Callendar pyrometer, the latter being checked from time to time. One such calibration-curve is shown in Fig. 14, the temperature being shown in terms of scale divisions of the galvanometer connected to the thermo-couple. The resistance in the circuit was always 16 ohms, together with

* It was not analysed, but Messrs Johnson, Matthey and Co. inform the author that it would contain 98·5 to 99 per cent. of nickel and about 0·75 per cent. of cobalt, the other impurities being mainly iron, copper and silicon.

about 5·8 ohms for galvanometer and leads—total, 21·8 ohms. One division on the galvanometer scale was equal to 2·57 micro-amperes, or 56 micro-volts.

The following table gives the results of all the calibrations made in the research. Where two or three calibrations are given for one thermo-junction, it was used for measuring engine temperatures in the intervals between the calibrations. Except in the case of the first exhaust-valve couple, the electromotive force varied very little with use.

Calibrations of Thermo-Couples.

Cold-junction at 0° C.

No.	Date	Electromotive force in hundredths of a volt					
		200°	300°	400°	500°	700°	
I	11.10.07	0·65	0·90	1·11	1·23	1·59	Exhaust-valve
	13.1.08	0·69	0·96	1·12	1·26	1·62	
II	8.10.07	...	0·91	1·08	1·20	...	Piston *A* and *B*
	14.1.08	0·66	0·91	1·07	1·20	...	
	9.7.08	0·69	0·93	1·08	...	...	
III	15.1.08	0·66	0·92	1·09	1·21	1·56	Inlet-valve
IV	16.1.08	0·65	0·91	1·08	1·20	1·55	Exhaust-valve
V	17.1.08	...	0·94	1·10	1·22	1·57	Pre-ignition bolt
VI	24.2.08	0·67	0·93	1·10	1·24	...	Piston-centre
	8.7.08	0·68	0·93	1·09	1·24	...	
VII	4.6.08	0·66	0·91	1·06	1·24	...	Inlet-valve
VIII	3.6.08	...	...	...	1·22	1·58	Pre-ignition bolt
IX	18.6.08	...	...	...	1·25	1·61	Pre-ignition bolt
	25.6.08	...	...	...	1·25	1·62	
X	19.6.08	...	0·93	1·07	1·21	...	Exhaust-valve
XI	1.7.08	...	...	...	1·19	1·53	Pre-ignition bolt

These may be compared with the values obtained by Mr E. P. Harrison* for the Fe-Ni junction, which were as follows:

200°	300°	400°	500°	700°
0·63	0·91	1·07	1·20	1·52

Usually the galvanometer deflections, whether in taking temperatures on the engine or in calibrating, were first corrected for the temperature of the cold-junction by adding the number of divisions corresponding to the electromotive force between that junction and a junction at 0° C.; the correction on this account was small, amounting to about 0·6 division per degree. As it was also much the same for the calibration and for the temperature measurement it had not much effect on the result and was omitted

* *Philosophical Magazine*, S. 6, vol. 3 (1902), p. 177.

where great accuracy was not important; in that case the deflection was taken to represent the difference of temperature between the hot and cold junctions, irrespective of the temperature of the latter, both in calibration and in the engine.

The temperature equivalent of one division on the scale ranged from $1\frac{1}{2}°$ to 6° C. at different points; the scale could be read to one division easily. Where there were two calibrations of the thermo-junction, one preceding and the other following a trial, the mean was taken. The results given by the two calibrations in such cases never differed by more than 15° C. Allowing for errors in the measurement of the cold junction temperature, which were considerably magnified at higher temperatures, it is probable that the absolute measurements are in all cases correct to within 15° C., and that differences of temperature taken simultaneously, *e.g.* between the two points in the piston, are correct to within 5° C.

APPENDIX II.

The Conduction of Heat in a Circular Disk.

The problem is to determine the steady distribution of temperature in a circular disk bounded by two parallel planes, having given that over one plane face heat is received at a uniform rate per unit area, while over the other there is no flow of heat, the cylindrical surface being maintained at a uniform temperature. The accurate solution requires the use of Bessel's functions, but a sufficiently close approximation can be obtained as follows:

Take the origin of co-ordinates at the centre of one face (Fig. 15, p. 328), and let Oz be the axis of the disk. Let θ be the temperature at any point (x, y, z), then θ must satisfy the equation

$$\frac{\partial^2\theta}{\partial x^2} + \frac{\partial^2\theta}{\partial y^2} + \frac{\partial^2\theta}{\partial z^2} = 0.$$

A solution of this equation is:

$$\theta = x^2 + y^2 - 2z^2$$
$$= r^2 - 2z^2,$$

where r is the distance of the point from the axis. This solution gives $\frac{\partial\theta}{\partial z} = 0$ over the plane $z = 0$, and $\frac{\partial\theta}{\partial z} = -4t$ over the plane $z = t$. In other words, it satisfies two of the conditions stated above, namely, no flow of heat over one plane face and a uniform flow ($4kt$) over the other plane face. It does not, however, satisfy the third condition that the cylindrical surface must be at a uniform temperature, for over that surface (putting a for the radius of the disk) $\theta = a^2 - 2z^2$.

In the case of a disk whose radius is four times its thickness, which closely approximates to the proportions of the piston-face in the Crossley

gas-engine, the temperature on the cylindrical surface given by this solution varies from 16 to 18 (in arbitrary units), the centre of the exposed face (P) being taken as zero. The isothermal surfaces are hyperboloids of revolution, each having the equation $r^2 - 2z^2 = \theta$, where θ is the temperature. These surfaces are shown dotted in Fig. 15, being drawn for equal intervals of temperature except the first three, the interval between which is half that between the others. The temperature θ is marked against each isothermal.

At points not very near the edge, the solution will obviously give a tolerably close approximation to the temperature distribution in a disk the cylindrical edge of which is at a uniform temperature equal to the mean of the actual temperatures, or say 17 in the case shown in the figure. Further, it is clear that with a given uniform heat-flow equal to the $4kt$ over the exposed face, the actual temperature at the centre P will differ from that at the edge (supposed kept at the same temperature all over) by more than

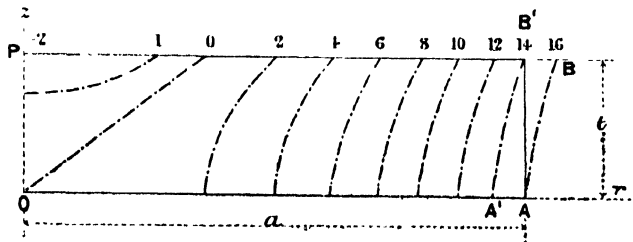


Fig. 15. Isothermal surfaces in a circular disk.

16 and by less than 18. These limits are only 12 per cent. apart, so that a calculation of the temperature at P, based upon the mean of them, cannot be in error by so much as 6 per cent. and is probably correct within 2 or 3 per cent. The uncertainty of the various factors entering into the practical problem is greater than this.

In general, the difference of temperature between the centre of the exposed face and the edge will always lie between $\frac{ha^2}{4kt}$ and $\frac{h(a^2 + 2t^2)}{4kt}$; where a is the radius and t the thickness of the disk, and h is the rate of heat-flow per unit area. When the disk is very thin these limits become equal to one another and to $\frac{ha^2}{4kt}$. The difference of temperature between the centre and a point in the face distant r from the centre will be very nearly $\frac{hr^2}{4kt}$, provided that the distance of the point from the edge is not less than the thickness of the disk.

APPENDIX III.

On the Stresses in an Unequally Heated Disk.

Let the temperature at a distance r from the axis of the disk be $\theta = \theta_0 \frac{r^2}{a^2}$, where a is the radius and θ_0 is the temperature at the edge. This is approximately the distribution of temperature in a disk heated uniformly over one face as described in Appendix II, but in that case θ_0 is, of course, negative.

Let u be the radial displacement of a point (distant r from the axis) from the position occupied when the disk has everywhere the temperature zero. It is assumed that this is the same throughout the thickness of the disk. The strains (in which term is included that part of the change in size and shape which is due to temperature-difference as well as that due to stress) are du/dr (radial) and u/r (tangential). The effect of the heating is to enlarge an element distant r from the centre in the proportion $1 + \alpha\theta$, where α is the coefficient of linear expansion. Hence that part of the strain which is due to stress is:

$$\frac{du}{dr} - \alpha\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{radial}$$

$$\frac{u}{r} - \alpha\theta \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{tangential.}$$

The corresponding stresses are:

$$\left.\begin{aligned} P &= \frac{E}{1-\sigma^2}\left\{\frac{du}{dr} + \sigma\frac{u}{r} - \alpha(1+\sigma)\theta\right\} \text{ . radial tension} \\ T &= \frac{E}{1-\sigma^2}\left\{\frac{u}{r} + \sigma\frac{du}{dr} - \alpha(1+\sigma)\theta\right\} \text{ . tangential or "hoop" tension} \end{aligned}\right\} \quad \ldots(1)$$

where E is Young's modulus and σ is Poisson's ratio. It is here assumed that there is no stress in a direction parallel to the axis.

From the equilibrium of an element of the disk, which is acted upon by no forces except those due to stress, there is obtained:

$$\frac{d}{dr}(Pr) = T;$$

whence, substituting for P, α, and T from (1) there follows after some reductions:

$$r\frac{d^2u}{dr^2} + \frac{du}{dr} - \frac{u}{r} = \alpha(1+\sigma)\, r\frac{d\theta}{dr}$$

$$= \frac{2\alpha(1+\sigma)\, r^2}{a^2}\theta_0$$

since $$\theta = \theta_0 \frac{r^2}{a^2}.$$

The general solution of this equation is

$$u = Ar + \frac{B}{r} + \frac{\alpha(1+\sigma)\,\theta_0 r^3}{4a^2}$$

where A and B are constants.

This is identical in form with the equation giving the displacement in a rotating disk, if obtained on the same assumptions as to the distribution of strains*. The velocity of rotation ω of the disk must be taken as given by

$$\omega^2 = -\theta_0 \frac{E}{1-\sigma} \cdot \frac{2\alpha}{a^2\rho}$$

where ρ is the density of the material of the disk. If θ_0 is positive, *i.e.* if the edge is hotter than the centre, ω^2 becomes negative and ω is imaginary. In other words, the centrifugal force must be taken as acting inwards instead of outwards. In the case of a gas-engine piston the edge is cooler than the centre and θ_0 is negative; ω is then real and the correspondence with the rotating-disk problem is exact, except as regards the boundary-conditions.

The constants A and B are obtained from the boundary-conditions, which are that there is no radial stress (P) at the edge, and that $u = 0$ at the centre. Application of these conditions gives:

$$A = \alpha\theta_0 \frac{1-\sigma}{4}$$

and

$$B = 0,$$

whence

$$u = \frac{\alpha(1+\sigma)\theta_0 r^3}{4a^2} + \frac{\alpha(1-\sigma)\theta_0 r}{4} \quad \ldots\ldots(2)$$

Putting $r = a$ in this expression the radial displacement at the edge is found to be

$$u = a\alpha\frac{\theta_0}{2},$$

or the expansion is half that which would occur if the whole disk were heated to temperature θ_0.

By substituting in equation (1) the value of u given by (2), the stresses at distance r are found, namely:

$$P = \frac{E\alpha\theta_0}{4}\left(1 - \frac{r^2}{a^2}\right)$$

$$T = \frac{E\alpha\theta_0}{4}\left(1 - \frac{3r^2}{a^2}\right)$$

and at the edge the hoop-tension T is $-\frac{1}{2}E\alpha\theta_0$, or half of that due to the radial displacement.

In the problem presented by the gas-engine piston the edge is cooler than the centre and θ_0 is negative. There is then a contraction at the edge equal to half that which would occur if the whole disk were cooled to the edge-temperature. Superposed on this is the expansion corresponding with the uniform heating of the whole disk to the temperature of the centre, which of course would produce no stress of itself. There is a positive hoop-tension at the edge. In the case of the rotating disk problem, the boundary condition is still that the radial tension shall be zero. But, owing to the ab-

* See Ewing, *Strength of Materials*, p. 215.

sence of the temperature terms in the stress-strain relations, this condition gives rise to a different equation when expressed in terms of u. For the rotating disk:

$$P = \frac{E}{1-\sigma^2}\left\{\frac{du}{dr} + \sigma\frac{u}{r}\right\},$$

while in the heated disk

$$P = \frac{E}{1-\sigma^2}\left\{\frac{du}{dr} + \sigma\frac{u}{r} - \alpha(1+\sigma)\theta\right\}.$$

The condition $P = 0$ in the latter case is therefore equivalent to taking $P = -\frac{E\alpha\theta}{1-\sigma}$ in the former. In other words, a radial pressure must be supposed to exist all round the edge of the rotating disk, such as might be produced by winding it with wire under tension, in order to bring it into exactly the same state as the heated disk as regards stress.

This solution of the problem of the unequally heated disk is based on the same assumption, and is subject to the same limitations as regards accuracy as the usual solution of the rotating disk problem. The assumption that there is no stress in a direction parallel to the axis, which is used in obtaining equation (1), requires that there shall be a displacement w in that direction. In the absence of any normal forces applied to its plane face, the disk will obviously become thicker or thinner when it is heated or subjected to centrifugal force. The displacement w parallel to the axis is readily calculated from the displacement u coupled with the condition of no normal stress, and is equal to

$$-\frac{\sigma z}{1-\sigma}\left(\frac{du}{dr} + \frac{u}{r} - 2\alpha\theta\right)$$

where z is the distance from the middle plane of the disk. This displacement, varying as it does with the radius, implies the existence of shearing stress proportional to $\frac{dw}{dr}$ which does not vanish at the plane faces as it should do. The simple solution given above may be taken as giving accurately the displacement and stresses in a disk whose plane faces, instead of being free as they are in actual fact, are subject to radial tractions of amount equivalent to the value of $\frac{dw}{dr}$ at those faces. These tractions are, however, small for a thin disk, and make no practical difference to the main stresses P and T in the disk. Another way of putting the matter, which was suggested to the author by Mr Inglis, lecturer in the Engineering Laboratory at Cambridge, is to say that there must, in the actual rotating disk, be some shearing stress in the axial planes, but that this vanishes at the free plane surfaces and also (by symmetry) at the plane midway between them. There is no room for it to develop any considerable value in the half thickness of the disk, and its influence may for practical purposes be neglected when the disk is thin. The approximation is of the same

kind and of the same order of accuracy as the assumption, in the elementary theory of bent beams, that plane sections remain plane.

Dr Chree has given a more accurate theory of the rotating disk, which takes account of the shearing stresses but still requires the application of radial forces over the cylindrical edge of the disk in order that the boundary conditions may be satisfied*. From this solution the error involved in the simpler one can be calculated; and it is so small as to justify the use of the latter in most practical cases.

* *Proceedings of the Cambridge Philosophical Society*, 1890.

21

A NEW METHOD OF COOLING GAS ENGINES.

["Proc. Institution of Mechanical Engineers," 1913.]

The Problem of Cooling. The most important peculiarity of the gas engine, that which determines the characteristic features of its design and operation, is the heat-flow from the hot gases into the cylinder-walls. About 30 per cent. of the heating value of the fuel passes into the metal of the engine in this way, and it is necessary to provide means for its removal as fast as it goes in. In all engines hitherto made (except the small air-cooled engines) the removal of the heat has been effected by the circulation of water round the cylinder, and (in large engines) in the substance of the piston and exhaust-valve. External water-cooling is the ultimate cause of most of the disadvantages under which the gas engine has hitherto laboured and which have retarded its development in large sizes. It is obvious that the provision of a jacket, and of the elaborate appliances necessary for the circulation of water in the moving piston and exhaust-valve, must be largely responsible for the great weight and cost of large engines of this type.

For many purposes these disadvantages might not in themselves be serious, having regard to the superior economy, were it not that there are secondary effects of this method of cooling which tend to make a large engine unreliable in working. In order that the heat may be caused to flow from the inner surface of the metal where it enters to the outer surface where it is removed, there must be a difference of temperature between these surfaces proportionate to the thickness. The necessary difference is of the order of 50° C. per inch, and while not of much moment in small engines, it may become serious in large sizes where the cylinder-walls are in places 3 inches thick or more. Furthermore, it is difficult in large engines to secure an adequate circulation about all parts of the cylinder-walls and piston, and some parts may become much hotter than others. The inequalities of temperature so set up in the metal are detrimental in two ways. In the first place they cause stresses, which are very liable to crack a casting already (by reason of the double wall required to form the jacket) difficult enough to design and manufacture. Secondly, the overheating of certain parts of the inner surface, whose temperature as pointed out is much above that of the water, is apt to cause pre-ignition of the charge, especially if deposits of carbon or tar are formed. Such deposits on a surface already overheated may, owing to their poor conducting

power, easily reach a temperature sufficient to fire the charge before the proper time. Pre-ignitions so caused, apart from their effect in reducing the efficiency and power of the engine, are a source of danger because they cause an excessive development of heat especially in the neighbourhood of the pre-igniting point, and also result in higher maximum pressures. In consequence of the dangers of overheating, it has been found impossible to work gas engines, especially of large size, continuously at the maximum power which they can develop. In order to obtain at all satisfactory results, it is necessary to use weak mixtures and even so trouble is apt to arise for the reasons stated. If it were possible to allow large gas engines to work continuously at the maximum power which they are capable of developing for short periods, the cost per horse-power would be reduced by from 20 to 40 per cent.

Cooling by Internal Injection. When once these difficulties, and their cause, have been clearly stated, it seems fairly obvious that they can be overcome by applying the cooling medium on the inside of the cylinder instead of to the outer surface. If water can be injected internally against the surfaces to be cooled, the heat is removed on that side of the metal on which it is generated, and therefore there is no heat-flow through the metal and no difference of temperature between the inner and outer surfaces. The water may be distributed by means of jets so that each part receives it in proportion to the rate at which it receives heat from the hot gases. Thus the engine can be maintained at substantially the same temperature all over and the stresses due to unequal heating may be eliminated. A simple single-walled casting can be used for the cylinder, resulting in a great saving in weight and cost and in improved reliability on account of the elimination of casting stresses. The arrangements for cooling the piston, which are necessary in large engines, can be dispensed with—a point of great importance, because these arrangements, besides being costly, frequently give trouble, and their failure may easily result in wrecking the engine. Finally, pre-ignitions are entirely prevented, for even a thick deposit of carbon, being cooled by the projection of water against the surface into which the heat flows, is kept at a temperature much below that full red heat which is necessary to fire the charge.

The idea of introducing water into an internal combustion engine is not new. It is a common practice in oil engines to introduce water along with the oil in order to enable the compression to be raised, and water has been sprayed into gas engines for the purpose of preventing pre-ignition. Proposals have also been made to introduce water for the purpose of cooling parts of the metal. None of the latter, however, has been a practical success, if indeed they have ever been more than suggestions on paper, apparently because their originators did not appreciate the conditions which must be satisfied if the injected water is to act as an effective cooling agent. Of these the most important is that the water must be

projected in comparatively coarse drops or jets directly against the surfaces to be cooled, so that it reaches these surfaces in the liquid form without much loss by evaporation on the way. Further, it must be distributed properly, so that each portion of the metal receives water in the proportion in which it receives heat. If the water be turned into steam before reaching the metal, it will not exert any cooling effect except indirectly by lowering the temperature of the flame, and such lowered temperature is accompanied by a considerable loss of efficiency*. If the water is not properly distributed, those portions of the cylinder-walls and piston which do not receive an adequate supply must lose by conduction to the properly cooled portions the heat which they receive and, in consequence of the inequalities of temperature so set up, an important advantage of this method of cooling (substantially uniform temperature) is lost. It is of no use to inject the water in a fine spray produced by an atomizer, or to introduce it into the gas- or air-pipe, so that it is carried in, suspended in the incoming charge, or (as is often done in oil engines) to spray it in along with the oil. Though some of these devices have proved useful for the prevention of pre-ignition and for the softening of the explosion, none of them is effective for the purpose of cooling. For that purpose it is necessary to project the water positively and directly against the metal surfaces, by means of properly arranged nozzles in a rose, or its equivalent, projecting into the combustion chamber.

The method of internal injection described in this Paper embodies this principle. Cold water is injected through a hollow casting projecting into the combustion chamber and provided with a number of holes or small nozzles about 1/32 in. diameter. The jets so formed are comparatively

* As there exists, even now, some misapprehension about this point it may be well to explain it rather more fully. Suppose that water to such an amount that its evaporation would absorb, say, one-tenth of the heat of combustion is injected into the cylinder at the moment of explosion, and that the whole of this water is evaporated in the flame and before it reaches the walls. The effect on the flame temperature will be substantially the same as though the supply of the combustible gas had been diminished by the amount required to evaporate the water, that is, in the ratio of 10 to 9, and there will be a corresponding reduction both in the flame temperature and in the flow of heat from the hot gases to the walls. Thus the heat which must be removed by the jacket-water or by evaporation of liquid on the walls will be reduced by roughly one-tenth of itself or, say, from 30 per cent. of the heat of combustion to 27 per cent. The absolute reduction in heat-flow is only of the order of one-third of the heat of evaporation of the water, and it is obviously impossible by this means to reduce the heat-flow to such a point that an external water-jacket can be dispensed with. Moreover, evaporation of water in the flame is accompanied by a considerable reduction in thermodynamic efficiency, because the suppression of the heat required for the evaporation of the water is only very partially counteracted by the added pressure due to the formation of the steam. Roughly speaking, any reduction in heat-flow due to evaporation of water in the flame must be accompanied by a reduction of the same order of magnitude in the work done.

On the other hand, water which reaches the walls in the liquid form and is there evaporated absorbs out of the heat given to the walls by the gas the *whole* of its own heat of evaporation, and there is no loss of thermodynamic efficiency because the heat used is waste heat which in a jacketed engine would go to warm the cooling water. Any steam formed in this way is pure gain; and, if anything, there is an increase in the work done.

coarse, so that even when projected into the flame the water reaches the part of the wall against which it is directed with but little evaporation on the way. The jets are directed to all parts of the surface of the combustion-chamber and against the face of the piston.

The projection of liquid water against the walls and the proper distribution of that water are the first essentials of effective cooling by water-injection; but there are other conditions which must be satisfied in order that the system may be a practical success. It is the experience of all who have had much to do with gas engines, that whenever liquid water has by accident or design been allowed to accumulate on the inner surface of the cylinder, it has been found to have very deleterious effects. Most producer-gas contains a certain proportion of sulphur dioxide. This dissolves very readily in cold water, forming sulphurous acid which rapidly corrodes any metal surface with which it may be in contact. Thus if the cylinder-walls are allowed to become and remain wet, they are rapidly destroyed by corrosion. Even when the gas does not contain sulphur dioxide, liquid water spoils the working of the engine by washing away the lubricant.

When the author first began to consider the use of internal injection as a means of cooling, these difficulties of corrosion and lubrication seemed to be an insuperable bar, until it occurred to him that they could probably be overcome by the simple device of regulating the amount of water injected in such a way that the temperature of the whole of the engine is kept well above 100° C. Under such conditions (which of course are only rendered possible by the absence of all external water-cooling) every drop of injected water is boiled when it reaches the walls, and no liquid can accumulate. The large drops of water projected from the nozzles can dissolve but little gas on their way to the walls, for their surface is relatively small and they are only in contact with the gas for a fraction of a second. What little they do absorb is at once driven off when they strike the hot metal, because the water is almost instantly converted into steam. That corrosion may be completely prevented in this way has been proved by actual trials, of which further particulars are given later.

The practical application of this system of cooling has been much facilitated by a discovery made by the author soon after he began experimenting with it. It was well known from the experiments of Dr Dugald Clerk, the author, and others, that the rate of heat-flow from the gas into the metal is far more rapid at, and soon after, the moment of ignition than at any other time. It seemed likely from these experiments that for practical purposes the heat-flow into the barrel of the cylinder during the last three-fourths of the expansion stroke might be so small compared with that in the first period, that direct cooling of this portion of the cylinder could be dispensed with altogether. This anticipation has been found to be correct. It is sufficient to inject water on to the surface of the com-

bustion-chamber and the head of the piston only, the whole of the cooling of the barrel being effected by conduction into the piston which is itself kept cool by the projection of water on to the head when it is near the in-centre. This, of course, is the opposite of what occurs in a jacketed engine, in which the heat flows from the piston into the jacketed barrel. By taking advantage of this fact, the application of water is confined to places where it can do no harm, none falling on the sliding surfaces. This is a point of some importance if the water contains much dissolved matter. The experimental engine described below has been worked for some thousands of hours and is now working with a very hard water containing about 0·35 gramme of salts to the litre (25½ grains to the gallon), so that the surface of the combustion chamber and the face of the piston have become thickly encrusted with salts. Yet no trouble whatever has arisen, because no water has been allowed to fall on the sliding surfaces. The absence of pre-ignition under such conditions is also noteworthy and shows the efficiency of this method of cooling.

Trials of 50-b.h.p. Engine.

Description of Engine and Injection Apparatus. In order to put these ideas to a practical test, a Crossley engine, 11½ inches diameter by 21 inches stroke, rated at 40 b.h.p. (with coal gas) at 180 revolutions per minute, was fitted with a new cylinder consisting of a plain barrel without any water-jacket. The valve motions were retained, and the valves and the shape of the combustion-chamber were the same, the only change being the removal of all external water-cooling. It was therefore possible to make an accurate comparison between the performance of the engine with the new system of cooling, and the results of the measurements of fuel economy and the temperatures of the piston and other parts of the engine which had been made by the author on the same engine when jacketed*. The compression ratio in the engine is 6·37, giving a compression pressure of about 175 lbs. per square inch abs. This is higher than is usual, and proved, when the engine was jacketed, to be too high for ordinary practical working. The successful working of the new cylinder therefore constitutes a satisfactory proof of the freedom from pre-ignition which is characteristic of cooling by water-injection.

A section of the new cylinder with water-injection rose is shown in Fig. 1, p. 338. The injection-rose is a hollow casting, projecting into the combustion-chamber. There are about 25 holes in the rose, each 1/40 in. diameter, and the jets proceeding from these are directed as shown against all parts of the combustion-chamber and piston-head. There is no jet on to the exhaust-valve, as it has been found that the drip from the rose is sufficient to keep this cool. The water is injected by a simple plunger pump

* These tests are described in the following papers: *Proceedings Inst. Mech. E.*, 1907, Part 4, p. 863. *Ibid.* 1908, Part 2, p. 417. *Proceedings Inst. C. E.*, vol. 176, 1908 9, Part 2, p. 210. Pages 226, 263, and 291 of this volume.

of the same kind as that used for the injection of fuel in Messrs Hornsby's oil engines. It is driven by a cam on the valve-shaft, whereby a charge of water is injected once in a cycle. The pump-stroke commences about 30° before and finishes about 30° after the point of ignition, so that water only goes in at a time when practically the whole of the sliding surface of the barrel is covered by the piston.

Fuel Economy and Consumption of Water. Immediately after erection with the new cylinder, the engine was run continuously for 120 hours on an electrical load with coal gas. Continuous observation was kept of the

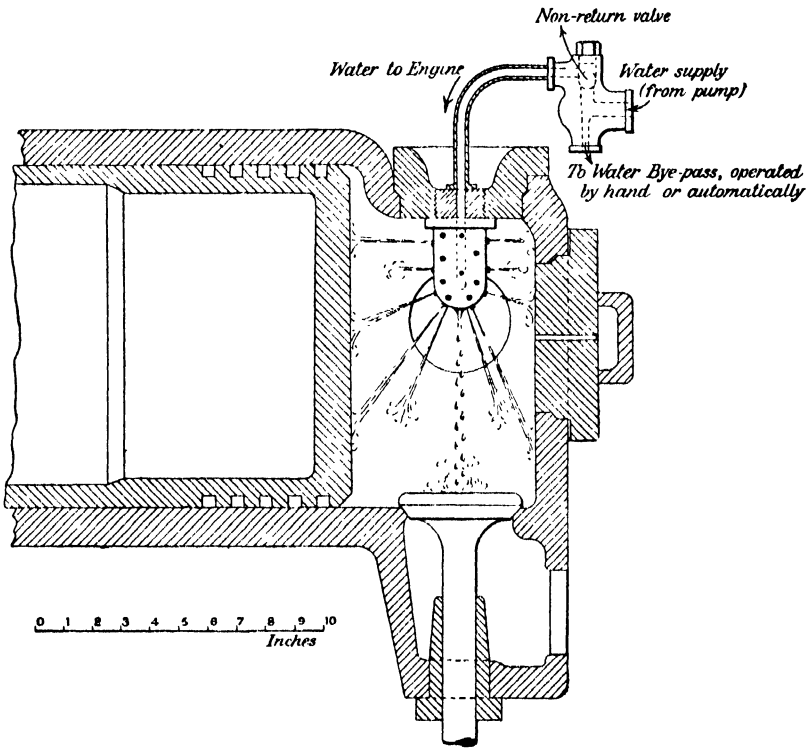


Fig. 1. Section of cylinder with water-injection rose.

gas consumption and of the load. The engine developed during this period 43 B.H.P. on the average, and ran very smoothly and steadily. The average mean effective pressure was 101 lbs. per square inch. When jacketed, the engine would not develop more than 40 B.H.P. continuously without overheating, and mixtures giving a mean pressure of more than 100 lbs. per square inch produced excessive maximum pressures (over 500 lbs. per square inch) with violent thumping explosions. The reduction in maximum pressure, under these circumstances, by water-injection is over 100 lbs. per square inch, and the effect is very marked, the explosion becoming almost inaudible. This effect of the presence of steam in the

explosive charge is of course well known, but the quantity of steam formed in an engine cooled in this manner is so large that it constitutes a substantial advantage of the method. It will be noticed that the formation of the steam does not involve any thermodynamic loss, such as occurs when water is sprayed into the cylinder in an atomized condition and evaporated before reaching the walls, since the heat used is that which would otherwise be wasted in the jacket-water.

The quantity of water used on this trial was, on the average, 102 lbs. per hour, equivalent to 2·4 lbs. per B.H.P.-hour. The temperature of the engine varied from 150° to 180° C. No water was visible on the piston or the spindles of the valves, and when the engine was stopped at the end of the trial, the inside of the combustion chamber was found to be perfectly dry. When the engine was jacketed and giving the same power for short periods the jacket-water removed about 67,000 B.TH.U. per hour, which would be sufficient to evaporate 108 lbs. of water at a temperature of 20° C. under atmospheric pressure. The agreement between the available heat and the amount of water evaporated is satisfactory, such difference as there is being accounted for partly by greater radiation loss consequent on the higher temperature of the engine, and partly by the reduction in flame temperature produced by the steam, which somewhat reduces the total amount of heat passing into the walls.

The engine consumed in this trial 15 cubic feet of Cambridge coal gas per B.H.P.-hour reckoned at atmospheric temperature and pressure. This is approximately the same as it burnt when developing the same power for short periods when jacketed. Tests at other loads have shown that with a weak mixture the gas consumption is slightly increased by the water-injection, but with very strong mixtures it is a trifle less. The difference, however, does not exceed 5 per cent. either way, and on the average it may be said that the economy is unaffected by the use of this method of cooling. Indicator diagrams taken in this long trial are shown in Fig. 2, p. 340, and a comparison of these with similar diagrams taken from the jacketed engine shows that the reduction in maximum pressure is counterbalanced by a slightly raised expansion line. The pressure is better sustained, partly by the formation of the steam and partly by the reduced loss of heat, with the result that the diagram is "fatter" and less "peaky."

Reliability and Wear under ordinary Working Conditions. After the trial just described, the engine was put to drive a dynamo in a factory engine-room. Its speed was increased from 180 to 195 revolutions per minute. It was left in the hands of the ordinary engine-room staff for several weeks; and was worked continuously for long periods of time at excessive loads. During this time it developed at times 50 B.H.P. with coal gas for several hours together—an increase of 25 per cent. on the maximum continuous load which it could safely carry when jacketed. Since then the engine has been brought to Cambridge, and is now engaged

in regular service with a suction-producer driving the workshops and producing electric current for the Engineering Laboratory. It is left to itself like an ordinary gas engine, giving no trouble at all, and has now been in regular work for two years, the total time of running being 5000 hours.

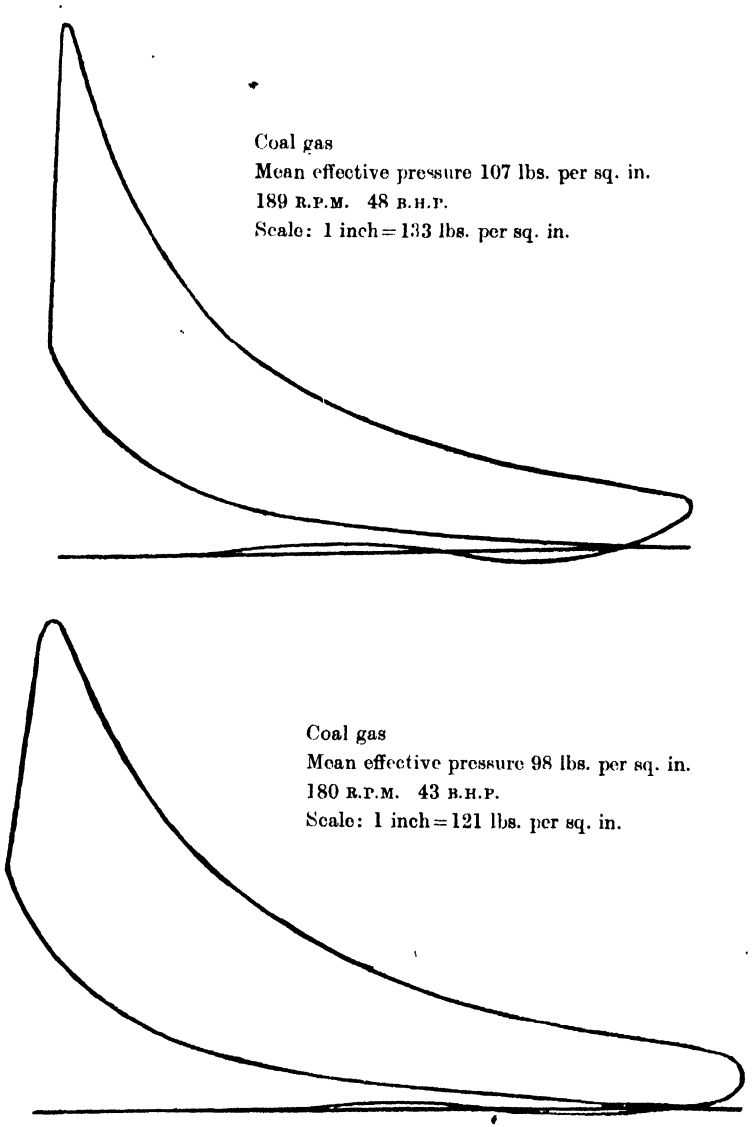


Fig. 2. Indicator diagrams from jacketless engine fitted with water-injection.

Anthracite coal is used in the producer, and this coal contains a considerable proportion of sulphur. Yet there has been no trace of corrosion in the engine. That the corrosion would be rapid, if liquid water were allowed to accumulate, is shown by the experience with the nozzles in

the injection-rose. These nozzles are, of course, continually in contact with water, and were at first found to corrode away rapidly. After many trials a suitable material has been found which lasts very well. Some corrosion has been observed at the bends of the exhaust-pipe where the gases impinge on the metal, and corrosion in the exhaust-pipe is also liable to occur at any place where water can accumulate. This, however, is not serious or rapid, and such as it is can easily be avoided by arranging the pipe suitably. Except at these points, no corrosion has been observed anywhere, and the experience of this engine has completely proved that the necessary and sufficient condition for the prevention of corrosion is that the engine should be kept so hot that all the water is boiled.

The cylinder was at first lubricated with a thick oil, such as is used with superheated steam in steam engines. A gallon of this oil, costing 3*s.* 3*d.*, lasts for 160 hours, equivalent to about one farthing per hour, or, say, 0·006*d.* per B.H.P.-hour. The lubrication was entirely satisfactory. During the past year "Super Mazoot" oil supplied by the Henry Wells Oil Co. has been used. This oil is much cheaper, but it is not quite so clean. The balance of advantage remains with it, however, and its use is being continued. Accurate measurements of the cylinder and piston have been made, with the following results:

	As delivered from makers	After 200 hours' running	After 1000 hours' running	After 4000 hours' running
	inches	inches	inches	inches
Piston, breach end	11·475	11·474	11·4735	11·472
,, crank end	11·490	11·489	11·489	11·489
Cylinder, breach end ...	11·50	11·50	11·50	11·505
,, crank end	11·502	11·502	11·502	11·502

It is quite certain that the combined wear (on cylinder and piston together) in the course of 4000 hours has nowhere exceeded one hundredth of an inch. Over the greater part of the surfaces it is much less, and in many places the tool marks are still visible.

Regulation of Water Supply. The ordinary working temperature of the cylinder is about 160° C., but the engine will run satisfactorily at any temperature between 120° C. and 200° C. In order to keep the temperature between these limits, some regulation of the water supply in accordance with the load is necessary. In the engine under consideration, which governs by hit and miss, this regulation is effected by coupling the pump to the governor so that the pump only takes a stroke when the engine takes gas. This method of regulation gives rather too much water at very low loads, but is satisfactory between the limits one-third and full-load. With a

throttle governor it is easy to connect the gas supply and the water supply in such a way that the correct amount of water is delivered at all loads.

When starting the engine cold, the adjustment just described gives too much water, and some of the water must be by-passed until the engine is warmed up. This may be done by hand, for which purpose a small screw-down by-pass valve, admitting of fine adjustment, is provided. In large engines there is no reason why hand adjustment should not be used during warming up, since there must be some one attending to the engine during this period. In smaller engines, however, it is important to make the whole thing automatic. For this purpose a simple form of thermostat has been designed, which opens the by-pass valve if the temperature falls too low. The design of a thermostat which could be relied upon proved to be difficult, but the difficulties have now been overcome, and a very simple and cheap form has been devised. It has been in use for some months, and is quite satisfactory. Once adjusted, it need never be touched and the engine can be started up in the morning day after day, and left to itself, no attention whatever being paid to the water supply. This thermostat, though especially valuable for small engines when it is desired to reduce the attendance to a minimum, will be of use also for larger sizes, since it deals automatically with changes in the quality of the gas.

Safety Plug. It is one of the advantages of this method of cooling that failure of the water supply—such as may occasionally occur owing to the pump-valve sticking—entails nothing worse than a temporary shut-down. If the water-cooling of the piston in a large gas engine is stopped for a few minutes, the engine is very likely to be wrecked by the seizing of the expanding piston in the cold cylinder. But if the engine is cooled by injection, nothing of the kind can occur because the various parts of the engine all heat up together. If the water supply be shut off, the engine heats up quite slowly, and if unattended it stops by the occurrence of pre-ignitions arising usually from the injection-rose. Even in a 36-inch cylinder it has been found that no serious harm is done by such an event.

In order to minimize the inconvenience, and to guard against any danger, arising from failure of the water supply, however, the engine is provided with a fusible plug, screwed into the wall of the combustion chamber. Should the temperature rise above about 200° C.—quite a safe working temperature—the plug melts and the noise of the escaping gases warns the attendant. This simple device has been thoroughly tested and has been found absolutely reliable. If, with the engine running at full-load, the water be shut off completely, the engine heats up quite slowly, taking perhaps 10 minutes or a quarter of an hour to reach the point at which the plug goes. There is thus ample time, before the engine becomes dangerously overheated, to reduce the load (if necessary) and to attend to any small defect such as a stuck valve or blocked pipe. A screw-down valve is provided for closing the hole made by the fusion of the plug, so that it is

not necessary to stop the engine for its replacement until a convenient time.

Trials of Large Engines. From the nature of this method of cooling it seemed almost certain that its effectiveness would be independent of the size of the engine. Each square foot of metal receives a certain amount of heat from the gas, and it is only necessary to deliver to that square foot as much water as will be evaporated by the heat which it receives. The heat received per unit area is greater in a large engine than in a small one, but it did not seem probable that this would materially affect the matter. The truth of this anticipation has been proved by applying the method to the cooling of larger engines—one an engine of 18½ inches bore giving 105 B.H.P., the other a 1000 H.P. Oechelhäuser engine of 36 inches bore. To the makers and owners of these engines—the National Gas Engine Co. and Messrs W. Beardmore and Co.—the author is much indebted for the facilities given. In each case the water was simply run out of the jackets, the injection-rose fitted, and the engine put again to its ordinary work, which was that of supplying electric power to the factory. The trial on the large Oechelhäuser engine is perhaps the more interesting. Three injection-roses were at first used, spaced equally round the combustion-chamber and mid-way between the pistons. There were 45 jets, each 1/16 in. diameter, arranged to deliver water all over the piston-heads and the surface of the combustion-chamber. The pump was driven by an eccentric on the side-shaft and was fitted with a valve which confined the delivery of water to the period 45° before and 45° after the in-centre, the rest of the pump-stroke being by-passed. There were two thermocouples in each piston-head and a number of thermometers in different parts of the barrel; and the temperatures were controlled by adjusting, by means of throttle-valves, the flow of water to the different roses. With no water in the jackets or pistons, the temperature of every part of the engine when on full-load could be kept between 100° and 200° C. The engine was taking full-load within a few hours of fitting the apparatus, and ran for 30 hours without a stop. After stopping for a short time for adjustments, it ran continuously for 70 hours under the same conditions, taking the ordinary factory working-load, which would fluctuate about an average of 800 B.H.P. Failure of the water supply, which occurred once or twice in consequence of the temporary nature of the pump gear, did no harm beyond causing some pre-ignitions from the roses and necessitating a reduction of load for a short time. In fact, the engine heats up so slowly and uniformly that there is ample time to deal with such a failure before anything serious happens. The quantity of water used was about 2·4 lbs. per B.H.P.-hour, and it is interesting to note that this quantity seems almost independent of the size of the engine. This is in accordance with the recent developments of gas engine theory, according to which the heat loss from flame, being largely due to radiation, increases greatly with the depth of the

flame, so that the heat-flow per square foot into the metal of a large engine is bigger than in a small engine, though the flame temperature may be the same.

The trials of this large engine, which continued for a considerable time, proved beyond any question that the largest cylinders now built can be cooled entirely by water-injection, if applied in accordance with the principles here enunciated. They also showed, however, as might be expected, that for ordinary commercial use the cylinder must be properly designed with a view to the employment of this method of cooling. The most obvious point is that the cylinder must be a plain barrel without any jacket. In the Oechelhäuser engine the jacket could only be removed, and the liner exposed, just round the combustion space. The rest of the barrel was surrounded by the jacket which not only made access difficult for the measurement of temperature, etc., but also (the water space being, of course, filled with air) formed a most efficient heat insulator, thus greatly complicating the problem of cooling. Some trouble was experienced because of this, in controlling properly the temperature of the exhaust ports and of the cylinder near them. Further, a good deal of hand regulation of the water was required, as the means of automatic regulation which have since been perfected were not available at that time. In fact, the experiment of running a 1000 H.P. jacketed cylinder without any water in the jackets was rather in the nature of a *tour de force*, and did not prove to be the best way of developing the idea commercially and in detail. It was, however, an interesting and striking experiment, which showed in a most convincing way the great capacity of the method of internal injection. The whole injection apparatus was made and put together in Cambridge; it cost about £20, and within a few hours of fitting it on the engine it was doing all the work of the complicated and costly plant—cooling tower, centrifugal pumps, 8-inch water mains, and the like—which is necessary for the cooling service of an engine of this size when jacketed. The author takes this opportunity of expressing his thanks to the engineers of Messrs Beardmore and Co., and particularly to Mr Stokes, the head of their gas engine department, for their unfailing courtesy and kindness to him during these trials, which occurred at a time of great pressure of work in the factory, and must have added materially to the burdens of the power-station staff.

The difficulties incident to carrying out experiments on an engine which is in regular use for power supply and cannot be shut down when required for adjustment and alteration, determined the author, with Messrs Beardmore's consent, to abandon for the time the experiments in Glasgow, and to build an entirely new engine designed *ab initio* with a view to the use of water-injection; and such as might, with but small modification, be put on the market. Arrangements with this object were made with Messrs Davey, Paxman and Co. The new engine, which is

completed and is now undergoing trials, embodies all the experience gained in the experiments which have been described. It is a 2-cycle, single-acting engine, 18 inches diameter by 24 inches stroke, with separate gas- and air-pumps, and is cooled entirely by water-injection.

The author wishes to acknowledge the valuable services of his assistant, Mr A. L. Bird, in connection with the experiments referred to in this Paper. In addition to supervising the trials of engines, Mr Bird has done most of the work of detailed design, and several novel features in the new engine are largely due to him.

22

THE CHARGING OF TWO-CYCLE INTERNAL COMBUSTION ENGINES.

["Trans. North-East Coast Institution of Engineers and Shipbuilders," Vol. XXX. Read in Newcastle at the Summer Meeting, July, 1914.]

The performance of 2-cycle internal combustion engines is determined very largely by the efficiency or otherwise of the process of charging. In the course of less than $\frac{1}{4}$ of a revolution the products of combustion resulting from the previous explosion have to be replaced as far as possible by the fresh charge of air or of gas and air, which is blown in through the inlet-valves and drives before it through the open exhaust-ports the exhaust gases. Inevitably some mixing occurs and some of the fresh charge or of the scavenging air passes away to the exhaust and is wasted. On the amount of this waste depends very largely the performance of the engine. Its economy suffers to the extent of the waste of fuel. Its weight and cost are affected by the size of the charging pumps, which must be big enough to deliver the whole charge of gas and air, including that which is wasted. The imperfection of the charging process is probably in large measure responsible for the fact that the 2-cycle engine, in spite of the obvious advantage of double the number of working strokes, has not hitherto competed successfully in small sizes with the 4-cycle type, and even in large sizes has achieved only moderate success.

In considering recently the designs of certain 2-cycle gas engines, the author was struck by the almost complete absence of data of general application on which to base, in the case of a new design, a calculation of the size of pumps required to give a certain mean pressure and a prediction of the economy which may be expected. Little or nothing has been published as to the amount of the loss to exhaust which occurs in existing engines, and there is no theory to guide designers in the use of such data as exist on this point or may become available. In the present paper an attempt is made to provide a working theory of this kind, some experimental confirmation is given of it, and methods are suggested for getting more information.

In the course of the charging process, for each unit of volume of air or mixture which enters at the inlet an equal volume of the gas near to the exhaust-ports will be driven out through those ports, which gas will consist partly of burnt products and partly of air which has mixed with the products. It is here assumed that the pressure in the cylinder during

the admission period is that of the atmosphere; the effect of the disturbance of this pressure by throttling at the exhaust-ports, and by the inertia of the gases in the exhaust-pipe will be considered later. Denote by z the proportion of air by volume contained in the exhaust gases at a point close to the ports at any stage of the charging. For this purpose z is not the volumetric proportion determined by an ordinary analysis; it is the volume, reckoned at atmospheric pressure and at the temperature of the air as it comes in, contained in a cubic foot; the balance $1 - z$ consists of burnt products from the previous explosion, whose volume is for this purpose reckoned at the temperature of those products before charging began. Obviously z will increase from zero as charging goes on and will approach, but never reach, the value unity; at first, when very little air has been put in the exhaust gases contain no air, when a large quantity has been blown through, the exhaust is almost pure air. The total amount of air lost in charging will be equal to the volume delivered (denoted by y) multiplied by the average value of z, and is readily calculable if this quantity is known at every stage. The amount of air retained in the cylinder is equal to y minus the loss; this will be denoted by x.

Two cases admit of very simple treatment. The first is the ideal case of perfect stratification. The air simply drives the burnt gases before it without mixing with them at all, and there is no loss until the amount of air exceeds the cylinder volume. It is quite certain that this condition is never even approached in practice. Probably the first 2-cycle engine ever built, that made by Dr Dugald Clerk in 1880, came nearer to attaining this ideal condition than any which has been made since. Dr Clerk, who thoroughly appreciated the necessity of getting as much stratification as possible, used a very long conical combustion chamber. Even so, there must have been a good deal of mixing, and in modern high compression engines (the compression ratio in the Clerk engine was 3) such a construction is impossible.

The second case represents much more nearly what actually happens. Suppose that instead of complete stratification there is no stratification at all—suppose the mixing so complete that the cylinder contents are at every instant of uniform composition throughout. Here again it is possible to calculate the proportion lost up to any stage with complete certainty and accuracy. The quantity z now represents not only the proportion of air in the gas which is going into the exhaust at any stage, but also the proportion then present in the cylinder as a whole. It is convenient in these calculations to take as the unit of volume the whole volume of the cylinder, and x the volume of air which has been retained in the cylinder then represents also the proportion of air in the whole cylinder contents, so that in the case now under consideration $z = x$. In Fig. 1, p. 348, x is plotted against y, the amount (reckoned in cylinder volumes) of air which has been injected. When an amount ON has been injected the air present in the

cylinder is PN, the remainder PM being burnt products. The effect of adding the further dose of air NN' is to expel at the exhaust the quantity of air $\frac{PN}{MN} \times NN'$. The balance remains in the cylinder, increasing the quantity of air there by $P'Q = \frac{PM}{MN} \times NN'$. So the curve can be constructed step by step. It is the well-known exponential curve whose slope at any point is equal to the ordinate PM. The relation between x and y (of course easily obtained mathematically) is $x = 1 - e^{-y}$*.

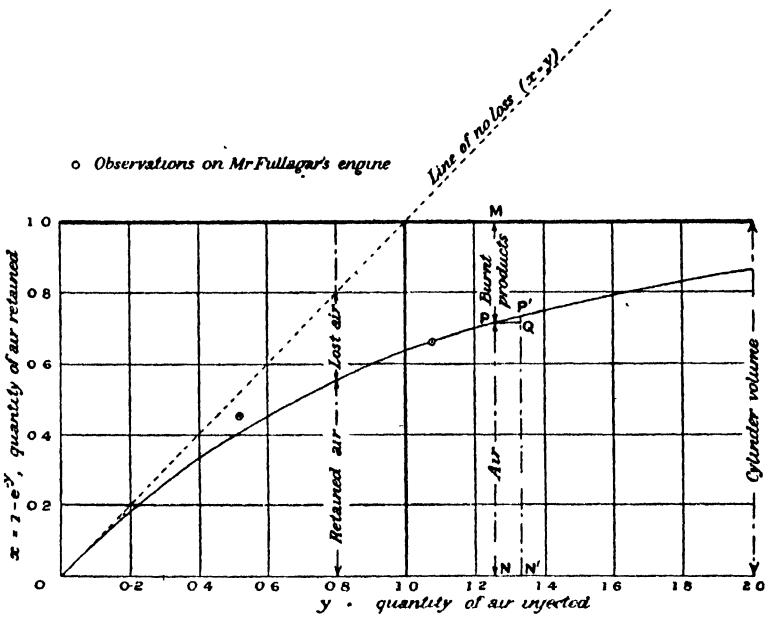


Fig. 1.

In many engines now in use the mixing is probably fairly complete, and the simple formula just given represents fairly accurately what occurs. Such are short-stroke Diesel engines with valves in the cylinder cover, and probably also the double-ported engines having the inlet-ports opposite the exhaust-ports and a deflector plate on the piston. In engines having relatively longer cylinders, such as the Oechelhäuser, there is some stratification of the cylinder contents, but even here the effects of stratification are probably not very great. They are of the nature of a correction, and before discussing this correction along with others, the necessity of which will appear later, it will be convenient to compare the simple formula with results actually obtained from an engine in the course of some recent trials conducted by the author for the Fullagar Engine, Ltd., and then to give some illustrations of its application to other cases.

* See Appendix I.

Tests on Mr Fullagar's Engine.

The engine in question was built to the designs of Mr H. F. Fullagar. It represents an important development in large power gas engines, and has several features of great interest but, as Mr Fullagar will be describing it himself, it is only necessary to give here such details as bear on the subject of this paper. It has four vertical cylinders of the Oechelhäuser type, coupled together in pairs, the upper piston of one being coupled by diagonal rods to the lower piston of its neighbour, in the manner invented by Mr Fullagar. The leading dimensions of each cylinder are given in Fig. 2. The air-ports (there is only one set of admission ports) communicate with a large receiver, to which air is delivered by an electrically-driven fan. Coal gas was used in these trials, and was admitted by a piston-valve at the centre of the cylinder. The valve was opened 5° of crank-angle before the exhaust-ports closed, and remained open for 36°

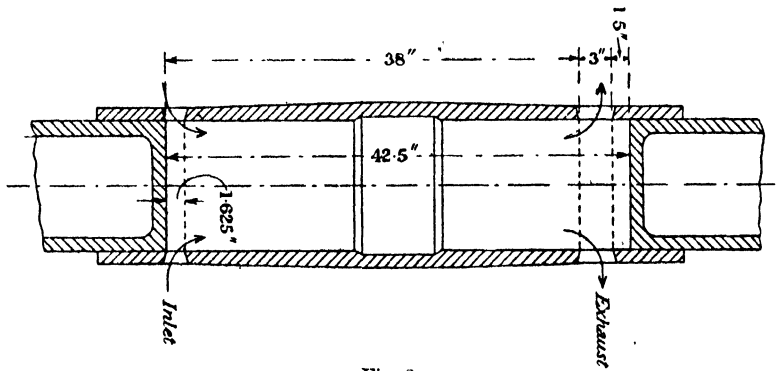


Fig. 2.

of crank-angle. The coal-gas was delivered at high pressure by a reciprocating pump.

The verification of the formula above given required the accurate measurement (1) of the total quantity of air delivered to the engine per minute, and (2) of the proportion of that air retained in the cylinder.

(1) *Quantity of Air.* A diaphragm with a circular hole was inserted in the air-delivery pipe, between the fan and the engine, and the drop of pressure was measured by means of a water-gauge. The diameter of the air-pipe was 18 inches, that of the hole in the diaphragm 9 inches. The current of air through the diaphragm was nearly uniform, the pulsations caused by the engine being nearly damped out by the large air receiver. The velocity of the air through the hole was calculated according to the usual formula, assuming 0·62 as the coefficient of discharge. The coal gas was also metered, and the quantity of coal gas used, together with an analysis of the exhaust gases gave an independent measure of the amount of air used. The quantity of air delivered to the engine was probably determined correct within 3 per cent.

(2) *Proportion of Air Retained.* For determining this samples were taken of the contents of all four cylinders, and analysed. The average proportion of carbon determined in the samples was compared with the proportion in a sample of the exhaust gas taken simultaneously. The ratio of the two carbon contents is equal to the ratio of the total air delivered to the air retained in the cylinder, $\frac{y}{x}$ in our notation.

For the purpose of collecting the samples of the cylinder contents, a pipe of very small bore was screwed into a hole drilled in the sparking plug. The other end of the hole was closed with a nipple having a small hole in it. The pipe communicated with the sampling vessel into which it delivered the gas slowly, a small quantity coming in at each explosion. The result of this procedure is that the sample consists partly of a mixture of unburnt gas and air blown through the pipe during the compression stroke, and partly of burnt gases which come through during the expansion stroke. Usually the proportion of burnt to unburnt was about 2 to 1. As the coal gas admission valve is opposite the sampling hole, both being at the middle of the length of the cylinder, the tendency of the method is to collect samples which are a little too rich in coal gas. But it is believed that the error so caused is not large. The greater part of the sample comes through while the pistons are near the in-centre, by which time any stratification of the coal gas will in large measure have disappeared, and will moreover be of less importance because of the compression of the cylinder contents.

Samples were taken from all four cylinders simultaneously and after absorbing the CO_2 the combustion of the residue was completed over palladium, and the further yield of CO_2 obtained. The total CO_2 gave the proportion of coal gas to air in the cylinder contents. A simultaneous analysis of the exhaust gases gave the proportion of coal gas to air delivered to the engine. A check was obtained by estimating the oxygen in the exhaust and in the cylinder contents. The analyses were made for the Fullagar Engine, Ltd., by Dr Mechie and Mr Hill of the Wallsend Laboratories. Great care was taken by these gentlemen to eliminate all errors in the estimations and the author believes that, thanks to their labour, the figures now given are of an order of accuracy rarely attained in a commercial trial.

The following is a summary of the results obtained in two trials, in one of which the volume of air delivered to each cylinder per stroke was about equal to the cylinder volume, in the other about half. In the first case the engine had to be run slow as the fan would not at the normal speed deliver a sufficient volume of air to give a cylinder-full per revolution*.

* For details of analyses see Appendix II.

Trial	I *a*	I *b*	II
Speed (revolutions per minute)	200	200	250
Air per cylinder per revolution (by diaphragm) cubic feet	2·70	2·70	1·42
Air per cylinder per revolution (by gas-meter and exhaust analysis) cubic feet ...	2·86	2·86	1·38
Coal gas per cylinder per revolution (by meter) cubic feet	0·189	0·182	0·163
$\frac{\text{Air}}{\text{Coal gas}}$ delivered to engine (exhaust analysis)	15·1	15·7	8·45
$\frac{\text{Air}}{\text{Coal gas}}$ in cylinder contents (analysis, mean of 4 cylinders)	9·4	9·45	7·35
Air retained, per cylinder per revolution, cubic feet	1·78	1·72	1·20

The volume of the cylinder when the pistons are at the out-centre is 2·75 cubic feet. When the inlet-ports are just closed it is 2·50 cubic feet. Having regard to the dwell of the pistons near the out-centre the mean volume during the period of injection will be nearer to the former figure; take it as 2·65 cubic feet. Dividing this volume into the volumes of air given on lines (3) and (7) of the above table we get the values of y and x. They are tabulated in the following table, together with the corresponding values of x calculated from the formula.

	I *a*	I *b*	II
y = air delivered per cubic foot of cylinder volume	1·08	1·08	0·52
x = air retained per cubic foot of cylinder volume	0·67	0·65	0·45
Calculated value of x ($= 1 - e^{y}$)	0·66	0·66	0·405
$y - x$ (measured)	0·41	0·43	0·07
$y - x$ (calculated)	0·42	0·42	0·115
Percentage of air lost to exhaust (measured) ...	38	40	13·5
„ „ „ (calculated) ...	39	39	22

It will be seen that there is rough agreement between the calculated and measured figures sufficient at any rate to justify the use of the simple supposition of complete mixing as a first approximation to what occurs. On the other hand the deviation with the smaller amount of air is too great to be ascribed to errors of observation. The observed loss of air is only about $\frac{2}{3}$ of the calculated loss. This shows that there are disturbing factors which must be taken into account as corrections to the simple theory. In trials I *a* and I *b* these connections apparently neutralize each other, producing as it happens almost exact agreement. Before dealing with those corrections, it will be convenient to give one or two simple illustrations of the application of the theory to practical cases.

(*a*) Diesel engine, stroke equal to bore, or slightly greater. Inlet-valves in cylinder cover. This is a case in which the mixing will be nearly complete. Suppose that the volume of the air pump is $1\frac{1}{4}$ times the total cylinder volume (including clearance space). The volume of air delivered, reckoned at the temperature of delivery, will be approximately that of the pump, the rise of temperature due to the pumping work compensating for the volumetric inefficiency of the pump. Hence $y = 1{\cdot}25$ and the

volume of air retained (PN in Fig. 1) is $x = 1 - e^{-1\cdot25} = 0\cdot71$. The volume wasted ($y - x$) is 0·54 or 43 per cent. of the amount pumped. The cylinder contents consist of a mixture of 0·71 air at 40° to 50° C. (temperature in air delivery pipe) and 0·29 of burnt products at perhaps 500° C. The scavenging is far from complete and the charge would be rather hot and the flame temperature correspondingly high, necessitating a low mean pressure in order to avoid over-heating. It may be added that no substantial improvement can be effected in this respect except by the use of excessive quantities of air. For instance, it will be seen from the diagram that to reduce the amount of burnt products left in the cylinder to 15 per cent. the air pump must be nearly twice as big as the cylinder.

(*b*) Gas engine, with separate gas- and air-pumps, using producer gas. In such an engine the pumps and admission valves or ports would be arranged so that as far as possible the air goes in first, the gas following it. Thus in the Oechelhäuser the air-ports open first, and a large part of the air is discharged through them before the gas-ports open. Then the gas comes in, mixing with the remainder of the air. The amount of air coming in with the gas in any case can be calculated roughly beforehand from the size of receivers and the size and position of gas- and air-ports, but such calculations are apt to be much upset by the exhaust back-pressure whose efforts are difficult to allow for and will vary in different cases according to the length of the exhaust-pipe. For purposes of illustration we will assume that at full-load the volume of air passing the air-ports before the gas-ports open is half the cylinder volume; and that this is followed by $\frac{1}{4}$ cylinder volume of gas and the same quantity of air, and that no air enters after the gas-ports are closed*.

The volume of mixed gas and air is half the cylinder volume. Hence $y = \frac{1}{2}$, and the amount of mixture retained in the cylinder per cubic foot of its volume is $x = 1 - e^{-0\cdot5} = 0\cdot394$. The proportion of the pumped gas which is retained and burnt is $\frac{x}{y} = 0\cdot79$, and the balance (21 per cent.) is wasted to exhaust. Assuming the engine to have a compression ratio of 6·5 (ratio of cylinder volumes at out- and in-centre) it would give an indicated efficiency on the basis of the gas actually burnt of 0·37 or 0·38, say 0·375. The quantity of gas retained and burnt per cubic foot of stroke volume is $0\cdot79 \times 0\cdot25 \times \frac{6\cdot5}{5\cdot5} = 0\cdot233$ cubic feet. If the gas has a lower calorific value of 140 B.TH.U. per cubic foot at atmospheric pressure and

* With a perfectly free exhaust, this distribution of air and gas is possible in the ordinary design of the Oechelhäuser engine, but the author is of opinion from data in his possession that in ordinary practice the distribution is not so good. Cards taken from the receivers in one case which is probably typical of many, show that the exhaust-pressure had not fallen to atmosphere when the air-ports opened, so that the air was held up, and more of it went in along with the gas than is here assumed.

at the temperature in the receiver the indicated mean pressure will be $0{\cdot}375 \times \frac{0{\cdot}233 \times 140 \times 778}{144} = 66$ lbs. per square inch, which is a little above the normal figure for an engine of this type. Allowing a mechanical efficiency of 83 per cent. the practical efficiency (ratio of brake power to the lower heat-value of the whole of the gas pumped) will be

$$0{\cdot}83 \times 0{\cdot}79 \times 0{\cdot}375 = 0{\cdot}246,$$

equivalent to about 10,400 B.TH.U. per B.H.P.-hour.

In practice, two-cycle gas engines sometimes do a little better than this, though at the high mean pressure of 66 lbs. per square inch it is doubtful whether any of them could be relied on to produce a B.H.P.-hour for much less than 10,000 B.TH.U.* The Oechelhäuser engine with its long cylinder gets the benefit of some stratification. In the Körting the admission valve is timed so that it closes after the exhaust-ports and some of the charge is thus trapped without any possibility of getting out. On the other hand, in both cases these favourable factors are apt to be neutralised by the exhaust-pressure holding up the scavenging air. But broadly speaking, and subject to corrections of this nature which, on the whole, are small, the simple theory leads to conclusions which are in accord with practical results. It may be noted in this connection that the Fullagar engine in which there was no waste of gas at all showed an indicated efficiency of 0·376 and a brake efficiency of 0·30; and the latter figure would reach 0·31 if the mechanical efficiency were raised to the normal figure for these engines by the use of a more economical method of serving the engine with air.

Effects of Stratification.

In the simple theory of the charging process which has been discussed, it is assumed that at every instant during the period of admission, the cylinder contents are homogeneous in composition and that their pressure is that of the atmosphere. In fact, there is in all engines some stratification of the cylinder contents, the portions near the exhaust-ports being poorer in air and richer in burnt products than the average. There is also in all cases some throttling in the exhaust-ports and exhaust-pipe, and inertia effects in the exhaust-pipe, in consequence of which the pressure in the cylinder varies during the admission period. The effects of these two factors will now be considered, beginning with stratification. Suppose that y represents as before the volume of air which has been delivered through the inlet-ports at any stage, and x the volume which has been retained in the cylinder, $y - x$ having been wasted through the exhaust-ports. These volumes are reckoned in terms of the total cylinder volume taken as unity, and x is therefore the average proportion of air (by volume) in the cylinder contents. These contents are now, however, not of uniform composition

* That is, with a weak gas. Much better results can be obtained with coke-oven gas, which, by reason of its small bulk, does not waste to exhaust so readily.

but near the inlet-ports they are richer in air than the average and near the exhaust-ports poorer. Thus a cubic inch of air delivered through the inlet now displaces and drives through the exhaust a cubic inch of gas containing a proportion z of air, where z is less than x. The full discussion of the relation between z and x, that is of the amount of stratification, is beyond the scope of this paper; it will suffice to state here that there are good *a priori* grounds for the supposition that in practical cases it is given by an expression of the form:

$$z = x\{1 - \lambda(1 - x)\}$$

where λ is a constant depending on the engine (proportions of cylinder, size and shape of ports, etc.). It will be seen that this formula satisfies two necessary conditions, the one that at the beginning of injection the exhaust shall contain no air ($z = 0$ when $x = 0$), the other that when a large quantity of air has been blown in, so that the burnt products have been nearly all cleared out, the exhaust shall be practically pure air ($z = 1$ when $x = 1$).

The constant λ which is characteristic of the engine as regards stratification I propose to call the "stratification constant." The greater its value the greater is the amount of stratification; and when $\lambda = 0$ there is no stratification, for then $z = x$, that is the proportion of air near the exhaust-ports is equal to the average proportion throughout the cylinder.

Once the proportion of air near the exhaust-ports (z) is known in terms of the average composition (x), the relation between the quantity of air retained (also x) and the total quantity injected (y) can be obtained with mathematical certainty. For the relation between z and x given above, it takes the form:

$$x = \frac{1 - e^{-(1+\lambda)y}}{1 + \lambda e^{-(1+\lambda)y}}.$$

This formula applies to such amounts of stratification as are likely to occur in practice, for which λ will not usually exceed 1. For larger values of λ the expression for z in terms of x obviously ceases to be accurate, since it would take a negative value unless x is nearly equal to 1. The more accurate relation between x and z, to which the simple equation given above is an approximation, leads to a somewhat more complicated formula for x in terms of y, which it is unnecessary to give here as it is not required in any practical case*.

In Fig. 3 are plotted the curves of loss $(y - z)$ calculated from this formula for various values of λ. If this constant is known for the type of engine under consideration the corresponding curve gives (subject to any correction for the effects of exhaust-back-pressure) the amount of air wasted out of any given quantity injected. It is to be noted that in most cases, λ is not much greater than unity, and a comparatively rough guess

* See Appendix I for details of the mathematical analysis.

at its value, based on experience with similar engines will give results good enough for practical purposes.

The points determined from the tests on Mr Fullagar's engine are noted on the same figure, from which it will be seen that the lower point falls between the curves corresponding to $\lambda = 0{\cdot}5$ and $\lambda = 1$, while the upper as already indicated falls near the curve $\lambda = 0$.

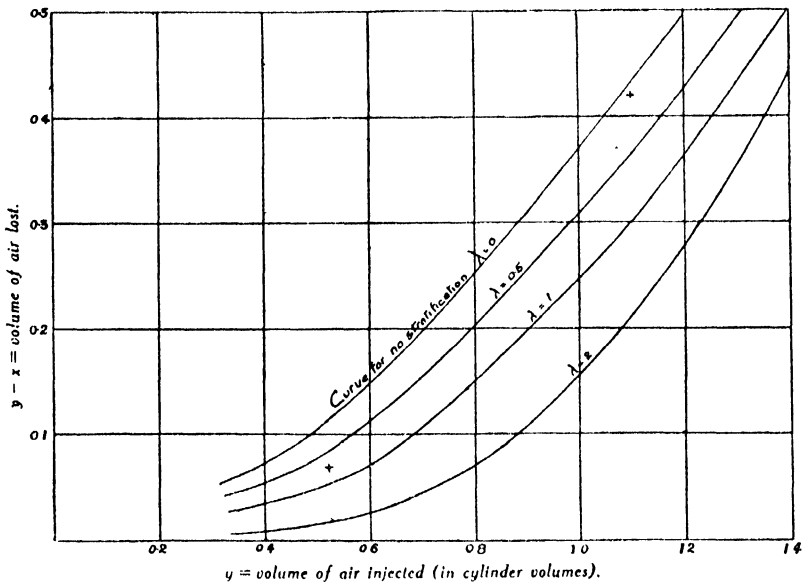


Fig. 3.

Experiments with a Water Model.

The phenomenon of stratification can be studied very conveniently by means of a model of the engine cylinder in which water is used instead of air. The model can be made to scale, of sheet iron, and is provided with an admission valve at the lower end which can be opened and closed by hand. Through the valve the cylinder communicates with a large chamber containing water with an air-space above whose volume is equal to or greater than that of the water. Air is pumped into the chamber until the pressure is 10 or 15 lbs. per square inch. The exhaust-ports at the upper end of the cylinder are permanently open and discharge into a channel whence a shoot leads to a tank. The cylinder is filled up to the edge of the exhaust-ports with brine containing a known proportion of salt; this corresponds to burnt products. In making an experiment the valve is rapidly opened and closed by means of a handle and crank, this operation taking in an ordinary case about 1 second. The water from the chamber (which corresponds to the air in the engine) rushes through the inlet-valve and displaces the brine. Samples of the contents of the cylinder and of

the exhaust-tank are then analysed for their content of salt and from the analysis the quantity of pure water which has passed out through the exhaust along with the brine can be estimated. The experiment corresponds, in fact, with that already described as having been made on Mr Fullagar's engine, in which simultaneous analyses were made of the cylinder contents and of the exhaust-gases.

The use of water models for ascertaining the behaviour of air is familiar to those who are concerned with aeronautics. The resistance of an airship for instance can be ascertained by towing a model in a water-tank. Theory, which has been confirmed by experiments of many different kinds, shows

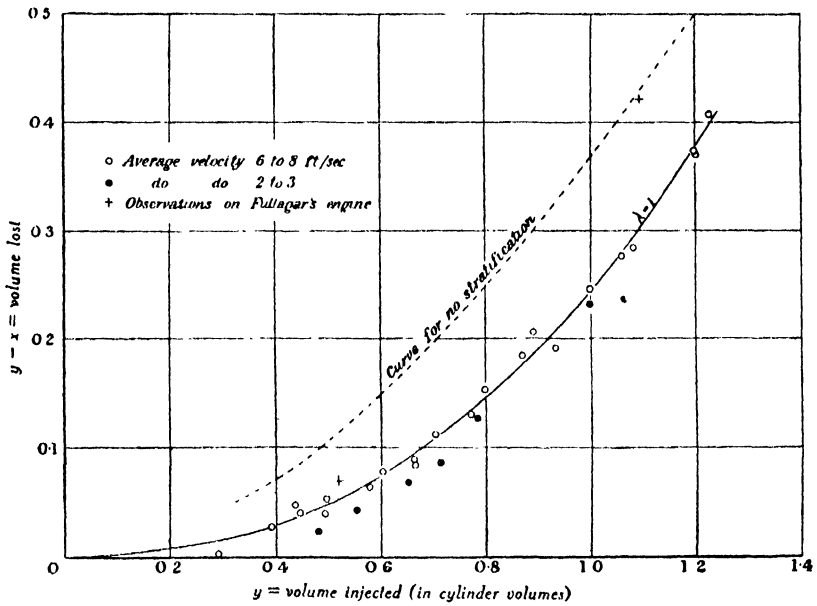


Fig. 4.

that the motions of the two fluids will be dynamically similar if the velocities and the linear dimensions are so related in the two cases that $\frac{VL}{\nu}$ is the same for both, ν being the viscosity. For air the value of ν is about 13 times as great as it is for water. Hence if the dimensions of the model and ship are the same, the motions will be similar if the model is towed 1/13 of the speed of the ship.

In the case of the engine cylinder the matter is more complicated because the hot burnt products which the cylinder at first contains have greater viscosity and less density than the air which displaces them. Moreover, owing to the varying back-pressure in the exhaust, changes of density occur in the cylinder contents which can have no counterpart in the water model. But subject to these points there cannot be any question that the

law of dynamical similarity will apply equally to this case. That is to say, if producer gas is injected into a cylinder containing cool air (as happens if there has been a previous scavenging charge) and possessing a completely free exhaust, the loss will be precisely the same as the loss of water in the water model when the same quantity of water is injected at 1/13 of the velocity. The period of injection of the water will be 13 times as great as that of the gas; thus the whole operation can be done slowly by simple mechanism.

Fig. 4 shows the results of a large number of experiments made on a full-size model of Mr Fullagar's engine. The curve is that corresponding to the formula given above for the case where $\lambda = 1$. It will be observed that within errors of observation the observed points agree with this curve. This is a satisfactory confirmation of the theory on which the

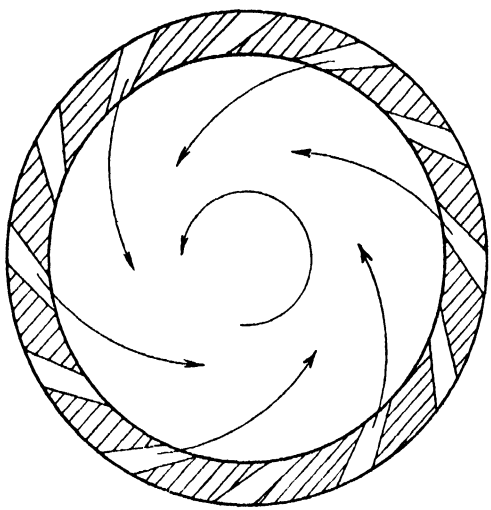


Fig. 5.

formula is based. It will also be noticed that the velocity of injection varies widely in different experiments but that there is apparently no considerable difference between high and low velocities as regards the loss to exhaust. This fact is of some importance because it follows from it that similar variations in the viscosity of the fluid would have a like small effect.

On the same figure are marked the two points showing the loss of air actually occurring in Mr Fullagar's engine. It will be seen that in both cases the loss in the engine is greater than in the model, though the difference is not very great in the case of the smaller quantity of air. The differences must be due to one or both of the two factors which have been mentioned as not represented on the model; that is the difference in viscosity and density between the air and the burnt products which it displaces, and the varying pressure in the cylinder. The effect of viscosity is certainly small, that of density may be considerable. But in the author's

view it is probable that the important cause of the difference is the varying pressure, which is considered in greater detail in the next section.

It is obvious that the use of water models cannot be relied on as a means of quantitative investigation until further comparisons have been made between engines and their models, and even then the disturbing factor of exhaust pressure complicates the interpretation of the results. But the author believes that it promises to be a useful way of comparing, with little expense and trouble, the properties of different forms of cylinder and of ports and valves as regards stratification. For instance it would appear from experiments of this kind which the author has made that a cylinder like the Oechelhäuser whose length is perhaps 3 times its diameter is much better from this point of view than a cylinder of one-third that length such as a short-stroke engine with admission valves in the cylinder cover. In the shorter cylinder none of the many devices that the author has tried with the view of directing the motion of the in-coming air have availed to produce any stratification. But in the larger cylinder it would appear that such an arrangement as is shown in Fig. 5 whereby a whirling motion is given to the air, has the effect of substantially reducing the loss.

Effect of Varying Pressure in the Cylinder.

Both the simple theory and the discussion of the correction of that theory for stratification rest on the assumption that the pressure in the cylinder is atmospheric during the admission period. As a matter of fact, in any actual case this pressure will vary, the variations being determined partly by the throttling effect of the exhaust-ports, but mainly by the inertia of the column of gas in the exhaust-pipe. The surging of this column of gas causes the pressure in the exhaust-pipe near the engine to rise and fall in a periodic manner, the period depending on the length of the pipe. A full discussion of the amount and nature of these variations of pressure is outside the scope of this paper, and all that will be attempted is a short consideration of their effects, in one or two simple cases, on the charging of a two-cycle engine.

It seems probable that those pressure variations which are likely to occur in practice will cause increased loss to exhaust. For instance, it may happen that the exhaust-pipe pressure is rising when the admission ports or valves are first opened, with the result that at first the injection of a cubic inch of air will not be accompanied by the discharge of an equal volume of mixed air and burnt products, but a less volume will be discharged. When at a later stage the exhaust-pipe pressure falls to atmosphere or (it may be) below atmosphere, the excess volume which had been held up in the cylinder will be discharged. The proportion of air in the gas which then goes out corresponds to the whole or nearly the whole amount which has been injected and will be greater than it would have been had the exhaust come out as fast as the air went in. To take an extreme case,

suppose that half of a cylinder volume of air is put in without anything going out of the exhaust-ports at all. The cylinder contents will then consist of 1 volume of burnt products more or less thoroughly mixed with half a volume of air, the pressure of the whole being $1\frac{1}{2}$ atmospheres. Now suppose that the exhaust-pipe pressure falls to 1 atmosphere. The accumulated cylinder contents, now well mixed up, will at once escape. The volume of gas passing out will be half the cylinder volume, and it will consist of air and burnt products in the proportion of 1 to 2. Thus the quantity of injected air which is lost will be $\frac{1}{3} \times 0{\cdot}5$ or $0{\cdot}17$ cylinder volume. Reference to the curve Fig. 1 will show that if the pressure had been uniform the quantity lost (in the absence of stratification) would be $0{\cdot}105$. This, of course, is an extreme case, which could not arise in practice, but it

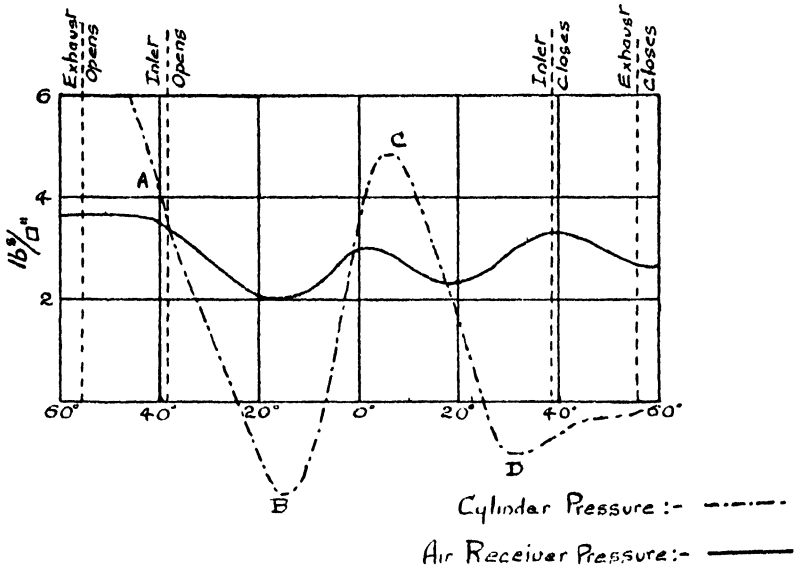


Fig. 6.

shows the tendency of a high exhaust pressure at the beginning of admission to increase the loss of air.

The actual pressure variations in the cylinder of the Fullagar engine at a speed of 250 r.p.m. are shown in Fig. 6. It will be seen that here the surging in the exhaust-pipe is of such a character as to produce a drop of pressure in the cylinder during the first part of the admission period (*A* to *B*). During this period the gas is going out quicker than it comes in, and the exhaust consists mainly of burnt products mixed with a little air. Then at *B* the pressure begins to rise, and the air accumulates in the cylinder. At *C* the pressure falls rapidly, and the accumulated cylinder contents, which have had plenty of time to mix, are rapidly discharged, thus reproducing at this stage the conditions described in the last paragraph. Moreover, at *C* the cylinder pressure is higher than the air pressure,

so that some of the cylinder contents (which still contain a large proportion of burnt products) will back into the receiver; some of the contents of the exhaust-pipe must also at this point come back into the cylinder. A quantitative estimate of the effect of the variations on the loss of air could be made by the use of the principles enunciated earlier in this paper, but would be a long and complicated piece of work. It will be sufficient to observe that the general result must be to increase the loss. The holding up of the air and the backward and forward flow, in the middle of the admission period, will mix up the air and products of combustion, and will destroy the stratification which would otherwise exist. The author believes this cause to be sufficient to account for the difference between the results obtained with the water model and with the engine.

Though the theory of the charging process which has been suggested in this paper cannot be regarded as completely proved as regards details, the author has ventured to bring it to the notice of those interested in 2-cycle engines in the hope of eliciting from them more facts about the loss to exhaust. It is to be regarded as a working hypothesis which may prove useful in the interpretation of data, but which will probably require modification in its details as more facts are accumulated. This remark applies more particularly to the corrections for stratification and for varying exhaust-pressure. The simple theory based on the assumption of complete mixing and uniform cylinder pressure is in the author's opinion a very fair first approximation, which will give results of sufficient accuracy in most practical cases, and as such he hopes that it may prove of use to designers.

The author wishes to express his gratitude to the Fullagar Engine Company, and to Mr Fullagar for permission to make use of the results obtained in the trial of their engine, and for the facilities which they provided in the course of that trial for making measurements, the commercial value of which was not immediately obvious. He is also much indebted to his assistant, Mr Bird, for his care and zeal in carrying out many experiments and designing apparatus used in connection with this paper.

APPENDIX I.

(1) *Complete mixing.* Let y be the quantity of air which has been injected at any stage, x the quantity which has been retained in the cylinder. These quantities are reckoned in cylinder volumes at atmospheric pressure, and at the temperature which the air would have after passing the ports if it took in no heat from the products of combustion. This temperature is the same as that in the air receiver, for the expansion is unresisted. Assume that the pressure in the cylinder is atmospheric. The quantity of products of combustion present in the cylinder is then $1 - x$ cylinder volume reckoned at atmospheric pressure, and at the temperature which the gases had after release and before the admission of

any air. This statement is obviously true if the entering air does not mix with the products of combustion, but merely pushes them before it and it is not difficult to see that it is equally true when the cylinder contents are mixed together. For if a closed vessel contains air in two separate portions at different temperatures, stirring up the contents will not alter the pressure.

If the mixing be complete at all stages of the charging process, that is, if the cylinder contents be at all times homogeneous, the effect of injecting a volume δy of air will be to expel an equal volume of mixed air and products containing the quantity $x.\delta y$ of air. The quantity of air remaining in the cylinder is therefore $(1 - x)\,\delta y$, and it is also equal to δx. Thus we have

$$\frac{dy}{dx} = \frac{1}{1-x}$$

whence

$$x = 1 - e^{-y}$$

the constant of integration being determined by the fact that $x = 0$ when $y = 0$.

(2) *Stratification.* For the reasons given in the text we assume that the quantity of air (always reckoned in cylinder volumes at the temperature and pressure above specified) per unit volume of mixed gas near the exhaust ports is

$$z = x\{1 - \lambda(1 - x)\}, \qquad \ldots\ldots(1)$$

where x as before represents the quantity of air which has been retained in the cylinder and therefore also the proportion of air in the cylinder contents. λ is a constant. Since the injection of δy of air is now accompanied by the expulsion of $z.\delta y$ of air through the exhaust ports, we have

$$\begin{aligned}\delta x &= \delta y\,[1 - x\{1 - \lambda(1 - x)\}] \\ &= \delta y\,(1 + \lambda x)\,(1 - x)\end{aligned}$$

whence, integrating:

$$(1 + \lambda)\,y = \log_e(1 + \lambda x) - \log_e(1 - x)$$

the constant of integration being again determined from the consideration that when $y = 0$, $x = 0$. This gives

$$x = \frac{1 - e^{-(1+\lambda)y}}{1 + \lambda.e^{-(1+\lambda)y}}.$$

As pointed out in the text this formula is open to the theoretical objection that the relation (1) on which it is based cannot hold for values of λ greater than 1 because it would make z negative in the early stages of charging. It is not difficult to construct a relation between z and x which is not open to this objection. For instance we may take

$$z = \frac{e^{2\lambda x} - 1}{e^{2\lambda} - 1}$$

and this leads to the equation

$$x = 1 - \frac{1}{2\lambda}\log_e\left\{1 + (e^{2\lambda} - 1).e^{-\frac{2y\lambda}{1 - e^{-2\lambda}}}\right\}.$$

This is the more complicated formula referred to in the text. With the values of λ met with in practice it gives results which are practically indistinguishable from those obtained from formula (2). It has therefore at present only mathematical interest, but might acquire greater importance if from any cause very long cylinders came into use, as might happen for example in two-cycle Humphrey pumps.

APPENDIX II.

Details of Analyses, etc. Samples were taken simultaneously from each of the four cylinders, from the exhaust-pipe, and from the gas-supply pipe. The exhaust-pipe sample was taken from a point about 30 feet from the engine, and 40 feet from the outlet. The coal gas sample was burnt with air, and the contraction of volume, the CO_2 formed, and the oxygen used in combustion, were determined. The CO_2 in the exhaust sample was absorbed and measured, and the residue was then passed over hot palladium to complete the combustion of any unburnt gas and the contraction and additional CO_2 resulting from this operation were measured. Finally the residual oxygen, viz., the oxygen not used in combustion, was absorbed and measured. From the total CO_2 (including that due to unburnt gas) and from the amount of oxygen used two independent estimates of the quantity of coal gas originally present could be made. An example of the calculation is given below.

The samples from the four cylinders were treated in precisely the same way as the exhaust gas, only here the quantity of unburnt gas was much greater, owing to the fact that from $\frac{1}{3}$ to $\frac{1}{2}$ of the sample comes out before ignition takes place.

In calculating the amount of air from the diaphragm the coefficient of contraction was assumed to be 0·62. The coal gas was metered with a dry meter, which was checked with a standard wet meter. The ratio of air to coal gas determined in this way agrees fairly well with that obtained from the exhaust analysis; where the two differ, that from the analysis has been taken for the purposes of the paper, as being probably the more accurate.

Three trials were run for this purpose. Two of them (I *a* and I *b*) were at a speed of 200 r.p.m., the third (II) was at the usual speed of 250 r.p.m. In all cases the engine was producing a mean pressure of about 64 lbs. per square inch.

TRIAL I *a*.—3.5 P.M., Tuesday, Feb. 10.

Speed, 200 r.p.m.

Combustion of coal gas:

Contraction on combustion ...	137	per 100 parts of coal gas.
CO_2 formed	43·6	
O_2 used	99·6	
Nitrogen content	18	

Exhaust analysis:

CO_2 in sample	2·8	per 100 parts of sample.
Contraction on completing combustion	0·1	
Additional CO_2 formed	0·15	
Residual oxygen	14·75	
Nitrogen (by difference)	82·20	
	100·00	

The total CO_2 formed is 2·95, equivalent from the coal gas analysis to 6·75 of coal gas.

The amount of nitrogen in the sample was 82·20 and of this approximately $0{\cdot}18 \times 6{\cdot}7 = 1{\cdot}2$ was in the coal gas. Thus 81·0 of N_2 equivalent to $\frac{81}{79{\cdot}1} \times 20{\cdot}9 = 21{\cdot}4$ of O_2 came in with the air. The residual oxygen is 14·75, hence the oxygen used is 6·65. This (from coal gas analysis) is sufficient to burn $\frac{6{\cdot}65}{99{\cdot}6} \times 100 = 6{\cdot}68$ of coal gas.

The mean of these two independent estimates is 6·72. The contraction produced by burning 6·72 of coal gas is $6{\cdot}72 \times 1{\cdot}37 = 9{\cdot}2$. Hence the volume of the original mixture of coal gas and air from which this sample was formed was $99{\cdot}9 + 9{\cdot}2 = 109{\cdot}1$*. Of this 6·72 was coal gas and 102·4 was air. Thus we have: Ratio of coal gas to total coal gas plus air taken into engine $= \frac{6{\cdot}72}{109{\cdot}1} = 6{\cdot}16$ per cent., and $\frac{\text{air}}{\text{coal gas}} = \frac{102{\cdot}4}{6{\cdot}72} = 15{\cdot}2$. The value of this ratio obtained by direct measurement of the constituents with diaphragm and with meter was 15·15.

It will be observed that an appreciable amount of the carbon in the coal gas is not burnt in the engine. In some cases the unburnt carbon is as much as 5 per cent. of the whole. In using exhaust analyses to calculate the amount of air it is therefore essential to complete the combustion of the sample by passing over palladium. On the other hand the additional *contraction* obtained by this process is comparatively small, showing that the hydrogen is more completely burnt than the carbon. The combustion is evidently to some extent selective and the thermal value of the unburnt gas in the exhaust is less than the percentage of the CO_2 which is formed by passing over the palladium.

The same method of calculation is used for the cylinder contents as in the case of the exhaust gas. The quantity of burnt gas obtained in the sample is to the amount of unburnt gas in the ratio of the amounts of CO_2 shown in the second and fourth columns of the table following.

* For precision the additional contraction of 0·1, which occurred on completing combustion is here deducted from 100, the original volume of the sample, before adding on the 9·2. This is, of course, of no importance in the present case, but makes a difference when the quantity of unburnt gas is considerable.

TRIAL I *a*. Cylinder Contents.

Cylinder No.	Per cent. of Sample				Total CO_2	Ratio $\frac{\text{coal gas}}{\text{air + coal gas}}$ in cylinder contents before firing, per cent.	
	CO_2 found	Contraction on combustion	Further CO_2 formed	Oxygen left		From CO_2	From O_2
1	2·33	7·81	2·43	9·23	4·77	10·2	10·4
2	2·94	4·15	1·42	11·04	4·36	9·1	9·0
3	3·63	3·97	1·40	10·11	5·03	10·2	9·75
4	2·35	6·43	2·04	10·49	4·39	9·45	9·2
						Mean: 9·45	9·6

TRIAL I *b*. 3.30 P.M., Tuesday, February 10th.

Speed, 200 r.p.m.

Combustion of coal gas:

Contraction on combustion	136	per 100 volumes of coal gas.
CO_2 formed	42·0	
O_2 used	96·8	
Nitrogen content	19	

Exhaust analysis:

CO_2 in sample	2·8	per 100 volumes of sample.
Contraction on completing combustion	0·05	
Additional CO_2 formed on completing combustion	0·1	
Residual oxygen	15·4	
Nitrogen (by difference)	81·65	
	100·00	

The quantities of coal gas used per cent. of the total air + coal gas taken into the engine, estimated from the CO_2 and the oxygen respectively as above, are:

From CO_2	6·31
From oxygen	5·70
Mean:	6·00

The ratio $\frac{\text{air}}{\text{coal gas}}$ is therefore 15·7. The value of this ratio obtained by direct measurement of each constituent by diaphragm and meter was 15·1.

TRIAL I *b*. Cylinder Contents.

Cylinder No.	Per cent. of Sample. CO_2 found	Contraction on combustion	Further CO_2 formed	Oxygen left	Total CO_2	Ratio $\frac{\text{coal gas}}{\text{air + coal gas}}$ in cylinder contents before firing, per cent. From CO_2	From O_2
1	3·26	6·43	2·14	9·39	5·40	11·6	10·2
2	2·66	5·74	1·54	11·0	4·20	9·3	9·1
3	2·22	6·22	2·16	10·5	4·38	9·7	9·4
4	1·93	6·66	2·34	11·07	4·27	9·5	8·85
						Mean: 9·8	9·4

It will be observed that throughout the analyses in this test the proportion of coal gas calculated from CO_2 is systematically higher than that calculated from the oxygen. This is probably due to a small error in the analysis of the coal gas, which would affect the calculations from all the other analyses equally. The mean of the values obtained by the two methods is probably within 3 per cent. of the truth. This statement is confirmed by the fact that the absolute amount of air calculated from this mean value and from the gas meter is precisely the same in test I *b* as in test I *a*. The diaphragm reading was the same in these two cases, showing that the quantity of air was in fact the same within 2 per cent.

TRIAL II. 4.5 P.M., Tuesday, February 10th.

Speed, 250 r.p.m.

Combustion of coal gas:

Contraction on combustion	135	per 100 volumes of coal gas.
CO_2 formed	40·8	
O_2 used	93·7	
Nitrogen content	17	

Exhaust analysis:

CO_2 in sample	4·8	per 100 volumes of sample.
Contraction on completing combustion ...	0·8	
Additional CO_2 formed on completing combustion	0·4	
Residual oxygen	10·6	
Nitrogen (by difference)	83·4	
	100·0	

Quantity of coal gas used per cent. of air and coal gas taken into engine:

Calculated from CO_2	10·94
,, ,, O_2	10·24
Mean:	10·6
$\frac{\text{Air}}{\text{Coal gas}}$, by analysis	8·4
,, by diaphragm and meter	8·7

TRIAL II. Cylinder Contents.

Cylinder No.	Per cent. of Sample				Total CO_2	Ratio $\frac{\text{coal gas}}{\text{air + coal gas}}$ in cylinder contents before firing, per cent.	
	CO_2 found	Contraction on combustion	Further CO_2 formed	Oxygen left		From CO_2	From O_2
1	3·93	4·57	1·51	9·37	5·44	11·8	10·8
2	5·23	3·68	1·21	7·20	5·44	13·4	12·85
3	3·41	7·73	2·41	7·57	5·82	12·8	12·3
4	3·92	5·23	1·81	8·86	5·73	12·4	11·2
						Mean: 12·6	11·55

As in test I *b*, the quantities of coal gas calculated from the CO_2 are systematically greater than those obtained from the oxygen. The cause is probably the same—a small error in the CO_2 formed or the oxygen used in the combustion of the coal gas. Such an error affects equally both the exhaust analysis and the analysis of cylinder contents, so that the estimate of the proportion of air lost to exhaust (which is the main object of this test) is practically the same whether it be derived from a comparison of the CO_2 or of the oxygen used in these two analyses.

23

EXPLOSIONS OF COAL GAS AND AIR.

["PROCEEDINGS OF THE ROYAL SOCIETY," A. Vol. LXXVII, 1906.
Communicated by Professor EWING, F.R.S.]

THE experiments here described consist in an investigation into the propagation of flame through a mixture of coal gas and air contained in a closed vessel and ignited at one point by an electric spark. A continuous record is taken of the variation of resistance of fine platinum wires immersed in the gas, at different points; and at the same time and on the same revolving drum the pressure is recorded. The arrival of flame at any wire is marked by a sharp rise in its resistance. Thus the progress of the flame can be traced. Moreover, the rate of rise of temperature of the wire after the flame has reached it is (after certain corrections have been applied) a measure of the velocity with which the gases round about it combine. In this manner it has been possible to settle in the case of certain mixtures, at any rate, the question of "after-burning," which has long been a matter of controversy in the theory of the gas engine, and to determine approximately the specific heat of the mixture of CO_2, H_2O, and inert gases which are the products of the combustion. Incidentally it has been necessary to find what relation the temperature of a fine platinum wire immersed in the heated gas bears to that of the gas. Burstall, who has measured the temperature in a gas engine cylinder by means of platinum wires, did not fully investigate this point, and his results are, in consequence, open to doubt*.

Before proceeding to a detailed account of the apparatus and records obtained it will be convenient to state shortly the principal conclusions reached. The experiments were all made on mixtures of air and Cambridge coal gas having an average "higher" calorific value of 680 B.TH.U. per cubic foot at 0° C. and 760 mm. The composition of the gas is given in an appendix. The mixture was fired at atmospheric pressure in a vessel

* *Proceedings Inst. Mech. E.*, 1901. The only attempt to apply the methods of platinum thermometry to gaseous explosions, of which I am aware, is this one by Burstall. He could not use very fine wire because it melted, and it seems probable that on this account his temperatures are a good deal wrong, even in the latter part of the explosion, and his results give no information as to the initial stages of the burning and throw no light upon the question of the velocity of combination. He used a rotating contact maker, which made contact at definite epochs in the cycle, but did not give a continuous record of temperature in any one explosion.

of dumpy cylindrical form and of a capacity of 6·2 cubic feet, which is shown in section in Fig. 1, page 372. The combustion was started by an electric spark at the centre of the vessel.

Explosion of Rich Mixture.

With nine volumes of air to one of gas the maximum pressure varied from 76 to 82 lbs. per square inch above atmosphere; and was reached about 1/4 of a second after firing; on Fig. 2, p. 374, is shown a facsimile of a pressure diagram (curve *A*). It was found with a mixture of this composition (air/gas = 9) that:

(1) The flame spreads from the spark with a velocity which varies somewhat in an apparently accidental manner, but which is roughly 150 cm. per second. The spread of the flame is of a rather irregular character, and differs slightly in different directions; Mallard and Le Chatelier found for a mixture containing 17 per cent. coal gas, a velocity of flame propagation of 125 cm. per second along a tube of 2 or 3 cm. diameter*.

(2) The flame reaches the walls when the pressure is of the order of 15 to 20 lbs. per square inch above atmosphere. At this point, however, only a small portion of the walls is in contact with the flame, namely, that nearest the spark, and most of the gas is still unignited. As the flame spreads a greater and greater area of the walls comes into contact with it, until the flame completely fills the vessel and is losing heat to every part of it. At this point the pressure is still a little short of the maximum, being about 70 lbs. per square inch when the maximum pressure reached is 82 lbs. Maximum pressure is attained in less than 1/30 of a second after the flame completely fills the vessel.

(3) At the centre of the vessel the temperature of the gas rises very rapidly, after ignition, to about 1225° C. It reaches that figure, within 50°, in a time which is certainly less than 1/20 of a second, and probably less than 1/50 of a second; in other words, the combustion is complete within about 4 per cent. in that time. The temperature remains nearly steady during the earlier part of the spread of the flame, the pressure during this time remaining very nearly constant. The combustion at the centre takes place very nearly at constant pressure, and is complete within 5 per cent. before the pressure has risen more than a couple of pounds above atmosphere. From this result, if it be assumed that no heat is lost until the flame reaches the walls of the vessel, it follows that the capacity for heat at constant pressure, reckoned between 50° and 1200° C. of the products of the explosion, is about 1·5 times that of the same volume of air. There is no doubt that the flame radiates some of the heat of combustion, but it is improbable that the loss from this cause exceeds 15 per cent. If that percentage be assumed then the capacity for heat is 1·3 times that of air.

* *Recherches sur la combustion des Mélanges Gazeux Explosifs.*

(4) In the adiabatic compression of the gas in the centre, which takes place in the later stages of the explosion, the temperature rises to a point which is considerably above the melting point of platinum—probably about 1900° C. If the ultimate temperature, corresponding to a compression of 6·5 atmospheres absolute, is 1900° C. then the average value of γ (ratio of specific heats) for these gases is 1·25 between 1200° and 1900° C.

(5) A platinum thermometer, placed about 1 cm. within the walls at the furthest point from the spark, reaches its maximum temperature, which varies accidentally, but is between 1100° and 1300° C., within 1/30 of a second after the attainment of maximum pressure. There is here but little adiabatic compression or rise of temperature after ignition. The gas at this point has been compressed to about five atmospheres before ignition, and the temperature due to this compression is about 200° C. Experiments by Petavel*, in which coal gas and air were exploded after compression to about 77 atmospheres, show that the rise of temperature on explosion is nearly independent of the pressure. If therefore there were no loss of heat, the temperature at this point would rise to about 1400° C. At the same instant the temperature at the centre is 1900° C. Consequently, even if the explosion were to take place in a vessel impervious to heat, there would be a difference of 500° C. between the maximum and minimum temperatures in the vessel. Similar differences must exist whenever a mixture is fired at discrete points; the temperature at the firing points will always be raised much above the mean by adiabatic compression after ignition.

(6) It seems certain from these experiments that in a mixture of this strength combustion is for practical purposes everywhere complete at the time of maximum pressure, and that subsequent to that time the gas is a mixture of CO_2, H_2O, and inert gases in chemical equilibrium.

(7) At the moment of maximum pressure the distribution of temperature is roughly as follows:

Mean (inferred from pressure) ...	1600
(*a*) Centre near spark	1900
(*b*) 10 cm. within the wall (*C*, Fig. 1)	1700
(*c*) 1 cm. from wall at end (*D*, Fig. 1)	1100 to 1300
(*d*) 1 cm. within the wall at side ...	850

At points *a*, *b*, and *c*, the gases can have lost but little heat at this time, and the differences of temperature are almost wholly due to the different treatment of the gas at different places. At (*a*) it has been burnt nearly at atmospheric pressure, and compressed after burning to about six and a half atmospheres absolute, while at (*c*) it has been first compressed to about six atmospheres as in a gas engine, and then ignited without any subsequent compression. At the point (*d*) much heat has been lost, since this is the first point on the wall reached by the flame; the gas here is ignited

* *Phil. Mag.*, May, 1902.

when the pressure is about two atmospheres, its temperature rises instantly to 1300° C., and at once begins to fall.

(8) Up to the time of maximum pressure convection currents in the gas have had but little effect upon the distribution of temperature, which is mainly determined by the treatment accorded to the gas at different points. After combustion is complete, however, the motion of the gas set up by the explosion or by convection currents rapidly obliterates these initial differences. Half a second after maximum pressure the distribution of temperature is as follows: The mean temperature of the whole of the gas (calculated from the pressure) is about 1100° C. The mean temperature, exclusive of a layer 1 cm. thick in contact with the walls (shown by the resistance of a long wire stretched from the centre to *D*, Fig. 1), is about 1160° C. The temperature of the hot core is fairly uniform, though it varies in an accidental manner about the mean value; thus, at the centre, in different explosions, I have found temperatures ranging from 1100° to 1200° C. at this time. The mass of gas during cooling may therefore be described as a hot core in which the temperature is approximately uniform (though it varies accidentally as the result of currents) surrounded by a thin layer wherein the temperature falls to the temperature of the walls. I find, by calculation, that if such layer were $\frac{1}{2}$ cm. thick, and if the fall of temperature were uniform, the mean temperature inferred from the pressure would fall short of that of the hot core by about the observed amount, viz., 60° C.

Explosion of Weak Mixtures.

The explosion of a weak mixture containing 12 volumes of air to 1 of gas differs markedly from that of a 9 to 1 mixture. In the latter case there is no time for the buoyancy of the light burnt gas to materially affect the propagation of the flame, though it doubtless causes the apparent velocity of propagation to be slightly greater in an upward direction than downward. Convection currents have no material influence on the phenomena until after the attainment of maximum pressure. But in the weaker mixture their effect is important from the outset. The small portion of the gas first ignited instantly rises with increasing velocity. At the same time it grows, by the ignition of that surrounding it, but at a rate which soon becomes considerably less than the velocity with which it is rising. In spite of the very slow propagation of flame from point to point, however, the combination of the gases, once initiated, is rapid. Thus the temperature of a wire placed close to the spark rises within 0·07 second to about 1000° C., and then remains nearly stationary for some time. A few centimetres below the spark the temperature will rise rapidly and then fall; the flame reaches the wire, and is then carried upward and away from it, the wire being cooled by the current of cold, unburnt gas which follows in the wake of the ascending flame. About 1 second after ignition, and while the

pressure is still less than 10 lbs. above atmosphere, the upper half of the vessel is filled with burnt gas which is in contact with, and losing heat to, the upper half of the walls. In the lower parts of the vessel the gas is still unburnt. The last portions of gas to be ignited are those immediately under the spark, and from 10 to 20 cm. away from it. A wire placed at this point shows a gradual rise of temperature, due to the adiabatic compression, followed by a sudden rise, due to ignition, slightly before the time of maximum pressure. In general, a platinum wire placed anywhere within the vessel shows, at some time *before* maximum pressure, a sharp rise in temperature lasting about 1/10 of a second, after which the temperature is steady for a time, and then falls slowly. There are fluctuations of temperature both up and down, but these are plainly accidental effects of convection currents, and are due neither to the ignition of portions of unburnt gas nor to the slow combination of gas already ignited.

My experiments seem to show that in the weakest inflammable mixtures and in strong mixtures alike, the combustion once initiated at any point, is completed almost instantaneously. Moreover, the complete inflammation of the gas is, even in the weakest mixture, nearly simultaneous with the attainment of maximum pressure. One-tenth of a second before maximum pressure there are undoubtedly places to which the flame has not spread, and it is extremely improbable that there are any such places 1/10 of a second after. It is safe to assume in dealing with a 12/1 mixture that 1/5 of a second after maximum pressure (when the loss of pressure by cooling is still less than 5 per cent.) there is present in the cylinder a mass of CO_2, H_2O, and inert gas in complete chemical equilibrium. In the 9/1 mixture this state is, of course, attained very much sooner. The difference in the behaviour of the weak and strong mixtures is wholly due to the very slow propagation of flame in the former; in a 9/1 mixture the flame seems to travel about 10 times as fast as in the 12/1 mixture.

In the 12/1 mixture maximum pressure is about 50 lbs. above atmosphere, and is reached about 2·5 seconds after the passage of the spark. During nearly half that time, at least half the area of the vessel has been in contact with the flame. It seems probable, therefore, that the proportionate loss of heat before the attainment of maximum pressure in a weak mixture is considerably greater than in a strong one. In other words, if the explosion were adiabatic the ultimate pressure produced would exceed the maximum actually observed by a greater proportion in the case of the weak than in that of the strong mixture, and this mainly by reason of the greater percentage loss of heat.

Description of Apparatus and Records.

Fig. 1 shows a section of the vessel along its axis, with its principal dimensions. *A* is the sparking point—nearly at the centre of the vessel. *B*, *C*, and *D* are three platinum thermometers; *B*, in the case shown, is close to the spark, *C* at a distance of about 30 cm., and *D* about 1 cm. from

the walls of the vessel. Each thermometer consists of a coil of about 5 cm. of pure platinum wire, 0·001 inch diameter. The coil is hard-soldered to two thicker platinum wires sealed into the ends of glass tubes, and the thicker wires are soldered to stout copper leads. The copper-platinum junctions, being just within the tubes, are protected from the flame. Owing to this protection, and to the rapidity of the changes of temperature to be measured, such changes are practically confined to the fine wire, and no compensating leads are necessary, nor is there any substantial thermo-electric effect. Each thermometer coil is placed in series with a storage cell and with a d'Arsonval galvanometer. The galvanometer has a stiff

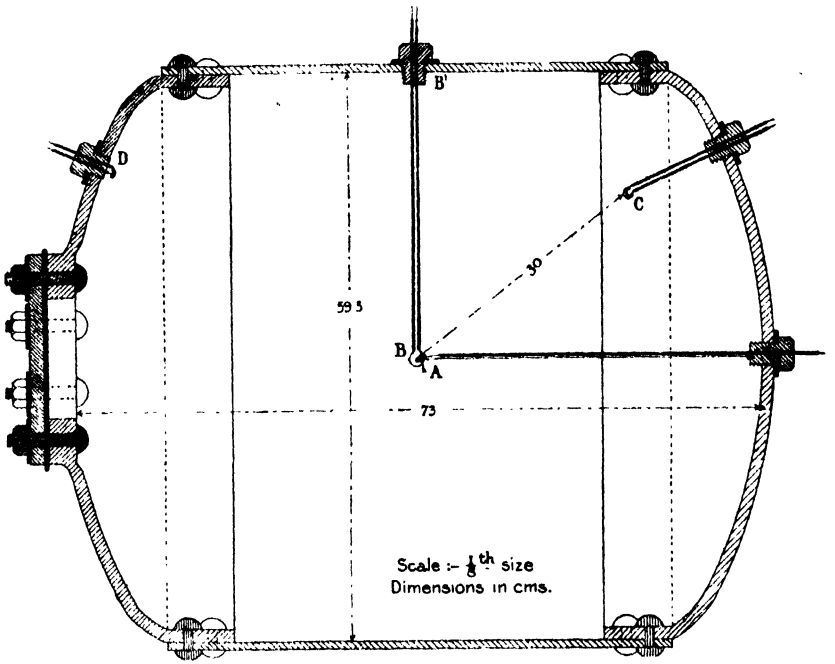


Fig. 1.

suspension of phosphor-bronze wire, giving a period of oscillation of from 1/50 to 1/30 of a second. The mirror of the galvanometer throws the image of a fine hole, illuminated by an arc-lamp, on to a revolving drum carrying a photographic film. The deflection of the galvanometer, when corrected for inertia effects, gives the current flowing, which is inversely proportional to the resistance of the circuit. From the resistance so obtained is deducted the resistance in the cold, and the difference is the rise in the resistance of the fine wire. Hence, knowing the resistance of that wire when cold, we can calculate its rise of temperature. Ordinarily two thermometers were in use at once, each with its galvanometer, and a record was thus obtained, on the same drum and in the same explosion, of the changes of temperature at two different points of the vessel.

A record of pressure was taken on the same drum. The indicator was of simple construction, consisting of a steel piston, which was forced by the pressure against a piece of straight spring held at the ends. The displacement of the spring tilted a mirror about a fulcrum, and the mirror cast an image of the above-mentioned fine hole on the moving film. A detailed description of the indicator is unnecessary; it will suffice to say that it had a natural period of about 1/300 of a second, and so was able to follow very rapid changes of pressure.

Before making an experiment steam was blown into the vessel, so that the air should be as nearly as possible saturated with moisture. As will be seen later, the quantity of moisture present has some influence on the temperature reached, and there is some uncertainty from this cause, as one could not be sure that the saturation was complete. The gas was measured in by exhausting the vessel until the pressure was 9/10 of an atmosphere and then admitting the gas. From four to six hours were allowed for mixing, and the completeness of the combustion was tested in a few cases, and found satisfactory by observing the fall of pressure due to condensation of the steam formed in the explosion. The galvanometers were calibrated at the time of the experiment by observing the deflections produced when known resistances were placed in the circuit instead of the thermometer coils. Fig. 2 is a facsimile of one of the records obtained. One revolution of the drum, equal to the length of the diagram, is about 1·15 seconds. The pressure record, which is marked A, needs no comment. It may, however, be observed that the indicator shows traces of very rapid oscillation of pressure just before, and for a short time after, the maximum. These oscillations have a frequency of about 1000 per second, which is roughly the frequency of the note heard when the explosion takes place. Having regard to the much longer period of the indicator, they are evidence of a violent state of motion within the vessel, due, no doubt, to the setting up of an explosive wave shortly before maximum pressure is reached. The curve marked B is a record of the temperature at the centre of the vessel, being traced by thermometer B (Fig. 1). B_0 is zero line for this curve, and is traced on the film immediately after the explosion by disconnecting the thermometer circuit. The current flowing in this circuit is, therefore, after allowing for inertia effects, proportionate to the ordinate of curve B, reckoned from the zero line B_0, and the resistance of the circuit is inversely proportional to that ordinate. It will be observed that the current, after remaining at the value corresponding to atmospheric temperature, suffers a sudden diminution at the point B_1, the galvanometer being thrown into violent oscillation thereby. This is, of course, due to the spread of the flame to the thermometer coil; and, as the coil is within 2 cm. of the ignition spark, B_1 marks approximately the initiation of the explosion.

Tracing the variations of the ordinate of curve B it will be noticed that

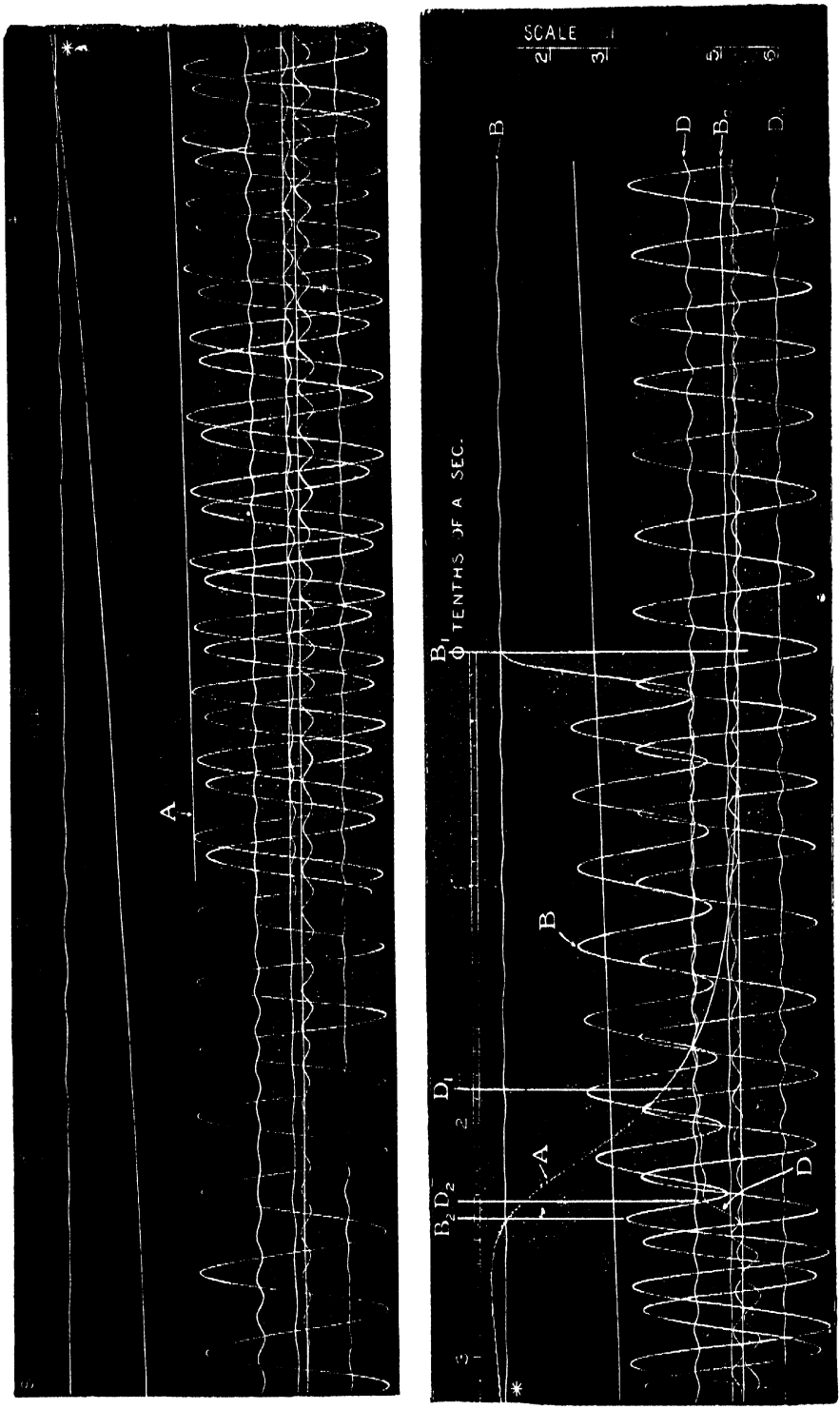


Fig. 2. Air/Gas = 9.

its mean value, when the oscillations are smoothed out, remains nearly constant after the first rapid fall, corresponding to a very slow increase in the temperature of the wire. It then diminishes owing to the rise of temperature produced by the adiabatic compression, until finally at the point B_2 it falls suddenly to zero. This point which is slightly before the maximum pressure, corresponds to the melting of the wire. The temperatures corresponding to the successive ordinates are plotted on Fig. 3. Turn now to the line marked D, Fig. 2. This is a record of the current in, and the tem-

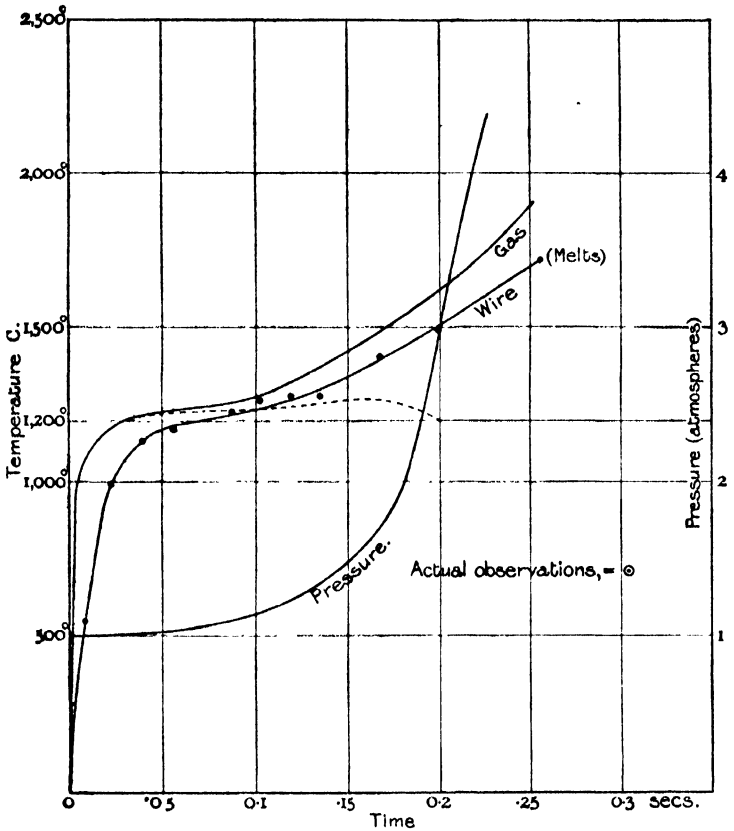


Fig. 3.

perature of, thermometer D (Fig. 1) which is placed about 1 cm. within the wall of the vessel. The galvanometer in this case has a stiffer suspension and a lower period of oscillation, and is therefore less sensitive. D_0 is the zero line for this record. It will be noticed that at the point D_1 the ordinate of this curve has begun gradually to diminish, showing the slow increase of resistance due to the adiabatic compression produced by the expansion of the ignited gas in the centre of the vessel, and it continues until the point D_2, where there is a sudden rise in resistance. This sudden rise is due to the flame reaching the thermometer coil and marks approximately the complete

the times taken to travel this distance (about 30 cm.) were respectively 0·19, 0·21 and 0·17 second. In another explosion a pyrometer was placed just inside the wall at the point B'. It was found that though this point is if anything rather nearer to the spark than C, the flame reached it 1/70 of a second later, a further illustration of the irregular manner in which the flame is propagated.

A large number of diagrams were taken with a weak mixture in which the proportion of air to gas was 12/1. The thermometers were placed in all sorts of positions. The general character of the results is indicated above. One diagram is reproduced in Fig. 4. In this case one wire only was used, and it was placed about 15 cm. from the spark and vertically below it. It will be observed that at first the temperature rises very slowly. More than two seconds after ignition (at A) it is only about 210° C., and such heating as has then occurred is almost entirely due to adiabatic compression*. The flame now reaches it (A) and the temperature rises in 1/10 of a second to 1300° C. The pressure has now attained its maximum value (50 lbs. above atmosphere) and about $2\frac{1}{5}$ seconds have elapsed since the spark passed. The temperature remains

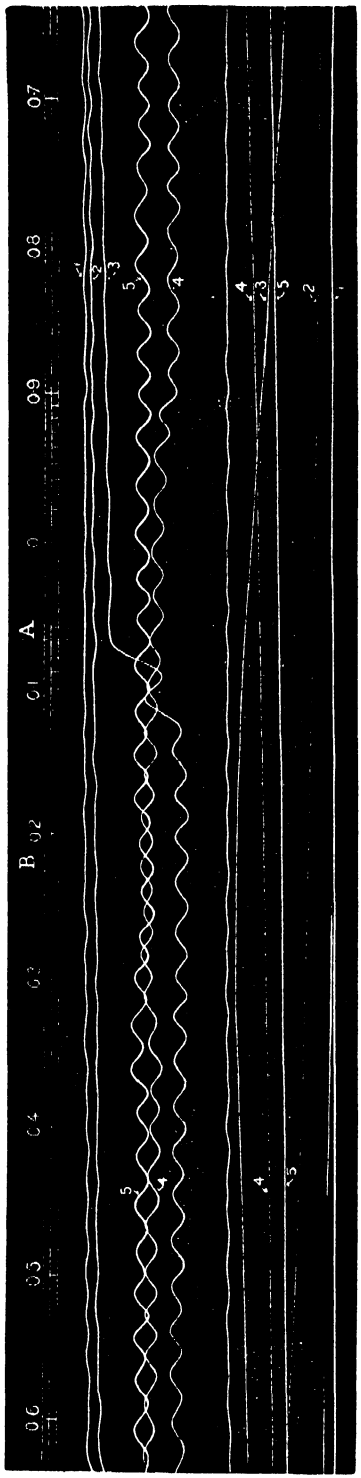


Fig. 4. Air Gas = 12.

[The successive portions of the pressure and temperature curves are numbered in the order of the corresponding revolutions of the drum.]

* Owing to the heating effect of the current the wire starts about 90° hotter than the gas. The compression of the gas to 50 lbs. above atmosphere will cause its temperature to rise 160° C. The wire will then be hotter than the gas, but the difference will be somewhat less than 90°, since the heat to be dissipated is less because of the increased resistance. At temperatures of 1000° and over, the resistance is so great that the current in the wire can have but little effect upon its temperature.

steady for a while and then falls; but there is no perceptible rise after the pressure begins to fall. In a large number of trials I was unable to discover any point at which inflammation occurred later than in the diagram here shown, so that it is probable that all the gas is ignited at the time of maximum pressure. Moreover, this must be nearly the last portion of the gas to be ignited, and here, if anywhere, the effects of slow combustion of gas already ignited would be perceptible in a rise of temperature after maximum pressure. In other words the gas is in chemical equilibrium within a very short time after maximum pressure.

The Specific Heat at Constant Pressure and the Velocity of Reaction.

Returning now to the record shown on Fig. 2 with a 9/1 mixture, the temperature of the wire at the centre of the vessel is plotted in terms of the time on Fig. 3. The gas temperature always exceeds that of the wire, partly because of time-lag, and partly because of radiation. The gas temperature is shown on the same figure, being calculated from that of the wire by methods to be explained later. On the same figure is shown the rise of pressure.

The dotted curve shows the temperature of the gas corrected for compression, that is, it is the temperature which the gas would have had if it had remained at atmospheric pressure. In calculating this curve it is assumed that the relation between pressure and temperature in adiabatic compression is $\theta \propto p^{0 \cdot 23}$. The selection of the exponent will be justified later; it corresponds to taking γ (the ratio of the specific heats) as 1·3.

It will be noticed that from 0·05 second to 0·2 second the temperature corrected for compression is within 30° of 1230° C., beyond this point the deviation becomes wider, but then the correction is no longer small, and the exponent of p diminishes as the temperature rises. Moreover a temperature of 1170° C. is reached in 0·02 second. We may take it then that the ultimate temperature produced by the combustion at constant pressure of the particular mixture used in this experiment is 1230° C., and that the combustion is complete within 5 per cent. in 1/40 of a second. The composition of the mixture is rather uncertain, but assuming that the gas has its average composition, and that, before explosion, both gas and air are saturated with moisture at 20° C. and 14·7 lbs. per square inch, the products of the combustion will be as follows:

CO_2	... ...	0·56 cubic feet
H_2O	... ...	1·53 ,,
N and O	... ...	7·60 ,,
Total	...	9·69 ,,

assuming that the CO_2 and H_2O occupy their proper molecular volumes and the volumes being reckoned at 20° C.

The heat produced by the combustion when the gas is burned in a calorimeter is approximately 620 B.TH.U., the products being cooled to 20° C. Of this, however, about 60 B.TH.U. are to be ascribed to the condensation of the steam produced in the explosion. If the cooling were stopped at 55° C. (when condensation begins) only 550 B.TH.U. would be obtained, about 10 heat units being due to the cooling of the gases other than steam from 55° to 20° C. Moreover in the burning as it actually takes place a certain portion of the heat is radiated from the surface of the flame*. The amount of this loss is quite uncertain; but it is improbable that it exceeds 15 per cent. If we assume that figure, there remain 470 B.TH.U. as the heat evolved in cooling the products at constant pressure from 1230° to 55° C. The same volume of air cooled through the same range would evolve 370 B.TH.U. Thus the average volume specific heat of the products is 1·3 times that of air. At constant volume the ratio would be 1·4, taking γ for the products as 1·3. If, on the other hand, we assume that the radiation can be neglected, the ratio of the specific heats of air and products is 550/370 or about 1·5. I think it is fairly certain that the true value is

* In the paper as originally written there was a note dealing with this point, in which I gave my reasons for supposing that the heat radiated could be neglected, and the specific heat was calculated on that assumption. Some remarks made by Professor Callendar at the discussion of the paper, however, have led me to look into the matter further, with the result that I must now admit the probability of a loss by radiation during the early stages of the explosion of 15 per cent. of the total heat then being generated. Professor Callendar stated that he had observed that an ordinary Bunsen flame radiates from 15 per cent. to 20 per cent. of the total heat of the gas used, which is a much greater proportion than I had thought possible; it is reasonable to suppose that the proportion of heat radiated from the flame at the centre of my vessel will at least be of the same order, though the flame is somewhat colder than the Bunsen flame. Among the reasons that I gave for neglecting the radiation was the fact that no rise of temperature, other than that due to the adiabatic compression of the gas surrounding them, was observed in any of the wires in the outer part of the vessel (*e.g.*, that at C, Fig. 1), until they were actually in the flame. On working out the actual figures, however, I find that radiation from the flame could not certainly be detected by such rise of temperature unless it amounted to 15 per cent. of the heat generated, in which case it would just be apparent.

An estimate of the specific heat, unaffected by radiation curves, can be obtained from the explosion of a weak mixture, such as that giving the diagram of Fig. 4. In this case the burning of the last portion of the gas to be ignited, which is that round about the thermometer coil, takes place in an inward direction, so that there can be but little radiation from the flame surface. The gas just before ignition has been compressed to 50 lbs. per square inch above atmosphere, and its temperature is very nearly that due to such compression, viz., 160° C. On combustion the temperature rises to 1300° C. The products of combustion of 1 cubic foot of gas in this case consist of about 2·1 cubic feet of steam and CO_2 and 10·8 cubic feet of N and O. The internal energy is the heat of combustion (550 B.TH.U.) plus the heat required to raise the mixture to 160° C. before firing (63 B.TH.U.); total, about 610 B.TH.U. evolved in cooling from 1300° to 55° C. Of this the N and O account for 427 B.TH.U., leaving 183 for the 2·1 cubic feet of CO_2 and H_2O. The capacity of heat for the latter is accordingly 2·16 times that of the same volume of air. If we take this figure and calculate from it the heat required to warm the products of the explosion of 1 cubic foot of gas and 9 of air from 55° to 1230° C., the result is 464 B.TH.U., whereas the total heat evolved in that explosion is 550 B.TH.U. The discrepancy may be accounted for by supposing a radiation loss of about one-sixth part; and if such loss be assumed, the specific heat of the products at constant pressure works out to about 1·3 times that of air.

The gas *within* the flame surface is of course unaffected by radiation, and its treatment must be sensibly adiabatic until convection currents come into play.

between 1·3 and 1·5, and probable that it is near the lower figure. This, of course, is the average value; at the upper temperature the ratio will be a good deal greater. At 55° C. the specific heat of the products is about 1·05 times that of air. Of the heat evolved in cooling, 290 B.TH.U. are accounted for by the nitrogen and oxygen present. The balance of 180 B.TH.U. (assuming the 15 per cent. loss) is the heat capacity of the 2·1 cubic feet of CO_2 and H_2O. The average volume specific heat of these gases is accordingly about $2\frac{1}{4}$ times that of air over the range 55° to 1230° C. It is obvious that the uncertainties in the amount of radiation, the composition of the gas, etc., are such that this can only be regarded as a very rough approximation to the truth.

It is not difficult to measure the original diagram correct to 2/10 mm. and the ordinates in the second column are probably within that amount of the truth, and certainly within 4/10. The corresponding errors in the temperature, at the highest temperature measured, are about 35° and 70° C. respectively. The order of accuracy of the results is further shown in the following table, which gives the temperature found at corresponding times in six different explosions. The composition of the mixture is nominally the same in all cases except the second, in which the preliminary steaming was omitted, so that the gases were very much drier than in the other cases.

Table II.

July 19*	Sept. 8*	Sept. 11*	Sept. 15	Sept. 19	Sept. 21†	Mean
1010°	1205°	995°	995°	1065°	965°	1000°
1110	1220	1135	1085	1085	1115	1130
1135	1260	1165	1115	1120	1200	1147
1200	1330	1165	1150	1200	1250	1190
1250	1330	1225	1200	1245	1230	1230
1225	1300	1260	1205	1195	1230	1220
1240	1300	1275	1185	1270	1230	1240

* Wire melted.

† Wire 10 cm. long, and further from spark.

In criticising this table it is to be remembered that the calorific value and composition of the gas were not determined from day to day, and the calorific value may have varied between limits differing by 5 per cent. Moreover, the amount of water vapour present must also have varied somewhat. The combustion of 100 volumes of gas in 900 of air produces 133 volumes of steam, in addition to this there might be present (at 20° C. 760 mm.) anything up to 23 volumes of moisture before explosion, according to the dryness of the gases. The high temperatures shown on September 8 may be explained by the fact that there the gases were nearly dry, whereas in the other cases they were nearly saturated. This result also serves to show the considerable influence of the amount of moisture

present—due probably to the high specific heat of steam at such temperatures. In calculating the mean temperatures in the last column, the results of September 8 are left out of account. The temperatures are those of the wire, uncorrected for radiation or time-lag.

The Ratio of Specific Heats.

In the adiabatic compression of any substance we have

$$\frac{dp}{d\theta} = \frac{c_p}{\theta\,(\partial v/\partial\theta)_p}. \qquad \text{.....(1)}$$

We have here to deal with a mixture of gases, 79 per cent. of which is perfect gas. The remainder, CO_2 and H_2O, is not perfect, but its deviation from the gas law $p\sigma/\theta$ = const. is dependent on dissociation, the effect of which is limited in amount to a reduction of the absolute density to two-thirds of its value at ordinary temperatures. If dissociation were complete the density of the mixture would become 10 per cent. less than at ordinary temperatures. In other words the characteristic equation is

$$p\sigma/\theta = R, \qquad \text{.....(2)}$$

where R is a continuously increasing quantity, which at very high temperatures may attain a value 10 per cent. in excess of its value at 50° C. At 1200° C. it is improbable that dissociation is complete, or that R has nearly reached the limit of its increase. Now differentiate (2):

$$\left(\frac{\partial v}{\partial\theta}\right)_p = \frac{1}{p}\left\{\theta\left(\frac{\partial R}{\partial\theta}\right)_p + R\right\},$$

and we may now write (1) in the form

$$\frac{dp}{d\theta} = \frac{p}{\lambda\theta}, \text{ where } \lambda = \frac{1}{c_p}\left\{R + \left(\frac{\partial R}{\partial\theta}\right)_p \theta\right\}, \qquad \text{.....(3)}$$

and is a quantity which changes slowly with c_p and R. The integral of (3) is $\theta \propto p^\lambda$ approximately. Now c_p has been shown by the experiments to be more than 25 per cent. greater at 1200° C. than at 50° C. It is unlikely that the term $R + \theta\,(\partial R/\partial\theta)_p$ increases in anything like so large a proportion, since R cannot increase by more than 10 per cent. We may, therefore, confidently expect a substantial diminution in λ as the temperature and pressure rise. Now for a perfect gas $\lambda = (\gamma - 1)/\gamma$, where γ is the ratio of the specific heats, and this relation is also nearly true for the mixture in question. At 50° C. the value of γ for this mixture is not accurately known, but it is probably not greater than 1·37, making λ something less than 0·27. It may be expected, therefore, that λ at 1200° C. is less than 0·27, and γ less than 1·37.

An inferior limit to λ at 1200° C. can be deduced from the experiments as follows: In the explosion described above, the wire melted at a pressure of 6·5 atmospheres. The temperature of the gas must then have been considerably above the melting point of platinum; in all probability it was

1900° C.* At a pressure of one atmosphere the temperature of the gas was 1230° C. Calculating from the equation $\theta \propto p^{\lambda}$ it follows that the average value of λ between 1200° C. and 1900° C., is 0·2. Since λ is a diminishing quantity, its value at 1200° C. must be greater than the average value between these limits, viz., greater than 0·2.

We may, therefore, assert with a high degree of probability that the value of λ for this mixture is between 0·2 and 0·27 at 1200° C. and that the ratio of the specific heats at that temperature is between 1·25 and 1·37. For the purpose of correcting the temperatures in the neighbourhood of 1200° C., the value 0·23 was assumed for λ, and for a small correction this is no doubt near enough to the truth.

The Measurement of Temperature.

The temperature of the gas differs from that calculated from the resistance of the wire from four causes, the effects of which must be considered:

(1) The law connecting the resistance of platinum and its temperature has not been experimentally verified beyond 1000° C. Up to that temperature, however, it has been found that the temperatures given by the platinum and gas thermometers agree within about 10° C. I found that a sample of platinum wire whose purity is indicated by its temperature coefficient—0·00388—melted when its temperature as shown by its resistance was 1670° C. The real temperature must have been slightly under 1710° C. It is probable that extrapolation up to 1500° C. will be within the general order of accuracy of these experiments.

(2) The ends of the wire are colder than the middle because of conduction. As the diameter of the wire is only 1/2000 of its length, it may be expected that this correction will not be important. Suppose that the temperature falls at the end of the wire by 1000° C. in 1 mm., and that the heat conductivity of the platinum is the same as when cold, viz., 0·08, then the amount of heat conducted away per second by the wire 0·001 inch diameter will be $\frac{10000}{200000} \times 0{\cdot}08 = 0{\cdot}004$ calorie per second. Now the heat supplied to the wire by the gas per millimetre length is shown below to be about 0·0035 calorie per second for every 100° C. by which the gas is hotter than the wire. A comparison of the two magnitudes shows at once that at a distance of 1 mm. from the end the temperature of the wire must have become sensibly uniform. This inference has been verified by comparison of the temperatures shown by wires of very different length (ranging from 3 to 10 cm.) under the same circumstances.

(3) The temperature of the wire lags behind that of the gas, when the latter is changing rapidly. To test the effect of this, wires of two different thicknesses—1/1000 and 2/1000 of an inch respectively—have been placed

* See the section "Measurement of Temperature," below.

as close together as practicable, and their temperatures compared in the same explosion*. The result is shown in Fig. 5, and was confirmed in several experiments. Both wires were within 5 cm. of the spark, so that the rise of temperature in the thin wire is similar to that shown on Fig. 1. It melted shortly before the attainment of maximum pressure. At the point *A* the temperature of the thick wire is rising at the rate of 5000° C. per second. At the same moment the temperature of the thin wire is rising about 1300° C. per second. Since the thick wire has four times the mass of the thin, it is receiving heat about 15 times as fast. Now, the rate at which a very fine wire receives heat from a gas, by conduction, is almost inde-

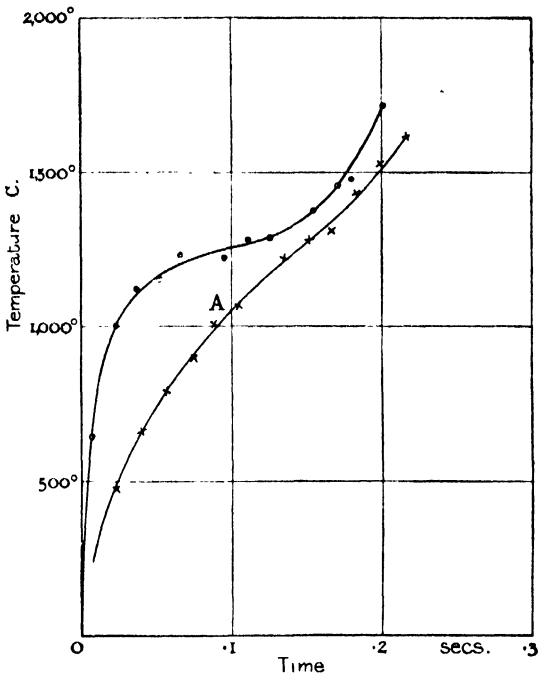


Fig. 5.

pendent of its diameter and, if this be true of our two wires, the difference of temperature between the thick wire and the gas must be 15 times as great as that between the thin wire and the gas, from this cause. Since the thick wire is here 200° C. hotter than the thin, the gas must be about 15° C. hotter than the thin wire—for our purpose an insignificant amount. It is possible, however, that convection currents play a more important part in giving heat to the wire than the kinetic theory in its simplest form would indicate. The effect of such currents is proportional to the surface of the wire, and the thick wire must in consequence receive heat at a greater absolute rate than the thin for the same temperature difference between

* A somewhat similar method has been proposed by Professor Callendar for determining the radiation correction, *Proc. Inst. Mech. Eng.*, 1901.

wire and gas. If convection currents were the predominant factor, the thick wire would receive heat twice as fast as the thin. In this case the thin wire would be about 30° C. colder than the gas at the point *A*. This may be regarded as a superior limit to the error due to time-lag at this point. Taking the assumption that the two wires receive heat at the same rate for the same temperature difference, it follows that when the temperature of the thin wire is rising 1000° C. per second it must be about 12° C. colder than the gas, and this is the constant used in correcting the temperature in Fig. 3.

As regards the use of platinum thermometers in general for the measurement of gas engine temperatures, the comparison of thick and thin wires here described shows that if the temperature is changing at the rate of 1300° C. per second, a wire 0·002 inch diameter will be at least 200° C. hotter or colder than the gas. In a gas engine running at 120 revolutions per minute the mean temperature falls from 1600° to 1000° C. in about $\frac{1}{4}$ of a second, so that wires of this diameter give untrustworthy measurement of temperature, except, perhaps, at the extreme end of the stroke*.

(4) One other source of error remains to be considered, namely, that due to radiation. In consequence of this the wire must always be somewhat colder than the gas, since it has to receive heat at the same rate as it loses heat by radiation. The correction necessary for this may be deduced from the comparison of thin and thick wires described in the last paragraph. At the point *A* the thick wire is getting hotter at the rate of 5000° C. per second. Its diameter is 0·002 inch, hence assuming that the specific heat of platinum at 1000° C. is 0·04, the capacity for heat of the wire is $1{\cdot}72 \times 10^{-5}$ gramme-water unit per centimetre. It is therefore receiving heat at the rate of 0·086 calorie per second, plus any loss due to radiation. It will presently appear that the latter item is small compared with the first. The gas at this point has a temperature of 1250° C. in round numbers. It follows that with a temperature difference of 250° C. between gas and wire, the latter will receive heat at the rate of 0·086 calorie per second per centimetre.

Now the heat radiated by a platinum wire at 800° C. has been found by Bottomley to be about 0·2 calorie per second per square centimetre of surface†. Petavel has extended Bottomley's investigation by comparing the radiation at various temperatures up to 1700° C.‡ Combining the results of the two researches it appears that a wire at 1200° C. will radiate 1·1 calories per second per square centimetre. The wire 0·001 inch diameter therefore radiates 0·0088 calorie per second per centimetre of length. To supply this amount of heat the gas must be 25° C. hotter than the wire. This is the correction applied in Fig. 3 in addition to that for time-lag.

* The smallest wire used by Professor Burstall was 0·0015 inch diameter. The error due to time-lag would be half that of a wire 0·002 inch diameter, but is still of the order of 200° C.

† Bottomley, *Phil. Mag.*, vol. 40.

‡ Petavel, *Phil. Trans.*, A, vol. 191.

The conductivity of the gas increases with the temperature. According to the kinetic theory in its simplest form, the conductivity should be proportional to the square root of the absolute temperature; but it is found, in fact, to vary more rapidly than this, though always less than in proportion to the temperature. So far as I am aware, there are no experimental data for temperatures above 1200° C., and I assume that the conductivity then varies as the square root of the absolute temperature. The temperature difference necessary to maintain the heat lost by radiation is then proportional to the rate of loss divided by such square root. The

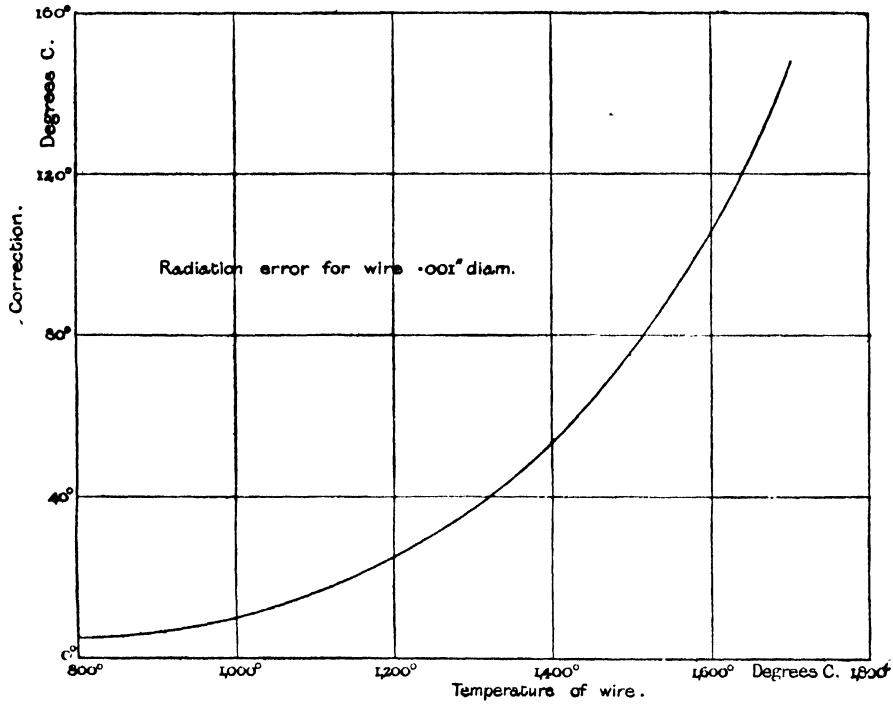


Fig. 6.

resulting values of the difference for wire 0·001 inch diameter, based on Petavel's determination of the amount of radiation, are plotted in Fig. 6. In the explosion of a 9/1 mixture, the results of which are shown in Fig. 2, the maximum temperature reached by the wire is somewhat above its melting point—say 1750° C. At this point the error is 145° C., and the gas temperature 1900° C. If the conductivity varied as the temperature the error would be 120° C., so that it makes very little difference which law is taken.

The radiation correction is proportional to the diameter of the wire. At 1000° C., with a wire 0·002 inch diameter, it is 25° C., or only about 1/10 of that due to time-lag at the point *A* (Fig. 5).

On the "Suppression of Heat" in Gaseous Explosions.

The experiments described above were undertaken largely with the object of finding the cause of the so-called "Suppression of Heat" in explosions. It has long been known that the maximum pressure reached in the explosion of coal gas or hydrogen, and air, is less than two-thirds of that which would be attained by an equal volume of air on the addition of the heat of combustion. A similar phenomenon appears in gas engine indicator diagrams. In most such diagrams the expansion line is somewhat above the adiabatic expansion curve for air, though it is known that much heat is being lost during expansion. But though the fact is well established, there is still much controversy about the cause.

Confine the attention for the moment to closed-vessel explosions, such as those described in this paper, in which the mixture is strictly homogeneous before ignition. At the time of maximum pressure the rate of loss of heat to the walls is just equal to the rate at which the chemical energy of the gas is being converted into thermal energy. This statement requires some qualification in the case of strong mixtures (a point which is dealt with below), but is probably very nearly true of weak mixtures, which possess most interest from this point of view. It follows that at the moment of maximum pressure the gas has not quite attained chemical equilibrium. At some time after maximum pressure equilibrium is reached for practical purposes, and the gas then consists of a mixture of CO_2, H_2O, and inert gas, whose internal energy is all thermal energy and is equal to the heat of combustion less the heat lost up to that time. Messrs Mallard and Le Chatelier and others assumed that this state was reached within a very short time of maximum pressure; and they utilised their records of explosion pressures to deduce the specific heat of the products of combustion. From a study of the curves of pressure during cooling they calculated the loss of heat during burning, and so determined the internal energy of the burnt mixture at or about the time of maximum pressure. This was assumed to be all thermal energy, and so the capacity for heat was determined. The calculation of the loss of heat is open to serious criticism, since the state of the gas during cooling is very different from its state while burning is in progress. But at the time of maximum pressure the correction is still a small one, and the possibly considerable error in it does not affect their broad result, which was that at 1200° C. or over, the specific heat of the products of combustion in the gas engine must be much greater than that at ordinary temperatures.

It has, however, been urged by Dugald Clerk* that the assumption that chemical equilibrium is almost simultaneous with maximum pressure is unjustifiable. The gas may have failed to attain equilibrium in either of two ways. Firstly, the flame may not have spread to every part. There may be discrete portions of gas which are still in their pristine state and in

* *Proc. Inst. C. E.*, vol. 85 (1885), p. 1.

which combustion is actually not started. If I understand their views correctly, all, including Mr Clerk, are agreed that this is not the fact, but that at the time of maximum pressure, if not some time before, combustion is fairly started everywhere in the vessel. My own experiments support this view. Mr Clerk, however, suggests the second alternative, namely, that though the reaction is initiated everywhere, it is not complete. On the analogy of other reactions we know that some interval must elapse between the beginning and the completion of the combination of these gases at any point. Mr Clerk contends that in the case of a weak mixture the interval is a long one—that in places heat is still being produced by the transformation of chemical energy long after the time of maximum pressure, and this though every bit of gas is inflamed before that time; and he seems to consider that even in strong mixtures a considerable proportion of the internal energy is in chemical form at the moment of maximum pressure. He suggests that the "suppressed heat" is to be largely, if not entirely, accounted for in this way.

My experiments do not support such a view as this; they appear to me to prove that even in the weakest mixtures combustion, when once initiated at any point, is almost instantaneously complete. Moreover, they show that the specific heat of the products is very much greater at high temperatures than at low, and the extent of the difference seems to justify the view that it is the main reason of the so-called "suppression of heat." It may be added that this rise in the specific heat is consistent with direct observations of that constant for CO_2, which have been made up to about 800° C., and prove that it increases considerably.

It was suggested above that in the case of strong mixtures maximum pressure does not necessarily mark equality between the rate of loss of heat and the rate of transformation of chemical energy into heat. At the moment of maximum pressure the gas, in addition to possessing untransformed chemical energy and heat energy, is in violent motion, and, moreover, it is very far from being in thermal equilibrium. The kinetic energy of the gas is being changed by viscosity into thermal energy, and, quite apart from want of chemical equilibrium, this might cause the pressure to be stationary, and in actual fact must certainly tend towards that result. The fact that the gas is not in thermal equilibrium would not have any effect upon the pressure observed if it were a perfect gas having a constant specific heat; in other words, the internal energy of a quantity of unequally heated perfect gas is dependent only on its pressure and not on the distribution of temperature. The specific heat of the products of combustion in an explosion is, however, far from constant, being greater at high temperatures than at low, and it is easy to see that the result of this is to make the energy of an unequally heated mass of such products greater than that of the same quantity at the same pressure when the temperature is uniform. Thus, if loss of heat to the walls were arrested at the moment of maximum pressure

after an explosion of a strong mixture there would be a further rise of pressure, due solely to the attainment of thermal equilibrium. I have shown that the differences of temperature amount to 500° C., at least, in a 9/1 mixture; in the present state of our knowledge it is impossible to say how much effect the equalization of these differences might have upon the pressure, but that it would cause some rise there can be no question.

But though I consider that Messrs Mallard and Le Chatelier and their followers were probably right in supposing that the gas was in chemical equilibrium at the time of maximum pressure, or very shortly after, it seems to me that only a rough approximation to the specific heat can be obtained by a study of explosion pressure records only. The uncertainty as to the loss of heat, the great differences of temperature between one part and another, and the violent motion of the gas all conspire to make the results inaccurate; and if the pressure be observed at so long a time after maximum pressure that the gas may be taken to be at rest and in equilibrium, the loss of heat will have become so great as to make the results wholly untrustworthy.

The circumstances of explosion in the gas engine cylinder are, of course, somewhat different from those obtaining in the closed vessel, where the gas before ignition is homogeneous and at rest. In the gas engine the gas and air are usually introduced simultaneously at a high velocity, and the engine is designed so as to ensure, as far as possible, the thorough mixture of the incoming streams. Nevertheless, there is a possibility that the mixture is not quite complete, since from the nature of the case it depends almost wholly upon mechanical agitation and not upon diffusion. The mixture must certainly be homogeneous in the sense that a sample of, say, 100 c.c. from whatever part of the cylinder it be taken, will show the same composition. On the other hand, it is probable that the composition, as shown by a sample very small compared with the general dimensions of the vessel, and yet immensely great compared with molecular dimensions, will have very greatly varying composition according to the precise point from which it is taken. The structure of the gas may perhaps be of a streaky character, consisting of line of rich mixture embedded in a matrix of weaker mixture. Moreover, the gases in an engine are probably in turbulent motion at the time of ignition, and the movement of the flame will be somewhat different from that observed in a homogeneous mixture at rest. The agitation of the gas will certainly accelerate the spread of the flame; the want of homogeneity may, perhaps, retard the combustion at any given point. It is quite unnecessary, however, to suppose that these influences have any substantial effect upon the gas engine indicator diagram in order to explain its peculiarities. These follow at once from the fact, which I think may be considered as established, that the specific heat of the working substance is much greater than that of air, and from the very slow propagation of flame through a weak mixture.

When a fairly rich mixture is exploded in the gas engine cylinder (say, air/gas = 9/1), the ignition being slightly before the end of the compression stroke, the diagram shows a very sharp rise of pressure, followed at once by the falling curve of the expansion stroke. The diagram is almost identical with that which would be given by the sudden addition of the heat of combustion (less a small percentage of loss) to the mixture of CO_2, H_2O, and inert gas, which results from the combustion, followed by expansion with some loss of heat to the walls. The internal energy of such a mixture at 1500° C. is about one and a half times that of the same volume of air at the same temperature and pressure; hence the rise of temperature on explosion will be about two-thirds of that which would have taken place if the working substance had been air instead of the mixture referred to. Moreover, the ratio of the specific heats of the mixture between 1500° and 1000° C. is something less than 1·3. The expansion line will therefore, if adiabatic, lie above the line $pv^{1\cdot3}$ constant. There is no necessity at all to suppose that in this case we have in the cylinder anything but a mixture of CO_2, H_2O, and inert gases in complete chemical equilibrium for at least nine-tenths of the stroke. The natural rate of propagation of flame through such a mixture is high enough to fill the compression space before the piston has sensibly moved; and the time required for this process will be still further reduced by the motion of the gas and (probably) by its high temperature before ignition.

When a very weak mixture is used, the form of the diagram is so far modified that the maximum pressure occurs later in the expansion stroke, the curve corresponding to which first rises and then falls instead of falling for practically the whole of its course. This is exactly what would happen in the explosion of a homogeneous weak mixture, the volume of which is made to increase rapidly while the flame is spreading. Plainly in such a case the rise of pressure due to the spread of the flame might be balanced by the fall due to increase of volume. In the gas engine the volume increases slowly at first, and the pressure then rises at an increasing rate as in the closed vessel. As the middle of the stroke is approached the increase of volume gets more and more rapid, until (at the moment of maximum pressure) the spread of the flame is just able to keep pace with it. Then the piston slows down, and the flame usually overtakes it, and fills the vessel before the end of the stroke. Occasionally, however, there may still be unburnt gas present at the moment of release. Such extremely weak mixtures are, of course, not of much practical importance, because the gas is burnt in a very uneconomical way, much of it being ignited at a low compression.

It would seem that in all cases of importance in gas engine practice the working substance may be treated as a mixture of CO_2, H_2O, and inert gas, in chemical equilibrium to which the heat of combustion, less a small percentage of loss, is added at the beginning of the stroke. The slowness of

propagation of flame will cause the attainment of equilibrium to be more or less delayed, especially in high-speed engines; but the efforts of designers will naturally be directed to timing the commencement of the process and hastening its completion, so that every part shall be ignited under the best circumstances, that is, when the compression is a maximum. The next step in the development of gas engine theory must be to ascertain the properties of this working substance, viz., its internal energy as a function of its temperature and pressure.

I wish to express my indebtedness to Professor Ewing for some valuable criticisms; and I have to thank Messrs G. B. Ehrenborg, of Christ's College, Cambridge, and W. N. Duff, of Trinity College, for assistance in the experimental part of the work.

APPENDIX.

Composition of Cambridge Coal gas.

	Per cent. by volume	O required	Steam	CO_2
H	47·2	23·6	47·2	
CH_4	35·2	70·4	70·4	35·2
Heavy hydrocarbons ...	4·8	22·6	16·0	14·4
CO	7·15	3·6	—	7·15
N	5·4			
Other gases	0·25			
	100·0	120·2	133·6	56·75

The second column shows the volume of oxygen required for complete combustion; the third and fourth, the resultant volumes of CO_2 and H_2O. The analysis was kindly given to me by Mr Auchterlonie, Engineer and Manager to the Cambridge Gas Company.

The composition of the "heavy hydrocarbons" is somewhat uncertain. One hundred volumes of gas require 576 volumes of air for complete combustion. One hundred volumes of gas burnt in 900 of air give about 133 volumes of steam, 57 of CO_2, and 780 of inert gases; assuming that there is no dissociation.

24

A RECORDING CALORIMETER FOR EXPLOSIONS.

[' PROCEEDINGS OF THE ROYAL SOCIETY," A. Vol. LXXIX, 1906.
Communicated by Professor H. L. CALLENDAR, F.R.S.]

THE determination of the rate of loss of heat to the walls of a vessel after an explosion within it is a matter of considerable scientific interest and of practical importance. Hitherto such determinations, if we except the recent work of Dugald Clerk on the loss of heat in the gas engine cylinder, have been based upon a study of the fall of pressure during the cooling of the gases after the explosion. From the pressure the mean temperature can be deduced, and thence, if the specific heat is known, can be found the rate of heat-loss at any moment. Such a calculation is, however, obviously unsatisfactory, because the only available values of the specific heat of gases at temperatures above 1500° C. are based upon explosion experiments, and involve doubtful assumptions as to the amount of loss before combustion is complete. Some means of determining the loss of heat at any instant without any knowledge of specific heat is therefore essential, both for finding the law of cooling of hot gases confined in a closed vessel and for placing on a satisfactory basis the specific heat values obtained from explosion experiments. I have devised a simple means of doing this which appears to be capable of considerable accuracy. It consists essentially in lining the explosion vessel as completely as possible with a continuous piece of copper strip and recording the rise of resistance of the copper strip during the progress of the explosion and the subsequent cooling. Knowing the temperature of the copper and its capacity for heat, the heat that has flowed into it from the gas may be calculated from the resistance, certain corrections being applied for the heat which the copper has lost to the insulating backing.

Up to the present I have only used the apparatus for the investigation of the loss of heat after an explosion of coal gas and air, but it might, I think, be applicable, with certain modifications, to finding the heat-loss during and after the combustion of solid explosives.

The explosion vessel is shown in section in Fig. 1, p. 392. It consists of a cast-iron cylinder 1 foot in diameter and 1 foot in length on to which are bolted two end plates. The cylinder was first completely lined with wood 1/4 inch thick, and the end plates covered with pieces of cork. Thirty-nine turns of copper strip of the quality used for electric lighting purposes, and of a high degree of purity, were then wound on the inside of the curved portion,

a clearance of about 1/20 of an inch being left between successive turns. The strip was 1/4 inch wide by 1/25 inch thick. The ends of this piece of strip were brought to terminals outside the vessel. The end plates were similarly covered with parallel pieces of copper strip of the same dimensions, as shown in Fig. 2, the ends being brazed to connecting pieces. The strips on the ends were electrically connected outside the vessel to the strip in the

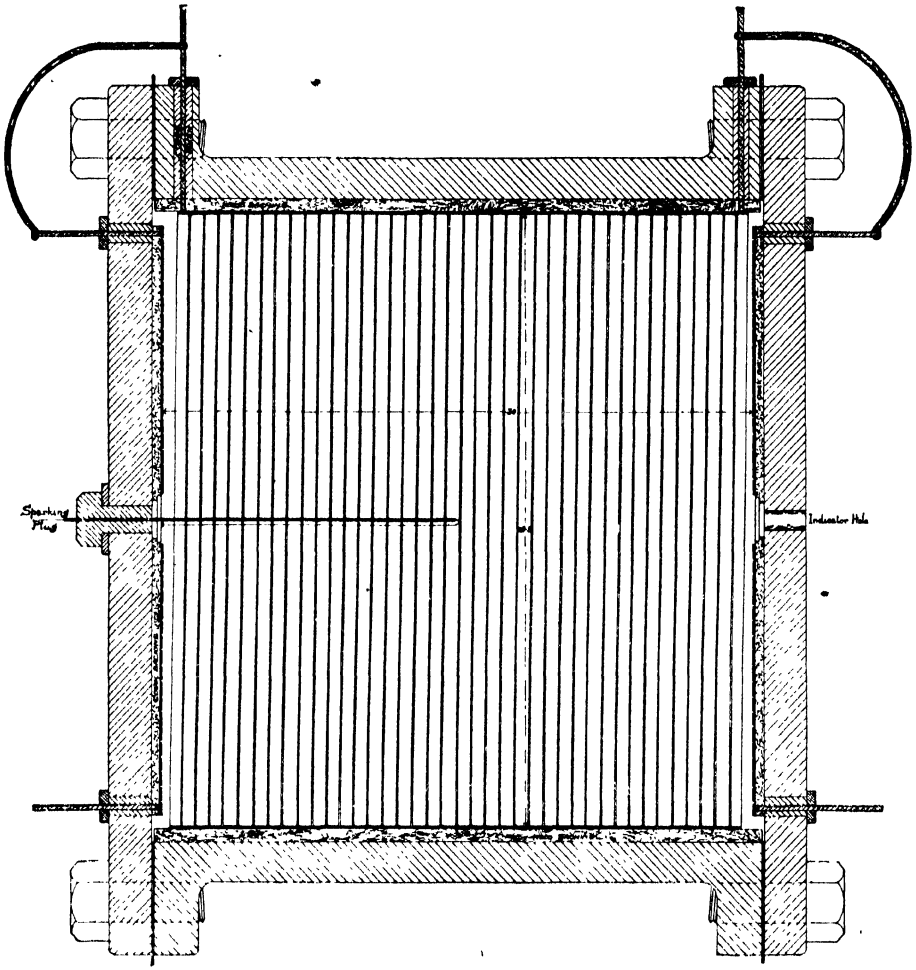


Fig. 1.

cylindrical part. The whole when put together formed an explosion vessel having a capacity of about 0·684 cubic foot which (except for the uncovered portions on the ends where the cocks, etc., came through) was completely lined with an electrically continuous length of copper having an approximately uniform section of 1/100 of a square inch. For recording the pressure I used an optical indicator, consisting of an iron piston which was forced by the pressure against a piece of straight spring held at the ends. The dis-

placement of the spring tilted a mirror about a fulcrum, and the mirror cast an image of a fine hole illuminated by an arc lamp on to a photographic film carried on a revolving drum. This indicator was repeatedly calibrated by dead weights, and I think its readings are to be trusted to within 1 per cent. of the maximum reached. The mixture was fired by an electric spark at the centre of the vessel, and it was at atmospheric pressure and temperature before firing.

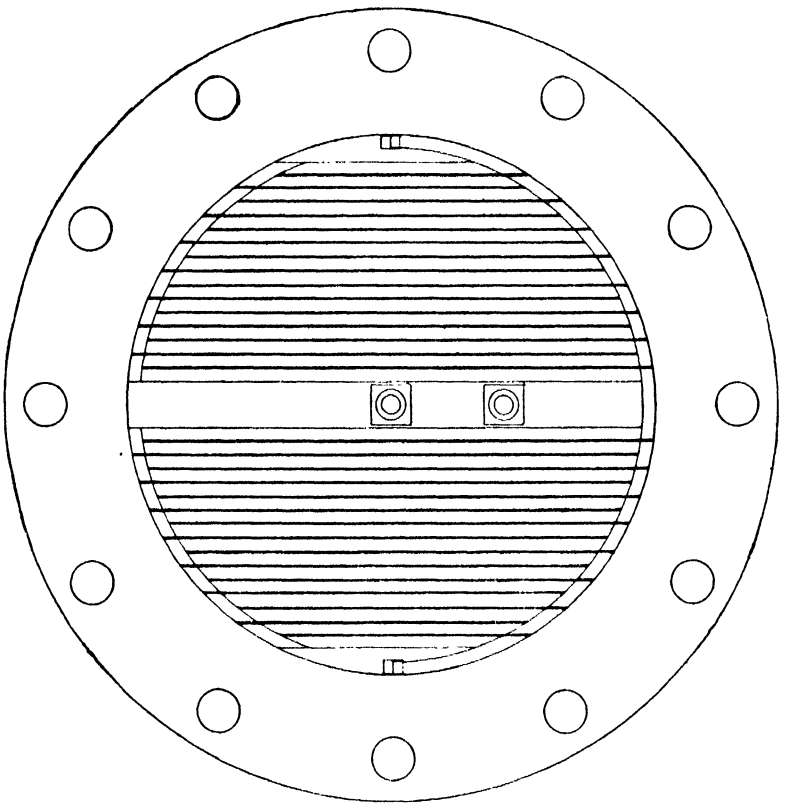


Fig. 2.

For recording the temperature of the strip, it was connected as shown in Fig. 3, p. 394. G is a d'Arsonval galvanometer having a stiff suspension and a periodic time of about 1/15 of a second. Its resistance is 3·2 ohms. It is placed in series with the copper strip and with a resistance R of about 1/4 of an ohm. A constant current of about 8 ampères is maintained in the strip by means of the battery B_1 (50 storage cells) and the bank of lamps L. A constant current of about $4\frac{1}{2}$ ampères is maintained in the resistance R by means of the battery B_2 (six storage cells) and an external resistance R_1 the current being in such a direction that the E.M.F. at the terminals of R opposes that at the terminals of the strip. The resistance of the strip S being 0·14 ohm at the temperature of the room, the result of this arrangement

is that there is an approximate balance of electromotive force in the galvanometer circuit before the explosion, and the galvanometer then shows little or no deflection. When the explosion takes place the copper strip S is heated and its resistance rises, and since the current in it remains constant during the short time occupied by the cooling of the gas to ordinary temperatures, the potential at the terminals of the strip rises by an amount proportional to the increase of resistance or to the increase of temperature. Since the potential at the terminals of the resistance R

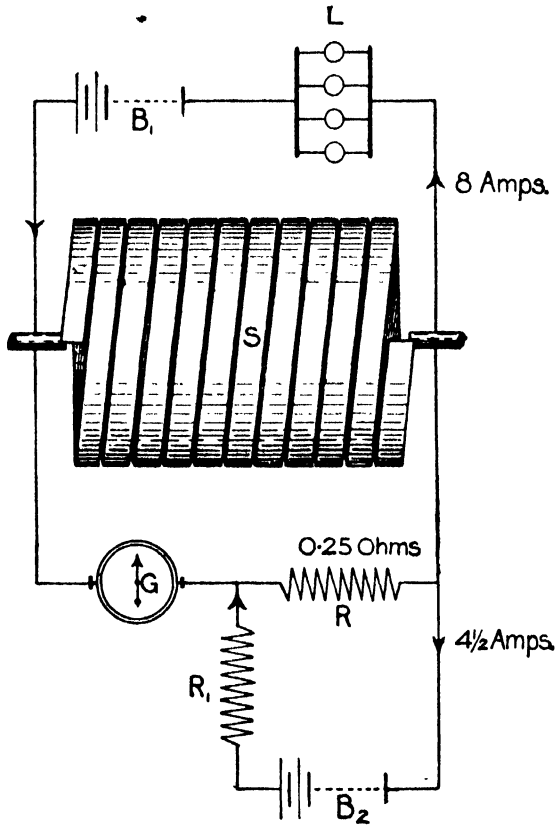


Fig. 3.

remains constant, except for the small disturbance due to the passage of the galvanometer current, the galvanometer deflection from the reading just before the explosion will be proportional to the rise of potential between the terminals of the strip or to the rise of temperature. The mirror of the galvanometer reflected on to the moving film an image of the same small hole as was used for recording the change of pressure, and a simultaneous record was thus obtained of the change of temperature of the strip and of the pressure in the vessel.

One such record is shown in Fig. 4 (Plate I). Curve A is the pressure

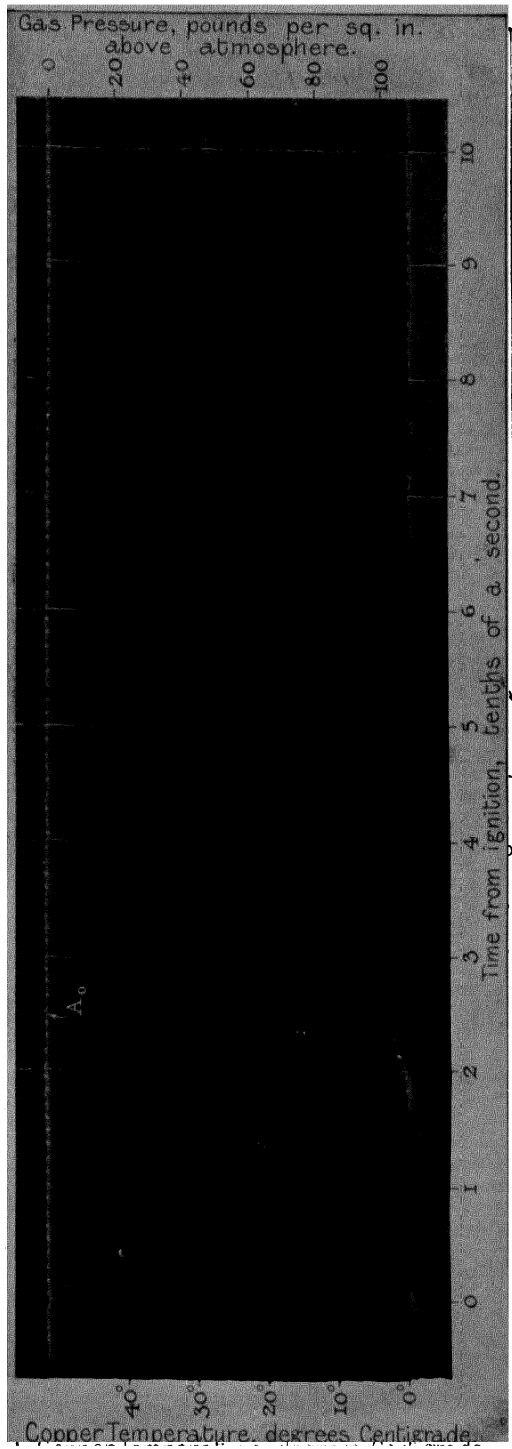

Gas Pressure, pounds per sq. in.
above atmosphere.
0
20
40
60
80
100
Time from ignition, tenths of a second.
0
1
2
3
4
5
6
7
8
9
10
A₀
Copper Temperature, degrees Centigrade.
40°
30°
20°
10°
0°

Fig. 1.

reckoned downwards from the atmosphere line A_0. Curve B is the galvanometer deflection reckoned upwards from the zero line B_0*. The galvanometer is thrown into slight oscillation, owing to the very rapid change of temperature which occurs when the hot gas first comes into contact with the strip. The white dots on the lines are due to the fact that an alternating current arc was used for illuminating the hole; they are very useful in measuring the diagrams, because they enable corresponding points on the two curves to be identified correct to about 1/500 of a second. On the pressure curve A, 1 mm. deflection corresponds to a pressure of 1·80 lbs. per square inch†. The barometer in this case stood at 753 mm., equivalent to 14·6 lbs. per square inch. The temperature just before explosion was 15° C., or 288° absolute. Allowing for the contraction of volume of 3 per cent., which occurs in the combustion of a mixture of one part of Cambridge coal gas and seven parts of air, it follows that 1 mm. on the pressure diagram corresponds to a rise of temperature of 36°·6 C. On the curve B_1, showing the galvanometer deflection, 1 mm. is equivalent to a rise of resistance in the strip of 0·00047 ohm, or to a mean temperature rise of 0°·83 C., assuming a temperature coefficient of 0·00428. The total weight of strip is 2870 grammes, and the specific heat is taken as 0·093; the quantity of heat corresponding to 1 mm. on the galvanometer curve is, therefore, 222 calories. The values assumed for the heat capacity and temperature coefficient are probably very nearly correct, since the strip is nearly pure copper; but it is difficult to determine them directly with sufficient accuracy. In order to confirm their correctness as far as is necessary for the present experiment, a portion of the strip was wound on a wooden frame and was electrically heated by passing a current of about 350 ampères through it for 1·4 seconds. The amount of energy put into the strip in this process was measured by a ballistic wattmeter, the suspended coil of which was placed (with a high resistance in series) as a shunt across the terminals of the strip, while the fixed coils carried the current. The resistance was measured just before and just after the passage of the current. In this manner a direct relation was established between the energy put into the copper and the rise of resistance produced thereby. It was found to be the same, within 1 per cent., as that deduced from the assumed temperature coefficient and specific heat.

Of the heat which passes into the copper, some part is lost to the wooden backing behind it, and it is the balance only which is directly measured in the diagram. The percentage of heat so leaking out is a correction which increases from less than 1 per cent. 0·1 second after firing up to about

* The doubling of the zero line is due to the fact that the mixture failed to ignite at the first attempt. The current was left on for a minute or so while this was being investigated, and slightly heated the strip. At the second (successful) attempt the position of the zero line had therefore shifted upwards to an extent corresponding to the rise of temperature.

† All measurements refer to the original film. The reproduction is approximately three-quarters of the original size.

20 per cent. 1 second after firing. In order to determine the amount of this correction, recourse was had to the method of electrical heating described above. A current of about 350 ampères was passed through the strip in the cylindrical part of the vessel for 1·4 seconds, and the resistance was determined immediately after the current ceased. The amount of energy put in under these circumstances was found to be 17·5 per cent. greater than the heat accounted for by the rise of temperature in the copper strip. The loss of heat to the backing was therefore 0·175 of that which had gone into the copper. The loss of heat depends on the manner in which the temperature of the copper rises, which is not quite the same in the explosion vessel as in the electrical heating. In the latter case the rate at which work is done on the copper is nearly constant, and the temperature rise is nearly proportionate to the time, whereas after the explosion the temperature rises at first very rapidly and then remains more nearly constant. By the methods of the Fourier analysis of the conduction of heat, however, if the loss of heat for any one surface variation of temperature is known it can be calculated for any other. The surface temperature of the wood is the temperature of the copper. We determine experimentally the flow of heat into the wood when its surface temperature is made (by electrical heating) to vary as a linear function of the time. From the result is calculated the heat-flow when the temperature varies in the manner given by Curve *B* in the diagram. Details of these and other calculations are given at the end of the paper.

In addition to the heat which passes into the copper and, *viâ* the copper, into the backing behind it, heat also goes into those parts of the walls which are not covered. The total exposed area covered by the copper strip is 3200 sq. cm. The total surface of the vessel is 4000 sq. cm. There are therefore 800 sq. cm., or about 1/5 part of the total, uncovered. Of the uncovered area, however, approximately 580 sq. cm. of it is the narrow strip left for insulation between the adjacent turns or pieces of copper strip. These turns are separated by 1·15 mm. on the average, and since the thickness of the strip is 1 mm., any gas that reaches the backing which is exposed between the turns must part with the greater part of its heat to the edges of the strip. This part of the uncovered area may be regarded as protected by the copper, though not actually covered by it.

The balance of 220 sq. cm. consists mainly of two large patches on the end-plates, and of an annular space round the copper on each end-plate. It will receive heat at approximately the same rate, on the average, as does the copper. The whole heat which the gas has lost, therefore, exceeds that which has gone into the copper in the ratio 4000/3780, or by 6 per cent. This correction is the most uncertain point in the whole experiment. It is under-estimated, because some heat, no doubt, finds its way between the turns. But the error on this account cannot be very large.

As a preliminary test of the accuracy of the copper strip calorimeter,

I have calculated the heat accounted for by it at points so far down the cooling curve that the specific heat of the gas may be considered as known. We may take, for example, a point about 1 second after ignition, when the gas temperature is 545° C. At this point the ordinate of Curve *B* (after smoothing the oscillations) is 35·3 mm., equivalent to 7850 calories. The correction to be added for loss of heat to the backing (see Appendix 2) is here 20 per cent.; the total heat which has passed into and through the copper is therefore 9420 calories. Multiplying this by the factor 1·06, we get 10,000 calories as the heat which has gone into the walls.

Before firing, the mixture was of the following composition, approximately:

Cambridge coal gas	0·082 cub. ft.	=	12·7 per cent.
Air (including some water vapour)	0·565 ,,	=	87·3 ,,
Total	0·647 ,,	=	100·0 ,,

the volumes being reckoned at 0° C. and 760 mm.

The calorific value of the gas, determined in a Boys calorimeter, was found to be 670 B.TH.U., or 170,000 calories per cubic foot. The heat produced in the explosion is therefore 14,000 calories, if the products are all cooled to about 15° C. If we suppose the cooling to be stopped at 545° C., the heat evolved will be less by the latent heat of the steam produced (about 30 grammes per cubic foot of coal gas) and by the amount evolved by the gaseous constituents in cooling from 545° to 15° C. The latent heat item is $600 \times 30 \times 0{\cdot}082 = 1480$ calories. For the other item we must have recourse to the results of Holborn and Austin, who have determined the specific heat of steam, CO_2, and air by direct heating at constant pressure up to 800° C. These results are shown in the second column of the following table, which also gives the composition of the products present in the explosion vessel and the heat evolved by each in cooling through the range named. The specific heats are expressed in calories per cubic foot at constant volume, and are the mean values between 15° and 545° C. The amount of steam is not accurately known, since no measurement was made of the degree of saturation of the gas and air before combustion; moreover, the steam does not all condense at the lowest temperature of 15° C., and some of it is, therefore, not cooled through the full range as gas. But the error on either account is very small.

	Amount	Specific heat	Heat evolved in cooling 530° C.
Co_2 ...	0·046	10·7	260
Steam ...	0·118	8·4	527
N and O ...	0·460	6·3	1540
Total ...	0·624		2327

Adding the heat of condensation (1480 calories) we get 3807 calories evolved in cooling the products of the combustion from 545° to 15° C. The heat evolved by the products of the explosion in cooling down to 545° C. is then 14,000—3807, or say 10,200 calories. This agrees very closely with the heat found by the copper strip calorimeter, viz., 10,000 calories.

As a further check, we may make a similar comparison at the end of half a second from the time of firing. At this point the gas temperature obtained from the pressure diagram is 840° C., which is just beyond the limit of Holborn and Austin's specific heat determinations. At this point we have:

Heat in copper strip, $30\frac{1}{4}$ mm.	6720	calories
Heat lost to backing, 12 per cent. of the above	810	,,
Heat to uncovered part, 0·06 × 7530 ...	450	,,
Total heat lost to walls	7980	,,

The heat evolved in cooling the products from 840° to 15° C. and in condensing the steam is 5180 calories. The heat evolved in cooling the burnt products to 840° C. should, therefore, be 14,000 − 5180 = 8820 calories. According to this estimate, therefore, the copper strip calorimeter is about 800 calories, or 10 per cent. wrong.

There is, however, a considerable possible error in the estimate derived from the calorific value of the gas at this point. In the first place that calorific value may itself be in error by as much as 2 per cent., or 280 calories. Again, Holborn and Austin consider that their measurements of specific heat may be in error by as much as 3 per cent., equivalent to a possible error of 120 calories in this experiment. Finally, it is possible that some of the gas is still unburnt. There are several small passages connected to the vessel, and there is a small annular clearance space between the end-plate and the end of the wooden lining of the cylindrical part. These spaces amount in the aggregate to only 1 per cent. of the whole volume of the vessel, but they are so situated that in the progress of the flame unburnt gas is compressed into them. Thus at maximum pressure these places will be filled with unburnt mixture at a pressure of about 7 atmospheres. On account of the large cooling surface in these places it is probable that the flame does not penetrate into them at once; the gas trapped in them burns slowly, coming out of its retirement into the main body of the vessel as the pressure falls, and then burning. At time 0·5 second the pressure is 3 atmospheres, and there may still be as much as 2 or 3 per cent. of the mixture remaining unburnt in the spaces referred to. Since 3 per cent. of the mixture represents 420 calories, this goes far to account for the discrepancy between the observed and calculated values of the heat-loss.

These experiments establish, I think, the substantial accuracy of this form of calorimeter. The most uncertain feature is the allowance to be

made for the uncovered area. This has been taken as equal to 6 per cent. of the covered area, which is an inferior limit. The close agreement of the calculated and observed values of heat loss, 1 second after firing, shows that this allowance is not far wrong. The other correction, that of the loss of heat to the backing, though far larger in amount, is capable of pretty accurate determination. It rests really upon a comparison of the rise of resistance produced by the application of the same amount of work to the copper strip when exposed to the air and when in contact with the backing. It is independent of the absolute meaning of the ballistic wattmeter readings and of any knowledge of the specific heat or temperature coefficient of the copper.

The Law of Heat-Loss.

The full line curve, Fig. 5,, p. 400, shows the heat-loss per square centimetre of surface in terms of the time. This is curve *B* of Fig. 4 (Plate I), with the oscillations smoothed out and corrected for the loss of heat to the backing. It may be noted that only the absolute values on this curve are affected by the uncertainty as to the effective area of the copper. The comparative heat losses at different times are no doubt accurately shown. This determination of heat loss is, of course, quite unaffected by the uncertainty as to when combustion is complete.

The loss of heat begins about 1/20 second after ignition, when the flame first comes into contact with the copper. At first the loss goes on at a very great rate, and by the time maximum pressure is reached (when the flame is in contact with the whole surface of the strip and losing heat to every part of it), about 1700 calories, or 12 per cent. of the gross heating value of the gas, has passed into the walls. The rate of loss of heat at this point is about 10 calories per second per square centimetre, and the mean gas temperature is 1760° C. At 0·2 second from ignition the rate of heat-loss is about 3½ calories per second, and the mean gas temperature is 1300° C. The mean temperature is reduced in the ratio 0·74 between these two points, the product of mean temperature and pressure is reduced in the ratio 0·55, but the rate of loss of heat at 0·2 second is only 1/3 of what it is at maximum pressure.

A study of these figures brings into prominence one aspect of cooling after an explosion, which has, I think, hardly received the attention it deserves. The immediate effect of the explosion is to raise the temperature of the mass of gas to a mean value of about 1800° C., and the boundary of this hot body is suddenly cooled by contact with the walls to about 15° C., which surface temperature remains practically constant during the time of cooling. The heat passing into the walls immediately after the flame touches them is drawn almost wholly from the surface layer of gas in contact with them, and as it has to be transferred so short a distance its flow is at first extremely rapid. This surface layer, however, soon parts

with its heat, and further supplies have to be drawn from the inner portions of gas, the cool surface layer now acting as heat insulation. Thus the rate of flow rapidly diminishes.

The cooling of the mass of flame may, in fact, be expected to follow substantially the same law as that of a solid body similarly treated. The Fourier analysis shows that if the surface of a body of large extent uniformly heated to temperature θ be suddenly cooled to temperature zero, and the surface temperature then maintained constant, the heat-loss per

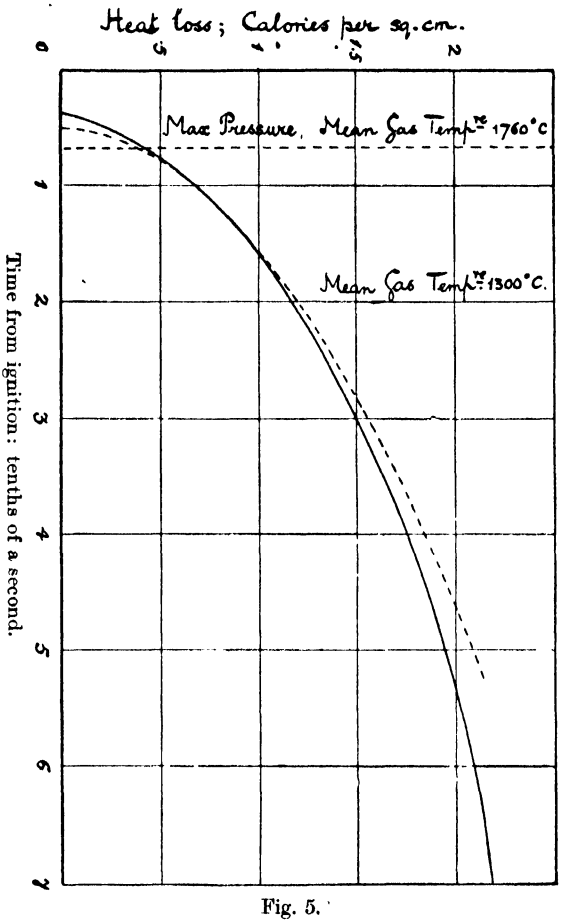


Fig. 5.

unit of surface up to any time t will be at first proportional to the square root of the time, being equal to $2\theta\sqrt{\frac{kct}{\pi}}$, where k is the thermal conductivity and c the thermal capacity per unit volume of the solid. Strictly true of a body of infinite extent having a plane boundary, this law holds approximately for finite bodies so long as the central portions have not been much cooled. It is interesting to enquire how far the cooling of the flame follows the same law.

On Fig. 5 I have shown in dotted line a curve whose ordinate is proportional to the square root of the time, reckoned from a point 0·05 second after ignition. This point was chosen because loss of heat does not begin at all points of the strip at the same time; the flame first touches the strip about 0·035 second after ignition, and it is not in contact with every part until 0·065 second after ignition. It will be seen that except for the first fiftieth of a second the two curves agree well up to about 0·3 second, when the heat-loss becomes considerably less than if it followed the square root law. This would equally happen with a solid body of finite dimensions when the cooling had penetrated to the interior; the square root law only holds accurately for a body of infinite dimensions.

The Fourier analysis is strictly applicable only to a solid body in which heat transfer takes place only by conduction, and in which the thermal capacity and conductivity are independent of temperature. In the gas, the transmission of heat takes place mainly by convection. The effects of convection may probably be mathematically represented in very much the same way as those of conduction; convection consists in the bodily transfer of gas from a place of high temperature to a place of low temperature, and the corresponding rate of transmission of heat will, for a given amount of agitation, be roughly proportional to the temperature gradient. There will be an effective conductivity k, depending mainly on the state of motion of the gas. But though in a general way the loss of heat may be expected to go on in the gas according to much the same laws as in the solid body, the differences between the two cases are so great that there is not much *a priori* ground for expecting exact quantitative agreement. The closeness with which the loss of heat after the explosion follows the square root law may be to some extent accidental, and hardly in itself justifies making quantitative applications of the same kind in other cases. It seems to me, however, to justify drawing a few general inferences. The most important is that the rate of loss of heat from a hot gas is not only a function of the mean pressure and temperature, it really has very little to do with these quantities. It is dependent essentially on the temperature gradient in the surface layer just within the walls, and this, in any practical case, is mainly determined by the history of the surface layer. No valid inference as to the rate of loss of heat after an explosion can be drawn from experiments on heat conduction or convection in a gas under steady conditions of temperature. Nor can it be assumed that two masses of gas, in similar vessels and at the same pressure and mean temperature, will lose heat at the same rate, though that assumption is often made. Another point is that the rate of heat-loss may be expected to increase considerably with the density of the hot gas, the temperature and state of agitation being the same. According to the solid body theory, the rate of loss of heat is proportional to $\sqrt{kc}$. Now in a gas c, the thermal capacity per unit volume is proportional to the density. Moreover k, the effective con-

ductivity, may also be expected to increase more or less in proportion to the density. For k represents the rate of heat transfer by the motion of the hot gas, and the quantity of heat carried by a given volume of gas at a given temperature is proportional to the density. It would not be surprising if the loss of heat after an explosion were found to be proportional to the density of the gas in spite of the fact that experiments under steady conditions of temperature show that the rate of removal of heat from a hot body by gas surrounding it does not increase in proportion to the pressure of the gas. I think it possible that a good many hitherto obscure phenomena connected with explosions may be explained by considerations such as these*.

Specific Heat Determinations.

By deducting from the total calorific value of the gas the heat lost up to any moment as determined by calorimeter, the internal energy (thermal and chemical) in the gas at that moment can be obtained. Since the mean temperature of the gas is known a curve of specific heats at all temperatures up to 1800° C. could thus be obtained. To use Dugald Clerk's phrase, however, these would be "apparent" specific heats, involving the assumption that all the gas is burnt. Now, though that assumption may very

* *February* 11, 1907. Since writing the above I have had the advantage of reading two papers by Lord Rayleigh (*Phil. Mag.*, March, 1889). In one of them he investigates mathematically the law of cooling of a mass of air by conduction, when the surface temperature is suddenly changed, the air being enclosed in a spherical enclosure of constant volume. The Fourier analysis is applied with the modifications necessitated by the fact that when any portion of the air is cooled below the temperature of the remainder it contracts and thereby causes all the other parts to expand, so lowering their temperature in a manner independent of conduction. It is shown that if the radius of the sphere be 16½ cm. the excess of the mean temperature of the air over that of the surroundings will fall to half its initial value in 26 seconds. In the second paper, experiments are described in which this result is verified, by observing the rate of fall of pressure in a mass of air, contained in a spherical enclosure, after it has been suddenly heated by adiabatic compression.

The experiment is closely analogous to observing the fall of pressure after an explosion, with the difference, however, that the range of temperature is only a fraction of a degree Centigrade as compared with 1000° or 2000° C. The mathematical analysis only applies when the range of temperature is small. It is, however, interesting to compare the rate of fall of temperature in the two cases. My explosion vessel happens to be of about the same dimensions as the sphere in Lord Rayleigh's experiments, and it will be seen from the pressure-curve (Fig. 4, Plate I) that the pressure falls to about half of its maximum value in 0·38 second, that is, in about 1/70 of the time taken for a similar amount of cooling in Lord Rayleigh's experiments. This result must be ascribed mainly to the effects of convection after the explosion; for neither the thermal conductivity nor the thermal capacity (which is the other constant determining the rate of fall) is very greatly different in the two cases. It is known that the gases are set into violent vibratory motion by the explosion, and it seems possible that the greatly enhanced "effective conductivity" is due to this fact. Possibly, radiation may also play some part in accelerating the cooling.

The curve of heat-loss, calculated by Lord Rayleigh for the spherical mass of air, agrees fairly well with a curve whose ordinates are proportional to the square root of the time, until the temperature has fallen to about 0·6 of its initial value. The fall of temperature in half that time is very nearly 0·75 of the fall occurring in the whole time. If it went as the square root of the time the ratio would be $2^{-\frac{1}{2}}$ or 0·71. After the explosion the temperature falls to 3/5 of its maximum value in about 0·3 second from the time of ignition, and, until then, the curve of heat loss is closely similar to Lord Rayleigh's curve, the deviations from the square root law being in the same direction and of about the same amount.

likely be true for a vessel with a smooth surface and without pockets, it is certainly not true in the vessel with which I experimented, and in which, as indicated above, the gas trapped in the pockets is a serious proportion of the whole. I will, therefore, defer all discussion of the information which this calorimeter may be expected to give as to the variation of specific heats, until I have fully investigated the question of delayed combustion.

I desire to acknowledge the very able assistance that I have received in this investigation from Mr L. du B. Hugo, who was until lately a student and assistant demonstrator in the Engineering Laboratory, Cambridge. He carried out most of the experimental work under my direction, and made several suggestions which materially facilitated it. I have also to thank Professor Callendar for the kind interest which he has shown in the work.

APPENDIX.

1. *Temperature Distribution in the Copper Strip.*

Different parts of the length of the strip receive heat at different rates, but the total heat given to any element of length is equal to the rise of temperature of that element multiplied by its thermal capacity. Since the strip is of uniform section from end to end, the thermal capacity of an element is proportional to its length. For the same reason the increase of resistance of an element is proportional to the rise of temperature multiplied by the length. The rise of resistance of the whole length, therefore, measures the heat given to the whole length, provided that the temperature may be assumed uniform over the section.

This assumption can be readily justified. The thermal conductivity of copper may be taken as 0·9. At maximum pressure heat is flowing into the copper, according to the curve, Fig. 5, at the rate of about 10 calories per square centimetre per second. The corresponding temperature gradient just within the surface of the copper is 11° C. per centimetre. If this temperature gradient were the same throughout the thickness of the strip, the temperature on the side away from the gas would be 1·1° C. less than on the exposed side. As a matter of fact, the gradient is less at points within the strip, and it is apparent that there can be no serious difference of temperature between one part of the section and another. A small deviation from uniformity is without effect, the rise of resistance being, to a first approximation, proportional to the mean temperature across the section.

2. *Determination of Heat Lost to Backing.*

If the temperature of the plane surface of an infinite solid vary according to the equation $\theta = F(t)$, where t is the time, then the Fourier analysis shows that the total amount of heat that has passed into the solid at time t is

$$2\sqrt{\frac{kct}{\pi}}\left\{F(t) - \tfrac{1}{3}tF'(t) + \tfrac{1}{5}\frac{t^2}{2!}F''(t) - \ldots\right\},$$

where $F'(t)$, $F''(t)$, ..., are the derived functions of $F(t)$ and k is the thermal conductivity and c the thermal capacity per unit volume of the solid.

In our case the solid is the wood backing, and the temperature θ is that of the copper strip. In consequence of the low thermal conductivity of wood, the temperature changes are confined to parts near the copper, and the backing may be treated as infinitely thick, at any rate for the purpose of finding a correction. The heat accounted for in the copper is $CF(t)$, where C is the thermal capacity of the copper strip per unit area. The heat lost to the backing is to that in the copper in the ratio

$$\frac{2}{C}\sqrt{\frac{\overline{kct}}{\pi}}\left\{1-\tfrac{1}{3}t\frac{F'(t)}{F(t)}+\tfrac{1}{5}\frac{t^2}{2!}\cdot\frac{F''(t)}{F(t)}-\ldots\right\}. \qquad \ldots\ldots(1)$$

The values of $F(t)$, $F'(t)$, ..., etc., are obtained at once from the curve (B in Fig. 4, Plate I) showing the rise of temperature of the strip in terms of the time. In order to obtain the percentage of heat-loss, therefore, it is only necessary to find the constant $\frac{2}{C}\sqrt{\frac{\overline{kc}}{\pi}}$ in some particular case in which $F(t)$ and the loss of heat are both known.

For this purpose, that part of the strip which is in the cylindrical surface was placed in series with a fuse of No. 18 copper wire, a storage battery of 100 cells, and the series coils of a ballistic wattmeter. The suspended coil of the wattmeter, in series with a non-inductive resistance of 8000 ohms, was placed as a shunt across the strip. On closing the switch in the battery circuit, the fuse allowed a current of about 350 ampères to pass for about 1·4 seconds before melting. A photographic record of the current was taken with a quick period galvanometer, and it was found to rise within 1/50 second to 365 ampères, it then fell in the course of 1·1 seconds to 340 ampères. Arcing then commenced at the fuse, and the current rapidly fell, ceasing altogether at 1·4 seconds.

The wattmeter was calibrated in the ordinary way with steady currents. Its period of oscillation was 9·77 seconds. The resistance of the strip was measured in the ordinary way at definite intervals after the current had ceased. From the observations of resistance a curve of cooling was obtained and the resistance immediately after the breaking of the fuse was thus calculated. This correction for cooling was small. A large number of observations gave the following results :

Work done on strip, as measured by wattmeter ...	3677 calories
Weight of copper	1950 grammes
Rise of temperature of copper	17·2° C.
Heat of copper, 17·2 × 1950 × 0·0935 ...	3135 calories
Heat lost to backing, 3677 − 3135 ...	542 ,,

The heat lost under these conditions is, therefore, 17½ per cent. of the heat in the copper. During the passage of the current, work is done on the strip at a nearly uniform rate for 1·1 second, and then at a diminishing

rate for another 0·3 second. The heat-loss will be very nearly the same as though the temperature had risen at a uniform rate for the whole time. Putting $t = 1{\cdot}4$, $F(t) = 17{\cdot}2$, $F'(t) = 17{\cdot}2/1{\cdot}4$, $F''(t) = 0$, etc., in the expression (1) above, the expression for the fraction of heat lost in this case becomes

$$\frac{2}{C}\sqrt{\frac{1{\cdot}4kc}{\pi}} \cdot \frac{2}{3} = 0{\cdot}175,$$

whence

$$\frac{2}{C}\sqrt{\frac{kc}{\pi}} = 0{\cdot}22.$$

In calculating the heat-loss at different points after the explosion, this value of the constant is assumed, and the values of $F(t)$, $F'(t)$, ..., etc., are taken from the curve of copper temperature (B in Fig. 4, Plate 1). For example, at $t = 1$, $F(t) = 35$ mm., $F'(t) = 7{\cdot}5$ mm. per second, $F''(t) = -2{\cdot}8$, and so on, the rest of the terms being unimportant. The percentage of heat loss obtained by substituting these figures in expression (1) is approximately 20 per cent.

The specific heat of the strip was determined by the same ballistic wattmeter method, a weighed quantity of strip being wound on a wooden frame, so that heat loss only takes place to the surrounding air. The rise of temperature produced by blowing a No. 18 copper fuse in series with this strip was 18·2° C., assuming a temperature coefficient of 0·00428. The corresponding value of the specific heat was 0·0935, deduced from the wattmeter reading. A correction of 2½ per cent. had to be applied to the temperature rise for the cooling which took place before the resistance could be measured. It should be added that in both ballistic wattmeter measurements the reading was corrected for the fact that the period for which the current passed (1·4 seconds) was comparable with the period of the instrument (9·77 seconds). The correction was applied on the assumption that a constant torque acted on the suspended coil for 1·4 seconds, and it amounted to an addition of 2½ per cent.

It was found that the amount of energy put into the copper during the blowing of a copper wire fuse of given size was remarkably constant, never differing by more than 2 or 3 per cent. from the mean. This method of determining specific heats is, I think, capable of considerable accuracy, and is certainly convenient.

25

ON RADIATION IN A GASEOUS EXPLOSION.

["Proceedings of the Royal Society," A. Vol. LXXXIV, 1910.]

In the first report of the British Association Committee on Gaseous Explosions, attention was drawn to the probable importance of radiation in determining the rate of cooling of the mass of hot gas produced by igniting an inflammable mixture in a closed vessel. In the second report reference was made to some experiments which I had made on the effect of coating the walls of the explosion vessel with bright tin-foil. It was found that if a mixture of coal gas and air of given composition were exploded in a vessel thus lined, the maximum pressure reached was nearly the same as that given by an identical mixture when the tin-foil lining was blackened, but the rate of cooling was decidedly less.

Though the experiment established a substantial difference between the rates of heat-loss in the two cases, it was hardly sufficient to justify giving quantitative results, nor was it absolutely conclusive as to the cause of the difference, though it seemed highly probable that it was due to the difference in the power of the walls to absorb radiant energy.

I have recently completed a more elaborate series of tests, which I think are sufficiently accurate and conclusive for publication. Instead of coating the inside of the vessel with tin-foil, I have had it plated with silver, which has in successive explosions of the same mixture been first highly polished and then blackened. As in the vessel lined with tin-foil, the cooling is slower when the walls are reflecting, but the difference is greater with the silver-lined vessel, amounting to about 50 per cent. during the first quarter of a second. Moreover, there is a difference in maximum pressure, amounting to about 3 per cent. Records have also been obtained of the change of temperature, during the explosion and subsequent cooling, of a bolometer placed outside a window of fluorite in the wall of the vessel, and these have shown that for one-tenth of a second after maximum pressure something like one-third of the whole heat-loss to black walls is due to radiation, and that the radiation is still perceptible half a second after maximum pressure, when the temperature of the gas has fallen to about 1200° C. Of the whole of the heat given up by the gas in cooling from 2100° C. to atmospheric temperature at least one-fifth part goes in radiation.

The explosion vessel employed was cylindrical, the internal dimensions being 30 cm. by 30 cm. It was made of cast iron, and the inner surface

after finishing in the lathe was electro-plated. The polishing was done at first by hand with a leather and rouge, but though this gave a surface which to all appearances was perfectly polished, it was found that appreciably higher pressures and much slower cooling could be obtained by putting more work into the polishing. Accordingly in the later trials a motor-driven buffing wheel was used, and the surface was also carefully washed with alcohol. For blackening the surface, a dead black paint consisting of lamp-black mixed with a little shellac was put on in a thin layer. The weight of this layer when dry was about 1·5 milligrammes per square centimetre, and the thickness about 0·02 mm.

The explosive mixture consisted always of 15 parts of Cambridge coal gas, having a higher calorific value (as determined with a Boys calorimeter) of about 370 lbs.-centigrade thermal units per cubic foot, or say 6000 calories per litre under standard conditions, to 85 of air; this being very nearly the mixture of maximum strength consistent with complete combustion. The mixture was at atmospheric pressure and temperature before firing. The products of combustion contained (by volume) about 8·5 per cent. of CO_2, and 20 per cent. of H_2O, the remainder being nitrogen with a small amount of excess O. In order to secure strictly comparative results a mixture of gas and air containing about 19 per cent. coal gas was stored in a separate tank for from 3 to 12 hours. A portion of this mixture was then transferred to the partially exhausted explosion vessel, being there diluted with sufficient air to give the required strength of 15 per cent. There was sufficient mixture in the tank to give three or four explosions, and, the quantity of air left in the explosion vessel before admitting the mixture from the tank being only about one-fifth of the whole, small errors in the measurement of this quantity were without appreciable effect upon the strength of mixture, which must have been substantially identical for all charges taken from the same filling of the tank. The first explosion was taken with the vessel highly polished. It was quickly followed with a second explosion of the same mixture, the walls having been in the meantime either repolished or blackened, and a third and sometimes a fourth explosion with blackened walls was taken. In this way three or four explosions were made, one or more of which were with polished walls and the remainder with blackened walls. The conditions for these explosions were identical not only as regards the composition of the charge but also in respect of barometric pressure and temperature.

The pressures were recorded in about 20 cases by a pencil indicator of the outside-spring type made by Dreyer and Company, the ordinary reciprocating drum of which was replaced by a revolving drum driven through worm gearing by an electric motor. In order to reduce friction, smoked paper and a fine point on the indicator were used instead of the usual pencil. A facsimile of a diagram so obtained is shown in Fig. 1, p. 408, from which it will be seen that there is slight oscillation due to the inertia.

In the results recorded below a correction has been applied for these oscillations, a smooth curve being drawn in each case, but the maximum pressure is uncertain, perhaps by as much as 1 lb. per square inch. Six diagrams were also taken with the optical indicator of the piston and spring type which I designed some years ago for gas engine work. Two diagrams given by this indicator are reproduced in Fig. 2*. This diagram shows no oscillation, and is probably in every respect more accurate than that given by the pencil instrument. It may be observed, however, that the errors of the latter instrument would not affect comparisons of different explosions taken under the same circumstances, except perhaps as regards maximum pressure.

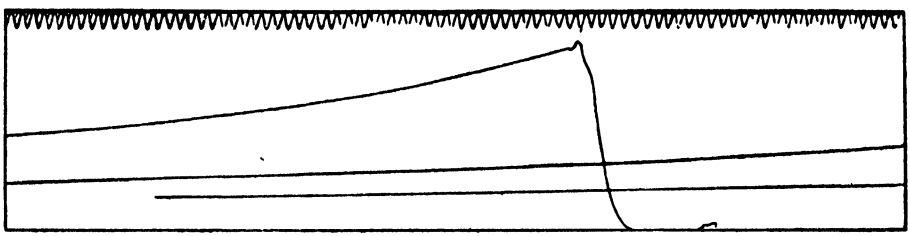


Fig. 1. Record No. 19. 0·76 of original size. Scale of reproduction, 1 cm. =4·7 lbs. per sq. in.

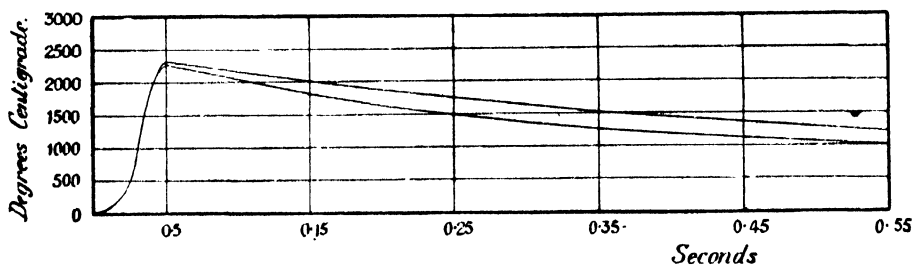


Fig. 2.

In order to show the degree of accuracy reached it has been thought desirable to give all the results of the 27 satisfactory records which were obtained. Particulars of the procedure adopted in polishing are also given in each case, as it was found that this had a material effect upon the results. The trials are separated into groups, each group being taken from one lot of mixture in the tank, as described above. The barometric pressure and temperature varied from one group to another, and in order to make the results comparable the pressures recorded have all been reduced to a standard barometer of 760 mm. and a standard temperature of 12° C., on the assumption that the absolute pressure is proportional to the barometric height and inversely proportional to the absolute temperature of the mixture before explosion. In comparing the results of one day with

* These diagrams, as also Figs. 4 and 7, are exact tracings from photographic enlargements of the originals, the tracing being subsequently reduced in reproduction for printing.

Table I.

Record No.	Pressure, lbs. per square inch					Polishing
	Maximum pressure	Times after ignition, secs.				
		0·15	0·25	0·35	0·45	
			Pencil Indicator			
1	111·9	96·6	86·2	76·5	—	First record taken in this vessel; polished with dry rouge.
2	109·0	95·0	82·7	74·2	—	Hand-polished with wash-leather.
3	107·8	85·5	68·3	58·0	50·5	Blackened.
4	109·0	—	72·5	—	—	Hand-polishing after blackening for (3).
5	107·6	—	72·4	—	—	Hand-polished.
6	105·9	83·3	68·2	57·5	49·9	Blackened.
7	109·5	97·0	84·2	—	—	Polished with motor-driven buff and dry rouge.
8	110·5	93·8	80·1	68·5	—	Hand-polished with wash-leather.
9	110·0	84·2	67·5	56·4	49·4	Blackened.
10	110·0	93·1	79·4	69·0	—	Polished with motor-driven buff and dry rouge.
11	110·0	92·6	79·0	69·5	—	Hand-polished.
12	111·7	87·0	71·9	61·0	53·5	Blackened.
13	110·4	87·0	70·8	60·1	52·5	,,
14	111·0	98·6	88·4	79·6	71·0	Polished with motor-driven buff and rouge mixed with methylated spirits, re-washed with methylated spirits and hand-polished.
15	110·0	93·3	81·2	70·0	—	Hand-polished.
16	110·6	87·3	70·8	60·4	52·3	Blackened.
17	108·1	86·2	70·4	59·4	51·3	,,
18	112·9	98·4	86·3	76·3	67·2	Polished as in (14).
19	112·0	95·1	81·1	—	—	Ditto, re-washing with methylated spirits omitted.
20	111·4	98·4	86·2	75·5	67·2	Polished as in (14)
21	110·2	86·3	71·1	60·3	53·0	Blackened.
			Optical Indicator			
I	114·0	98·7	84·2	73·5	64·9	Highly polished motor-driven buff and rouge, washed with methylated spirits and again polished.
II	114·0	98·7	84·2	73·5	65·4	Ditto.
III	110·8	89·2	73·1	61·3	53·5	Blackened.
IV	114·9	99·0	85·4	73·5	64·9	Highly polished as in I and II.
V	111·6	89·5	73·9	62·1	53·8	Blackened.
VI	111·2	89·1	73·9	62·1	54·7	,,

another day it should, however, be remembered that the calorific value and composition of the gas vary slightly from day to day.

Looking first at the records of the explosions in which the walls were blackened, it will be seen that the two cooling curves from the same mixture agree at all points within 1 lb. per square inch. With the optical indicator (V and VI) the agreement is closer, and here it extends also to the maximum pressure. On account of oscillation the pencil indicator is not so reliable at maximum pressure. The results may be summarised as follows:

Table II.—Blackened Walls.

	Maximum pressure			0·25 sec.		
	Min.	Max.	Mean	Min.	Max.	Mean
Pencil (8 cards) ...	105·9	111·7	109·3	68·2	71·9	70·0
Optical (3 cards)	110·8	111·6	111·2	73·1	73·9	73·6

The optical indicator reads appreciably higher than the pencil instrument. This may be partly due to a change in calorific value, but it is probable that there is also some difference between the instruments. Both were calibrated with dead weights.

The "polished" records do not show such good agreement, and it is at once apparent that the procedure followed in polishing is important, differences hardly apparent to the eye greatly affecting the rate of cooling. The 13 pencil records with polished walls show at 0·25 second pressures ranging from 72·4 to 88·4 lbs. per square inch, a range of 16 lbs. Explosions of identical mixtures give cooling curves which differ at this period by 5 or 6 lbs. per square inch. This uncertainty disappeared largely but not entirely after record 14, when the practice was adopted of washing the surface with methylated spirits so as to remove all traces of the shellac used in the previous blackening. By combining this washing with the use of a motor-driven buff more consistent results were obtained.

In what follows reference will only be made to those "polished" records in which this procedure was followed, namely, Nos. 14, 18, 20, 21, I, II, and IV. The results of these experiments agree moderately well, but it is evident that there are still differences in the walls which substantially affect the rate of cooling, though quite inappreciable to the eye.

(*a*) *Maximum Pressure.* The maximum pressure reached with the polished walls is in every case higher than that reached with the blackened walls. The difference shown by the pencil indicator is in one group (14, 16, 17) 1·7 and in the other group (18, 20, 21) 1·9 lbs. per square inch, the mean being 1·8; while the differences shown by the optical indicator are respectively 3·2 and 3·5 lbs. per square inch, the mean being 3·3. Taking the optical indicator as probably the more accurate, it follows that the

maximum temperature reached with a polished lining is approximately 70° higher than that reached with the blackened lining. In considering this result it is to be remembered that the volumetric heat of the products of combustion is probably about 50 per cent. greater at this temperature than is the mean volumetric heat between 100° C. and 2000° C., so that the 3 per cent. difference in maximum temperature may represent something like 5 per cent. difference in total thermal energy. It is probable, however, that this 5 per cent. is not wholly represented by a difference in the heat which has been lost to the walls, but that it is partly due to incompleteness of combustion. At the moment of maximum pressure the rate of transformation of chemical into thermal energy just balances the rate of loss of heat to the walls. This heat-loss being less when the walls are polished, the percentage of uncombined gas will also be less, since the rate of transformation of energy diminishes with the amount remaining to be transformed. Any effect of the reflecting state of the walls on the rate of combustion or on the ultimate state of equilibrium would, of course, invalidate this argument; such an effect is not impossible, but I have not yet discovered any evidence of it*.

(*b*) *Rate of Cooling.* That the gas cools more slowly when the lining is reflecting is at once obvious from Table I or from Fig. 2, which shows records I and III, obtained with the optical indicator from the explosion of the same mixture, superposed. The following table shows the time (from the moment of ignition) taken by the gas to cool to 1700° C. and 1200° C. respectively with different surfaces. Since the heat lost by the products of explosion in cooling to a given temperature must be very nearly the same whatever the state of the walls, the times are inversely proportional to the mean rate of cooling. This ratio is shown in each case, the rate of cooling with the black walls being taken as the unit.

Table III.

Record No.	1700° C.			1200° C.		
	Black	Polished	Ratio	Black	Polished	Ratio
14, 16, 17 ...	0·178	0·320	0·56	0·297	0·567	0·52
18, 20, 21 ...	0·177	0·289	0·61	0·302	0·473	0·64
I, II, III ...	0·193	0·269	0·72	0·320	0·455	0·70
IV, V, VI ...	0·196	0·277	0·71	0·327	0·450	0·73

* Some recent experiments by Mr W. T. David suggest that the state of equilibrium is not independent of the walls. He finds that the gas radiates more strongly at a given temperature when the walls are polished. If this be so, it must mean that the energy of the molecular vibrations is a greater proportion of the whole when the walls are polished than when they are black, and, consequently, that the energy of a gas enclosed in polished walls is greater than in black walls, though the temperatures (as inferred from the pressures) are the same. In other words, the apparent specific heat of a gas varies with the vessel in which it is contained. It cannot be said that this conclusion has yet been completely established, but it suggests some interesting possibilities.

It will be seen that the rate of loss of heat to the polished surface varies in different observations from rather over half to about three-quarters of the loss to black walls. The differences between one experiment and another cannot be ascribed in any large measure either to errors of observation or to the indicators. Indicator errors would affect both records equally and would not affect the ratios of times. It seems quite certain that in the optical records, which were taken together after all the others, the surface was not so good. Further there seems to be no reason to doubt that in explosion 14 the rate of cooling with the polished surface was not much more than half of that with the blackened walls. The tangents to the two cooling curves at corresponding points (that is points for which the temperatures are the same) tend to become parallel as the temperature falls. The slopes of these tangents are the ratio of the rates of heat-loss, which therefore tend to equality as the gas cools.

Comparisons similar to the above with a tin-foil lining first bright and then black showed that the ratio of rates of cooling down to 1200° C. was about 0·87. If the difference between this ratio and unity be taken as a measure of the radiation reflected it will be seen that tin-foil reflects for this purpose less than half as well as silver. It is not surprising, therefore, that no difference in maximum pressure could be detected with the tin-foil lining, using a pencil indicator.

Bolometric Measurements.

Having obtained the above results, it was decided to make bolometric determinations of the actual heat-loss to the walls and of the radiation. For this purpose a recording bolometer was devised, consisting of a silver or platinum strip whose resistance could be recorded on a revolving drum at the same time as the pressure developed by the explosion. The method used for recording the temperature of the strip was the same as that which I have described in a previous paper*. Briefly, it consists in passing through the strip a constant continuous current sufficient to produce some difference of potential at its terminals, balancing this difference by means of a source of constant E.M.F., and recording by a reflecting galvanometer the increment of E.M.F. which arises when the strip is heated by the explosion. The deflection of the galvanometer is proportional to the increase in E.M.F., which again is proportional to the increase of resistance of the strip, and therefore to its rise of temperature. The connections are shown in Fig. 3. The

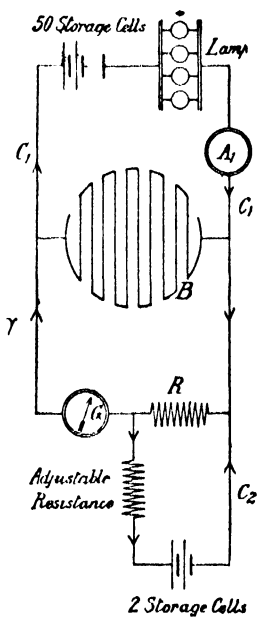


Fig. 3.

* *Proc. Roy. Soc.*, A, vol. 79, p. 138. Page 391 of this volume.

current C_1 passing through the bolometer B is taken from a number of storage cells working through a resistance high enough to ensure that the small variation of resistance in the bolometer during the explosion shall have no appreciable effect upon it. An ammeter A_1 is included in this part of the circuit, and the current is adjusted to a definite value before the experiment. The terminals of the bolometer are also connected to the recording galvanometer G through a resistance R, in which a current C_2 is maintained by means of storage cells in such a direction and of such amount that the E.M.F. at the terminals of the bolometer when cold is approximately balanced. The resistances are such that C_2 does not appreciably vary when the bolometer is warmed. When the bolometer is heated the difference of potential between its terminals rises in proportion to the rise of temperature, while the balancing difference between the terminals of resistance R remains the same except for the small change in the current flowing in it consequent on the diversion of some part of the current C_1 from the bolometer to the galvanometer circuit. This change of current in R and in the bolometer is small, but as it is recorded on the galvanometer it can be allowed for if necessary.

The recording galvanometer used in these experiments was of the ballistic suspended-coil type. The coil was of the standard pattern made by Messrs R. W. Paul, but the usual suspension, which would give a period of about 4 or 5 seconds, was replaced by a much stiffer suspension, giving a period of about 1/25 second; and for the usual permanent magnet was substituted an electro-magnet magnetized nearly to saturation, which gave a strong and fairly constant field, whose amount could be accurately adjusted to a definite value by a comparatively rough adjustment of the magnetizing current. The resistance of the galvanometer was about 3·5 ohms, and its deflection was recorded photographically on the same revolving drum as that on which the pressures were recorded by the optical indicator.

It will be obvious that with the above arrangement the deflection of the galvanometer is nearly proportional to the increase of resistance of the bolometer strip; and this is so, even though exact balance is not obtained before the explosion. In order to get the relation between these two quantities, the current C_2 was reduced to zero* when the bolometer strip

* Strictly speaking, this should be done by reducing the E.M.F. in the circuit C_2 to zero (short-circuiting the battery) and not by means of the adjustable resistance; and similarly the reduction of the current in circuit C_1 should be effected by a reduction in the E.M.F. of the battery. The resistances in the circuit being then all the same in calibration and in use, the relation between galvanometer deflection and change of resistance is given accurately in the manner described, being independent of the small changes in C_1 and C_2 consequent on the disturbance of the circuit by the increase of the bolometer resistance. In practice it is simpler to make the resistances in circuits C_1 and C_2 so large that they can be treated as infinite, and to use them for adjustment of current; but it must not be forgotten that any variation in either of these currents has the same effect as an equal percentage variation of the bolometer resistance. If, for example, it be desired to measure a rise of temperature of 40° C. correct to 1 per cent., the rise of resistance amounts to perhaps 15 per cent., and consequently C_1 or C_2 must be constant to within 1 part in 600.

was cold, and the ratio was found between the galvanometer deflection under these conditions and the current in the bolometer strip, which latter was, of course, for this purpose reduced to a fraction of the value employed when taking explosion record. If c be the current under these circumstances corresponding to any deflection of the galvanometer, then, when there is a current C_1 shown on the ammeter A_1, the proportion of increase in the bolometer resistance corresponding to the same deflection of the galvanometer is $\frac{c}{C_1 - \gamma}$, where γ is the current in the galvanometer.

Since the rise of temperature is directly deduced from the proportion by which the resistance rises, it will be seen that the calibration of the bolometer, that is, the reduction of galvanometer deflection to temperature increase, requires only an accurate knowledge of the ratio of the currents c and C_1, together with a rough knowledge of the galvanometer current γ, provided the latter is small compared with C_1. In the experiments under consideration, γ never exceeded about 1 per cent. of C_1, and could be neglected altogether.

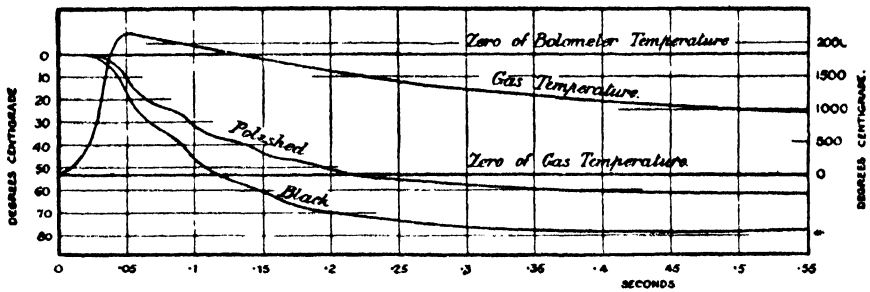


Fig. 4.

(1) *Polished and Blackened Silver.* The first bolometer used consisted of a plate of chemically pure silver, 1/5 mm. thick, cut up so as to make a continuous strip having a resistance of 0·065 ohm at 14° C. The weight of the silver per square centimetre was 0·210 gramme, and it was mounted on a backing of linoleum 1/4 inch thick, which was fixed on to the flat end-cover of the explosion vessel, a little above the centre. The temperature coefficient of the resistance of the silver was measured, and found to be 0·0040; its specific heat was assumed to be 0·056. The silver strip was fixed to the linoleum by means of shellac varnish and a few pins. Comparative records were taken with this bolometer first polished as highly as possible, and then blackened over, sometimes with the same blackening mixture as was used in the experiments described above, and sometimes with camphor smoke. The layer of black on the silver slightly increases its thermal capacity. The layer weighed something under 1 milligramme per square centimetre, equivalent to perhaps 0·2 milligramme of water, which is less than 2 per cent. of the heat capacity of the silver. This small addition is neglected in what follows, the heat capacity of the bolometer being supposed

the same whether blackened or not. A considerable number of records were taken with the bolometer in these two states. The rest of the surface was blackened in all cases.

Tracings of two records are shown superposed in Fig. 4; the pressure records in these two cases were identical. It will be seen that the galvanometer is thrown in slight oscillation by the sudden increase in heating when the flame strikes the bolometer. The temperatures of the silver shown in these records after smoothing the oscillations are given in the following table, their ratios being shown in the fifth column:

Table IV.

Time (from ignition)	Gas temperature	Bolometer temperatures		Ratios of temperatures	Ratios of heat absorbed
		Black	Polished		
0·05	2150	15·9	12·0	0·755	0·755
0·10	1940	45·8	31·9	0·695	0·70
0·15	1750	61·0	44·1	0·725	0·72
0·20	1590	70·0	51·7	0·74	0·73
0·30	1350	76·8	59·0	0·77	0·75
0·50	1030	78·7	62·5	0·795	0·77

In the course of being heated by the flame the silver loses a considerable quantity of heat to the backing with which it is in contact. The percentage of heat so lost depends upon the thermal conductivity and the thermal capacity of the backing, and on the manner in which the temperature of the silver rises. If the two curves for polished and blackened silver were precisely similar, differing only in respect of the scale of temperature, the proportion of heat lost at corresponding times would also be the same for both, and the quantity of heat absorbed would be proportional to the temperature. The curves are not quite of the same shape, and the small correction applicable on this account has been calculated and is allowed for in the last column. The figures there given represent, probably within 5 per cent., the proportion in which the black and polished silver surfaces absorb heat when exposed to gas temperatures varying in the manner shown by the above record. It appears that the mean rate of absorption during the first quarter of a second by the polished surface is 0·74 of the absorption by the blackened surface. The records of pressure obtained with polished and blackened walls, to which reference is made in section (*b*) above, showed that the mean rate of cooling in a polished vessel during the first quarter of a second was in different experiments from 0·56 to 0·71 of the rate observed when the walls are blackened. The agreement is as good as can be expected, having regard to the great effect of small variations in the state of polish. The bolometer also shows that approximation of the rates of cooling in the two cases as the temperature falls

which appeared in the pressure records. The difference in heat absorption represents no doubt the greater part of the radiant heat emitted by the gas in the time, but it does not represent the whole of it. For the black surface is not perfectly absorbent, nor is the polished surface perfectly reflecting. Thus it is quite certain that of the heat given by the gas to the walls of a blackened enclosure during the first quarter-second after maximum pressure, at least 30 per cent. is radiant heat, and the proportion is probably a good deal higher.

The absolute determination of the quantity of heat-loss represented by the rise of temperature of the bolometer strip cannot be effected very accurately, owing to the large proportion of heat lost to the backing. In order to estimate this proportion, the backing may be considered as an infinite solid the temperature θ of whose plane face is at each instant that of the silver, and is therefore known in terms of the time from the record. Let k and c be the thermal conductivity and thermal capacity per unit of volume respectively of the backing. Then it is easy to show that the total heat which has passed into the backing at time T, reckoned from some moment before the silver began to warm up (say the moment of ignition) is

$$2\sqrt{\frac{kcT}{\pi}}\int_0^T \frac{d\theta}{dt}\sqrt{1-\frac{t}{T}}\,.\,dt.$$

The ratio between the total heat which has passed into the backing and that remaining in the silver ($C\theta_T$) is then

$$\frac{2}{C}\sqrt{\frac{kcT}{\pi}}\cdot\frac{1}{\theta_T}\int_0^T \frac{d\theta}{dt}\sqrt{1-\frac{t}{T}}\,.\,dt.$$

The fraction $\frac{1}{\theta_T}\int_0^T \frac{d\theta}{dt}\sqrt{1-\frac{t}{T}}\,.\,dt$ is easily obtained graphically for various values of T from the graph of θ, and depends only on the shape of that graph, being independent of the scale to which it is drawn. To complete the calculation of the proportion of heat lost, it is only necessary to find or estimate the quantity $\frac{2}{C}\sqrt{\frac{kc}{\pi}}$. This might be done by direct measurement of the thermal conductivity and thermal capacity of the backing. The fact that the silver does not cover the whole surface, and that there is a layer of shellac of varying thickness between it and the linoleum, however, introduces a good deal of uncertainty. A more accurate method is to compute the total energy in the gas from its temperature at some time when it has cooled so far that its specific heat may be considered known, and to calculate the total loss of heat at that instant by deducting the energy from the heat developed in the combustion. Comparing this heat-loss with the heat then accounted for in the silver, a value of $\frac{2}{C}\sqrt{\frac{kc}{\pi}}$ may be found, which value can be used for computing heat-loss at earlier periods when, from want of accurate knowledge of the internal energy of the gas,

it cannot be determined directly. The thermal conductivity and capacity of the backing determined in this way will be effective values of these quantities, in which the various factors of incomplete covering, etc., are taken into account.

Applying this method in the present instance, it has been found that 0·5 second after firing, when the temperature of the gas is 1000° C., the total heat-loss to the walls (averaged over the whole surface) is 2·5 calories per square centimetre*. The temperature of the blackened silver is then 79° C., equivalent to 0·93 caloric per square centimetre in the silver. The heat lost to the backing at this epoch is therefore $2·5 - 0·93 = 1·57$ calories per square centimetre. It is here assumed that the heat absorbed by the bolometer is equal to the average absorption over the whole surface. When $T = 0·5$, we have, therefore,

$$\frac{2}{C}\sqrt{\frac{kcT}{\pi}} \cdot \int_0^T \frac{1}{\theta_T}\frac{d\theta}{dt}\sqrt{1-\frac{t}{T}}\,.\,dt = \frac{1·57}{0·93} = 1·69.$$

The value of the integral is obtained from the record of temperature (see Fig. 4), and is found to be 0·895 when $T = 0·5$. It follows that

$$\frac{2}{C}\sqrt{\frac{kc}{\pi}} = 2·7.$$

A direct measurement of the thermal conductivity and thermal capacity of the linoleum, by methods which need not be described here, gave

$$k = 0·00032,$$
$$c = 0·62,$$

whence

$$\frac{2}{C}\sqrt{\frac{kc}{\pi}} = 1·35,$$

or only about half the value given by the other method.

Assuming that $\frac{2}{C}\sqrt{\frac{kc}{\pi}}$ is equal to 2·7, the table, p. 418, shows the absolute quantities of heat absorbed by the blackened and polished surfaces per unit area at different times. The second column gives the temperatures reached by the two bolometers; the third, the corresponding heat quantities in the silver (calculated on the assumption that the specific heat of pure silver is 0·056); the fourth, the factor

$$\frac{2}{C}\sqrt{\frac{kcT}{\pi}} \cdot \frac{1}{\theta_T}\int_0^T \frac{d\theta}{dt}\sqrt{1-\frac{t}{T}}\,.\,dt,$$

which gives the proportion of heat lost to the backing; and the fifth, the actual absorption after allowing for this loss. The sixth column is the difference in the heat absorption of the blackened and polished surfaces.

If the absorption by the bolometer is equal to the average over the whole surface of the vessel, the total heat lost to the walls of the blackened

* See Appendix.

vessel at maximum pressure (0·95 second) is 1080 calories, or 6½ per cent. of the whole heat of combustion. This agrees well with the results of experiments in which the resistance of a copper strip completely lining the vessel was recorded*. These experiments showed a total loss of 1200 calories, or 7·5 per cent., at the moment of maximum pressure after the explosion of a 15 per cent. mixture. The difference between the absorption of the blackened and polished surfaces at this moment would amount to 260 calories for the whole vessel, or about 2 per cent. of the heat of combustion. As already stated, the observed difference in maximum temperature probably corresponds to 5 per cent. in the thermal energy. The balance, which amounts to 2 or 3 per cent., represents a difference in completeness of combustion, in favour of the polished walls.

Table V.

Time	Temperatures °C.			Heat in silver, calories per sq. cm.		Proportion lost to backing		Total absorption		Difference calories per sq. cm.
	Gas	Silver								
		B	P	B	P	B	P	B	P	
0·05	2150	15·9	12·0	0·188	0·142	0·30	0·30	0·245	0·185	0·06
0·10	1940	45·8	31·9	0·530	0·376	0·50	0·50	0·795	0·565	0·23
0·15	1750	61·0	44·1	0·720	0·520	0·70	0·69	1·22	0·88	0·34
0·20	1590	70·0	51·7	0·826	0·610	0·90	0·88	1·57	1·15	0·42
0·30	1350	76·8	59·0	0·906	0·696	1·22	1·18	2·01	1·52	0·49
0·50	1030	78·7	62·5	0·929	0·737	1·70	1·63	2·51	1·94	0·57

(2) *Bolometer behind Fluorite Screen.* A direct measurement of the radiation was obtained by a recording bolometer placed outside the vessel behind a fluorite window. This instrument, which was designed by Mr W. T. David, is shown in Figs. 5 and 6. It consists of a platinum strip (Fig. 5) mounted on the end of a hollow wooden cylinder. The strip weighed 0·25 gramme per square centimetre and had a thermal capacity of 0·008 gramme of water per sq. cm. (specific heat assumed to be 0·032). The resistance was at first 0·15 ohm, but was reduced in later experiments by short-circuiting two of the coils to about 0·115 ohm (room temperature). The temperature coefficient was measured and found to be 0·0036. The connections of the bolometer were the same as in the experiments last described (blackened and polished silver—see Fig. 3). The bolometer, which was blackened with camphor smoke, was pushed into a gun-metal tube screwed into a boss on the end-cover of the vessel, which carried at its inner end the plate of fluorite (Fig. 6).

The fluorite was ¼ inch thick, and the clear opening 1½ inches diameter. The interior of the vessel was blackened.

* Full details of the method are given in *Proc. Roy. Soc.*, A, vol. 79, p. 139. Page 391 of this volume.

One record taken under these conditions during the explosion of 15 per cent. mixture is reproduced in Fig. 7. The figure is an exact tracing

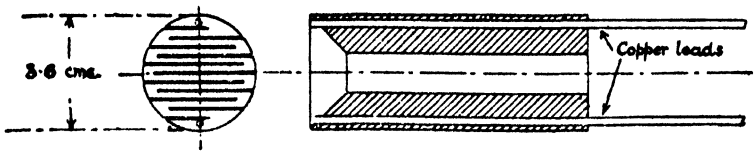


Fig. 5.

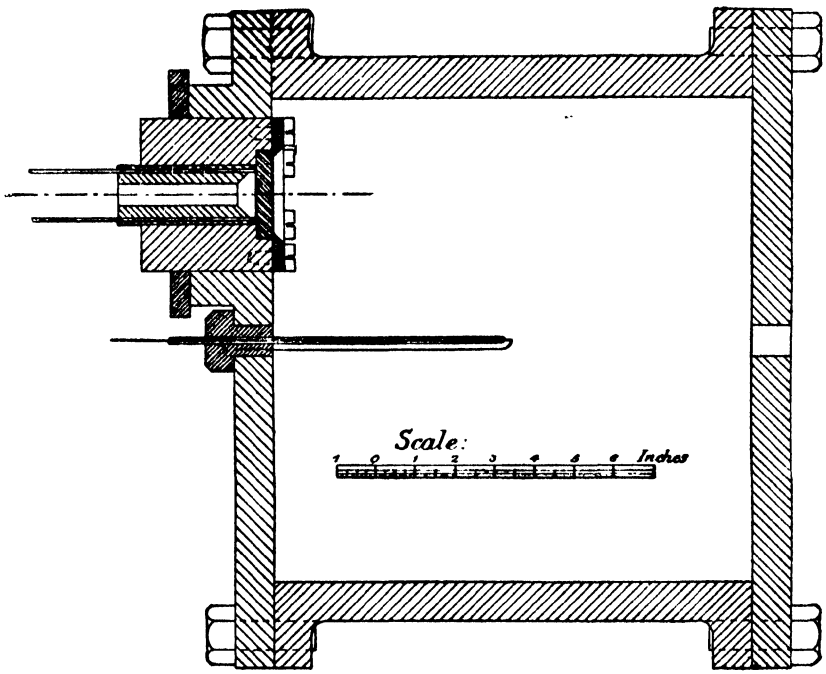


Fig. 6.

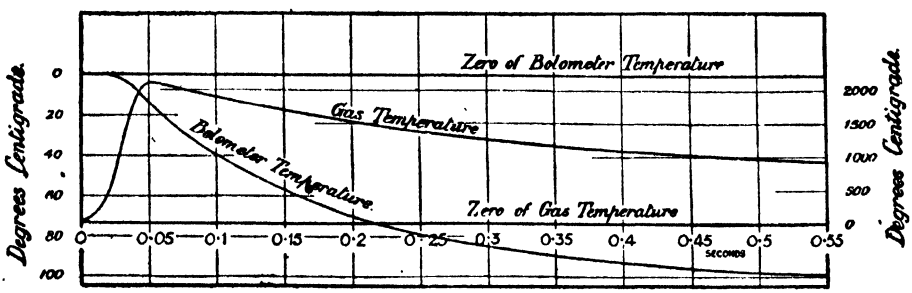


Fig. 7.

from an enlargement of the original. It is noteworthy that there is no oscillation of the galvanometer, doubtless because there is no sudden

heating such as occurs when the flame touches a surface in contact with the gas. The following table shows the results of this record:

Table VI.

Time from ignition	Temperatures °C.		Heat in platinum, calories per sq. cm.	Loss	Total absorbed, calories per sq. cm.	Percentage of heat of combustion
	Gas	Platinum				
0·05	2090	13·6	0·11		0·11	3
0·1	1870	39·6	0·315		0·315	8·5
0·15	1690	57·4	0·46		0·46	12·5
0·2	1510	70·3	0·57		0·57	15·5
0·3	1290	84·2	0·675	0·025	0·70	19
0·4	1110	91·7	0·735	0·035	0·77	21
0·5	980	96·4	0·770	0·05	0·82	22

The loss of heat from the platinum by conduction and radiation (shown in the fifth column) was determined by calculation from the rate of fall of temperature in the later stages of cooling, when no radiation was being received. The percentages in the last column are calculated on the assumption that radiant heat is absorbed by every part of the blackened walls at the same rate as by the bolometer, no allowance being made for absorption in the fluorite or reflection from the bolometer. Probably about 10 per cent. should be added on this account to get the true amount of radiation.

The amount of radiation shown in this experiment is from 40 to 50 per cent. in excess of that given by comparison of the blackened and polished silver bolometer inside the vessel, at corresponding times. The uncertainty attaching to the latter observations, partly because they are difference measurements, and partly because of the very large corrections for loss to the backing, will not have escaped observation. I think, however, that the discrepancy is too great to be accounted for in this way, and that the difference between the absorption by the blackened and polished silver surfaces is really a good deal less than the whole of amount of radiation. Mr W. T. David has suggested that a film of water may perhaps be formed on the polished surface at an early stage of the cooling and impair its reflecting qualities. About 5 grammes of water are produced in the explosion, and practically the whole of this ultimately condenses on the walls, covering them to an average depth of 1/100 millimetre all over, thus destroying in large measure their reflecting power. It is probable that the formation of this layer commences almost as soon as the flame touches the metal, and that a much thinner layer would be adequate to absorb a considerable fraction of the radiation falling upon it. The fluorite plate, on the other hand, must almost instantly become too hot on the surface to admit of the deposit of moisture. It is possible that the rate of formation

of this film depends on the exact state of the metal surface, and that we have here the explanation of the great effect of apparently inappreciable differences in polish on the rate of cooling.

It was found that if a plate of glass was substituted for the fluorite, the heat received by the bolometer was reduced to about one-third; and that if the platinum were polished the heat received was only about 20 per cent. of that recorded with a blackened surface. The latter proportion agrees with the figure given by Hagen and Rubens for the reflecting power of polished platinum.

The experimental work described in this paper has been carried out by Mr W. T. David, advanced student at the Engineering Laboratory, to whom I am much indebted for the ability and zeal with which he has prosecuted the research and for many suggestions of value. He is engaged upon experiments with the recording bolometer similar to those last described, by which it is hoped to determine the nature and origin of the radiation, and its dependence on temperature and density. The work of R. von Helmholtz* and of Julius† proves that the radiation from a non-luminous flame (*e.g.* a Bunsen flame) burning at atmospheric pressure consists mainly of two bands, of wave-lengths $4{\cdot}6\,\mu$ and $2{\cdot}8\,\mu$, which come from the carbonic acid and steam respectively. It is probable that the radiation in a closed-vessel explosion is of substantially the same quality, and Mr David's work, so far as it has gone, confirms this view‡.

APPENDIX.

Calculation of Heat-loss from Pressure Record.

At 0·5 second from the time of ignition the mean temperature of the gas calculated from the pressure is nearly 1000° C. The energy is shown in the table, p. 422:

The first factor in the energy of each constituent is the quantity of heat (in calories) evolved by that constituent per gramme-molecule (taken to be 22·25 standard litres in each case) in cooling at constant volume from 1000° to 100° C. These energies are calculated from the results of Holborn and Henning for the specific heat of these gases, but a correction of 5 per cent. has been added in each case because there is reason to suppose

* *Die Licht- und Wärmestrahlung verbrennender Gase*, Berlin, 1890.

† *Die Licht- und Wärmestrahlung verbrannter Gase*, Berlin, 1890.

‡ It has been suggested that possibly some of the radiation comes from the walls of the vessel which are heated by the explosion. From consideration of the rate of flow of heat per sq. cm. however, it can be shown that the surface temperature of the metal can never exceed about 40° C. which is of course quite inadequate to give any appreciable radiation. Further, Mr David has found that if a patch of the wall opposite the bolometer be highly polished, the radiation received is *increased*. The fluorite plate will be heated to a fairly high temperature at the inner surface but the radiation omitted by this surface will be absorbed in the fluorite and none of it will reach the bolometer. No appreciable heating of the outer surface can occur during the measurement of the radiation.

that Holborn and Henning's values are too low. The figures are uncertain to the extent of about 5 per cent. The quantity of gas present is 20·3 litres under standard conditions, or 0·905 gramme-molecule (volume of vessel

Table VII.

Gas	Volume (gramme-molecule)	Energy 1000° to 100° C.
CO_2 ...	0·09	9000 × 0·09 = 810
H	0·21	6500 × 0·21 = 1370
N	0·70	5000 × 0·70 = 3500
Total ...	1·00	5680

22·3 litres, pressure before explosion 763 mm., temperature before explosion 16° C., contraction 4 per cent.). Hence the total heat present in the gas (reckoning from 100° C.) at 0·5 second is 5200 calories. The heat evolved by burning the coal gas, taking a calorific value of 115,000 calories per gramme-molecule (580 B.TH.U. per standard cubic foot, lower value), is

$$0{\cdot}15 \times 115{,}000 \times 0{\cdot}945 = 16{,}300 \text{ calories},$$

since the volume before combustion is 4 per cent. greater than after. Hence at this time the total heat which has passed into the walls is 16,300 − 5200 = 11,100 calories. The total surface of the vessel is 4380 square centimetres, so that the average loss per square centimetre is about 2·5 calories.

26

THE PRESSURE OF A BLOW.

[Evening Discussion at the Royal Institution, Friday, Jan. 26, 1912: LORD RAYLEIGH in the Chair.]

THE scientific analysis of a blow requires first the determination of the actual pressures or forces set up between the colliding bodies, and second an investigation of the distribution of these pressures and of their physical effects. The pressure produced by a blow does not differ in kind from that produced by any other agency, such as an hydraulic press, but it differs in degree because of its great intensity and of its extremely short duration, and these characteristics, as we shall see, have a marked influence on the effects which it produces.

The first part of the problem, that is the calculation of the pressure in tons or pounds, is based on the familiar principles of mechanics which were first precisely stated in Newton's laws of motion. The cause of the pressure is the rapid change of motion of the colliding bodies which occurs when they come into contact, and, according to Newton's second law, the force is simply proportional to the rate at which this change is effected. The rate of change may be measured in terms of energy and distance, or in terms of momentum and time. Thus, a hammer head, moving at a rate of 16 feet per second, and weighing 1 lb., possesses 4 foot-lbs. of energy, because its velocity could have been acquired by falling freely through 4 feet. If it strikes a nail and drives it one-eighth of an inch, the energy which was generated by the weight of 1 lb. acting through 4 feet is destroyed in 1/400 part of that distance, and the force necessary to effect this change of motion is 400 times as great, say 400 lbs. The same effect would be produced by a 4 lb. hammer striking with the velocity which would be acquired by falling through 1 foot, namely 8 feet per second. Regarding the same instance from the point of view of momentum, the 1 lb. hammer would take half a second to fall 4 feet, and the quantity of motion or "momentum," reckoned as the product of the force acting into the time required to generate it, would be one-half of a pound-second unit. While driving the nail in, the hammer covers a distance of $\frac{1}{8}$ inch with a velocity which starts at 16 feet per second and drops to zero. To cover the distance of $\frac{1}{8}$ inch with the average velocity of 8 feet per second takes 1/800 second, which is 1/400 of the time ($\frac{1}{2}$ second), which it takes the weight of the hammer-head (a force of 1 lb.) to generate its motion. Thus the pressure required for the rapid stoppage is as before, 400 lbs.

We may take another instance essentially similar to the hammer and nail, but differing greatly as regards scale. A 14 inch armour-piercing shell weighs about 1400 lbs., and when moving at 1800 feet per second possesses about 31,000 foot-tons of energy, or about 17,000,000 times as much as our hammer-head. Such a shell would just pierce a plate of wrought iron $2\frac{1}{2}$ feet thick, and the average force which must be exerted to pull it up in that distance, which is of course the pressure which it exerts on the plate, is 31,000 divided by $2\frac{1}{2}$, or about 12,000 tons. This is equivalent to some 80 tons on the square inch.

When a hammer strikes a nail, the force acting during the blow is practically constant, and the average value obtained as above by dividing the energy by the distance moved, or the momentum by the time taken, is equal to the actual force exerted throughout the impact. In many cases, however, this force is not constant, and it is then necessary to divide the course of the impact into short intervals either of space or of time, calculate the change of energy or momentum in each, and add the result. A familiar instance is that of two billiard balls. We may suppose one ball to strike the other full with a velocity of 16 feet per second, which corresponds to a fairly hard stroke. It simplifies the consideration of the problem, if instead of one ball moving and the other at rest we suppose them to be travelling in opposite directions with equal velocities of 8 feet per second. At the instant when the balls first touch there is no pressure between them, but as they continue to approach, each flattens the other at the point of contact. The balls no longer touch at a point, but over a circular area which rapidly increases in diameter. Corresponding to any given amount of flattening or distance of approach, there is of course a definite pressure which might be measured by actually squeezing the balls together under known forces and measuring the corresponding amount of approach. Or the relation between pressure and distance could be calculated as was done by Hertz. The results are shown in the curve (Fig. 1), and the area under the curve connecting pressure and distance up to any point gives the number of foot-pounds of energy destroyed. When this is just equal to the original energy of the balls they will have been reduced to rest, and in the case supposed, the distance of approach is then 14/1000 inch, and the total pressure between them 1300 lbs. This pressure is distributed over the circle of contact which is 1/6 inch in diameter, and the average intensity of the pressure is 27 tons per square inch. The distribution, however, is not uniform, the pressure at the centre being $1\frac{1}{2}$ times the average. (The shape of the balls at the moment of closest approach was shown on a drawing with a magnification of 100.) The balls are then like compressed springs, their original energy of motion having been completely transformed into strain energy in their substance. The reason of the high intensity of pressure developed is that this strain energy is concentrated into a very small volume of ivory near to the point of

contact. The balls then begin to separate, and the whole process of compression is gone through in reverse order, the strain energy being transformed back into energy of motion by the pressure. Finally the balls rebound unstrained, with nearly the velocity with which they approached.

If for the ivory balls we substituted hollow balls of steel having the same mass, the pressure produced by the blow would be greater, because the steel is much more rigid than ivory and gives less under a given force. Thus the distance of approach is less, the circle of contact smaller, and the maximum intensity of pressure much greater. It reaches 280 tons per square inch averaged over the surface of contact. Such a pressure could only be sustained without permanent effect by a very hard steel. Ordinary

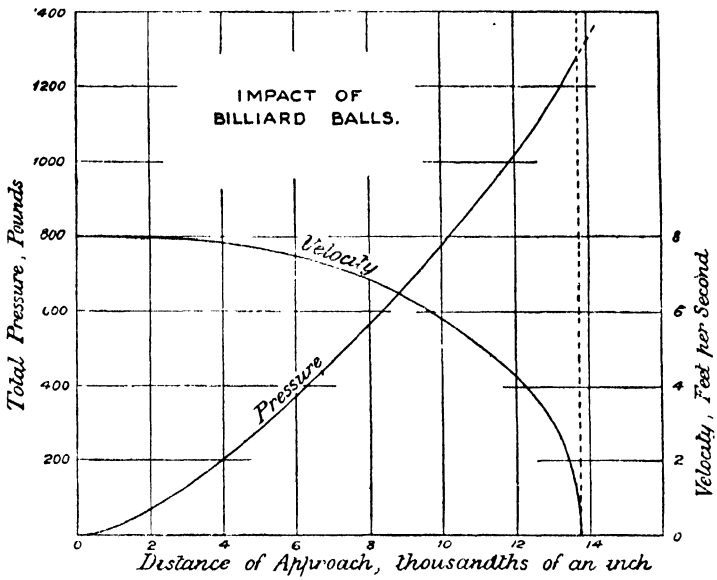


Fig. 1.

mild steel would begin to flow when the pressure passed about 100 tons, a permanent flat would be left by the blow, and the balls would rebound with less velocity than that of approach. The theory whose results I have given does not, of course, apply to such a case, as it depends on the assumption of perfect elasticity.

It is rather remarkable that materials can sustain without injury such large pressures as are produced by these blows. Mild steel balls are not crushed perceptibly till the pressure reaches 100 tons per square inch; yet a short column of the same steel would be crushed by a pressure of 30 tons per square inch. One reason is the extremely short duration of the pressure—it has no time to produce much effect. The other is the fact that in the blow it is accompanied by large lateral pressures exerted by the metal surrounding the area of contact. Pressure equal in all

directions, such as is exerted by the water at the bottom of a deep ocean, produces generally no permanent effect on solids or liquids. To produce breakage or permanent deformation there must be difference of pressure in different directions, and the most important if not the only factor determining whether such breakage or deformation shall occur is the amount of the difference. If for example, our columns of mild steel, which in the absence of lateral support begins to crush at 30 tons per square inch, were surrounded by a jacket exerting a radial pressure of 30 tons per square inch, it is probable that the end pressure might be increased to 60 tons per square inch without any movement occurring. In the impact of balls the metal surrounding the point of contact by resisting the lateral expansion of the compressed part, sets up radial pressure of this kind. It can be shown, in fact, that the lateral pressure at the centre of the circle of contact corresponding to a maximum normal pressure of 100 tons per square inch is 75 tons per square inch, leaving 25 tons per square inch effective for producing deformation or breakage. The greatest difference of pressures, however, is not at the centre of the circle of contact but at points near the circumference of that circle. Thus, as was found by Hertz, fracture commences by the formation of a circular crank of small radius surrounding the point of first contact.

These calculations of pressure are based on theory, and it may be asked what direct experimental evidence we have that the theory is correct. It is not, of course, possible actually to measure the pressures over the minute circle of contact between the balls, nor is it possible accurately to measure the amount of the flattening. We can, however, pursue the calculation a little further, and determine the time during which the balls are in contact from the moment when they first touch to the moment at which they separate on the rebound. In the case of billiard balls moving with a relative velocity of 16 feet per second, this time is 1/4000 second. A precisely similar calculation can be made for balls of steel or other metal, and it is not difficult to measure in the laboratory the time during which such balls remain in contact. The method is of considerable use in connection with impact problems, and it consists in making the two balls, by their contact, close a galvanometer circuit in which there is also a battery and resistance. A certain quantity of electricity, which is simply proportional to the time of contact, then passes through the galvanometer and produces a proportionate deflection in it. It has been found that the time of contact measured in this way for steel balls is exactly that predicted by theory, and it may be inferred that the theory is correct in all its details, and that the pressure calculated by its aid corresponds with the facts. This method was first used by Pouillet in 1845, and has recently been brought to great perfection by Mr J. E. Sears*, who showed, among other things, that the relation between pressure and deformation of steel is almost exactly the same when the pressure is applied for an excessively

* *Proc. Camb. Phil. Soc.*, vol. 14,

short time, as in the case of impact, as it is when applied steadily, as in a testing machine. The assumption that this is the case lies of course at the root of the calculations, and its verification was therefore a matter of considerable importance.

When one billiard ball strikes another the effect of the blow is practically instantaneously transmitted to every portion of the colliding balls, or, to speak more precisely, the time taken to transmit the pressure is short compared with the total time of contact. Except for the minute relative displacement near the point of contact the balls move as a whole, every part having the same velocity at each instance of time and coming to rest at the same moment. In many cases of impact, however, and in those possessing the most interest from a practical point of view, this is by no means the case. We may consider, for instance, the impact of an elongated lead rifle bullet against a hard steel plate. Under the enormous pressures developed lead flows almost like water, and in the absence of lateral support it is as little capable of transmitting those pressures. Thus, when the nose of the bullet strikes, the metal thus brought into contact with the plate immediately flows out laterally, its forward motion being destroyed, but the hind parts of the bullet know nothing of what has happened to the nose because the pressure cannot be transmitted to them, and they continue to travel on with the original velocity until they in their turn come up to the plate and have their momentum destroyed. The process of stopping the bullet is complete when its tail reaches the plate, and the time required is simply that taken by the bullet to travel its own length. Thus a Lee-Metford bullet is $1\frac{1}{4}$ inches in length, or, say, 1/10 of a foot, and if moving at 1800 feet per second, which is about the velocity given with a rifle, it would be stopped in 1/18,000 second. The bullet weighs approximately 0·03 lb., and possesses with this velocity about 1·7 lbs. second units of momentum. The force required to destroy this in 1/18,000 second is 18,000 multiplied by 1·7 lbs., or, say, 14 tons. This acts over the sectional area of the bullet, which is 1/14 square inch, giving a pressure of about 200 tons per square inch. This is the average pressure throughout the impact, but the pressure is probably nearly constant. It is to be noted that the pressure per square inch depends only upon the velocity (varying as its square), and not upon the length or diameter of the bullet. Increase in diameter only alters the area over which the pressure is applied, and increase in length the time during which it is supplied.

If for the bullet of lead we substitute one of hardened steel which will not flow, the problem at once becomes much more complicated. In order to reduce it to its simplest terms, and to bring the theory into such a form that it can be tested in the laboratory, we may suppose that instead of the bullet we have a cylindrical steel rod, say $\frac{1}{2}$ inch in diameter by 10 inches long, with flat ends, and that it strikes quite fair against an absolutely unyielding surface. The latter condition could not be fulfilled in practice.

because there is no substance more rigid than steel. So far as the effects on the rod are concerned, however, it can be fulfilled by making two rods, moving with equal velocities in opposite directions, collide end on; and this device has been used in the laboratory for imitating the effect of impact against an unyielding surface. We have to consider how long it takes to stop the rod under such conditions. When the end first strikes it is pulled up dead, just as in the case of the lead bullet, only it does not now flow out sideways. The pressure, however, set up at the end of the rod cannot be instantaneously transmitted through it, and consequently the hind parts do not at once feel this pressure, but continue to move on as before. The pressure travels with the velocity of sound, which for steel is about 17,000 feet per second, and it takes accordingly 1/20,000 part of a second before the pressure has been transmitted throughout the 10 inches length of the rod. The course of the impact is shown in the diagram (Fig. 2). A wave of pressure is initiated at the first contact and travels along the

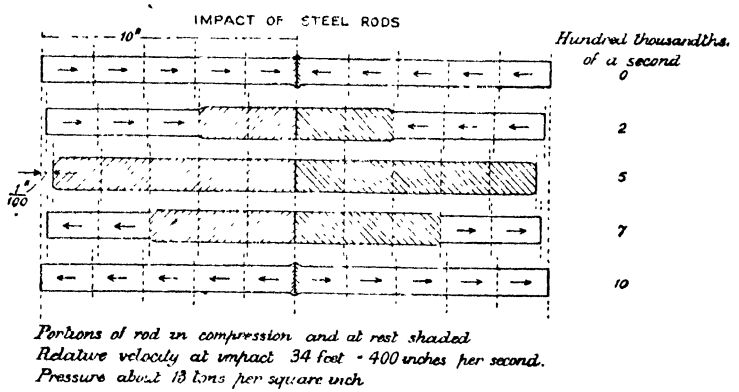


Fig. 2.

rod. At any instant the part of the rod which has already been traversed by the wave will be at rest and in compression (as indicated on a greatly exaggerated scale by the shortening and thickening on the diagram), while the remainder which has not yet been reached by the wave, and accordingly as yet knows nothing of the impact, will still be moving forward with the old velocity. Each section continues to move on until the wave reaches it, when it is stopped with a jerk, the sections thus pulling up successively until the whole rod is at rest, which happens when the wave has travelled to the free end. From the momentum of the rod, and the time taken to stop it, the pressure can be calculated by the use of the principles already illustrated. Thus a rod 10 inches long is stopped, as we have seen, in 1/20,000 second, and if it be moving with the moderate velocity of 17 feet per second, the pressure required to pull it up in this time is 13 tons per square inch. This pressure is constant throughout the impact, and it is obvious that here again the intensity of pressure is dependent only upon the velocity and not on the weight of the rod. For if with the same

velocity the length is increased, the corresponding increase of momentum to be destroyed is cancelled by the greater time required for the transmission of the pressure wave, and if the area is increased, the total pressure is merely increased in proportion, the pressure per unit area remaining the same. For a hard elastic body the pressure is proportional to the velocity, a principle which is probably generally applicable in the initial stage of all impacts.

At the instant of greatest compression, when the rod is reduced to rest, it is like a compressed spring, and there being no pressure acting at its free end to keep it compressed, it proceeds to expand again. Starting at the free end a wave of expansion travels down the rod, the several portions being successively jerked into motion with approximately the original velocity. The whole process of restoring motion to the rod is completed when this wave reaches the impinging end, when the rod rebounds as a whole with the original velocity. The whole time of contact is then that taken by a wave of sound to travel twice the length of the rod. Here, again, by electrical measurement of the time of contact, it is possible to check the theory. It is found that the actual time is longer than that predicted. This is due to the fact that one cannot in practice make the rods hit absolutely true all over the ends; they strike at one point first, and the metal near that point has to be flattened out before the ends come into contact all over and initiate the simple plane pressure wave of the theory. The complete analysis of the discrepancies between theory and experiment so caused was long a puzzle to physicists interested in these matters. It was finally effected by Mr J. E. Sears, who determined mathematically the corrections necessary on this account, and submitted his theory to experimental test with entirely satisfactory results*.

Another simple instance of the propagation of waves along rods illustrates a point of importance in regard to the general effect of blows. Instead of maintaining the pressure during the whole passage of the wave up and down, as in the end-on impact, a pressure is suddenly applied to one end, maintained for a short time, and then removed. A corresponding pressure wave travels along the rod. Each portion of the rod is only stressed or in motion during the passage of the wave over it, and after the passage of the wave it is left with a certain forward displacement, but without any velocity or stress. Furthermore, the whole momentum of the blow is concentrated in the short length of the rod covered by the wave. On its arrival at the other end the wave is reflected, but the reflected wave is a wave of tension. As it comes back the head of the tension wave is at first wholly or partially neutralized by the tail of the pressure wave, but after a time it clears this, and the rod is then put into tension of amount equal to the original pressure. If there be a crack or weak place in the rod at a sufficient distance from the free end, the pressure wave will pass over it

* *Trans. Camb. Phil. Soc.*, vol. 21, p. 49.

practically unchanged; but on the arrival of the reflected tension wave the rod will part, because the crack cannot sustain the tension, and the forward part will move on, having trapped within it the whole momentum of the blow. The rest of the rod will remain at rest and unstrained (Fig. 3).

(The propagation of waves in rods was illustrated by means of a model, consisting of horizontal wooden bars fixed at equal intervals to a vertical wire.)

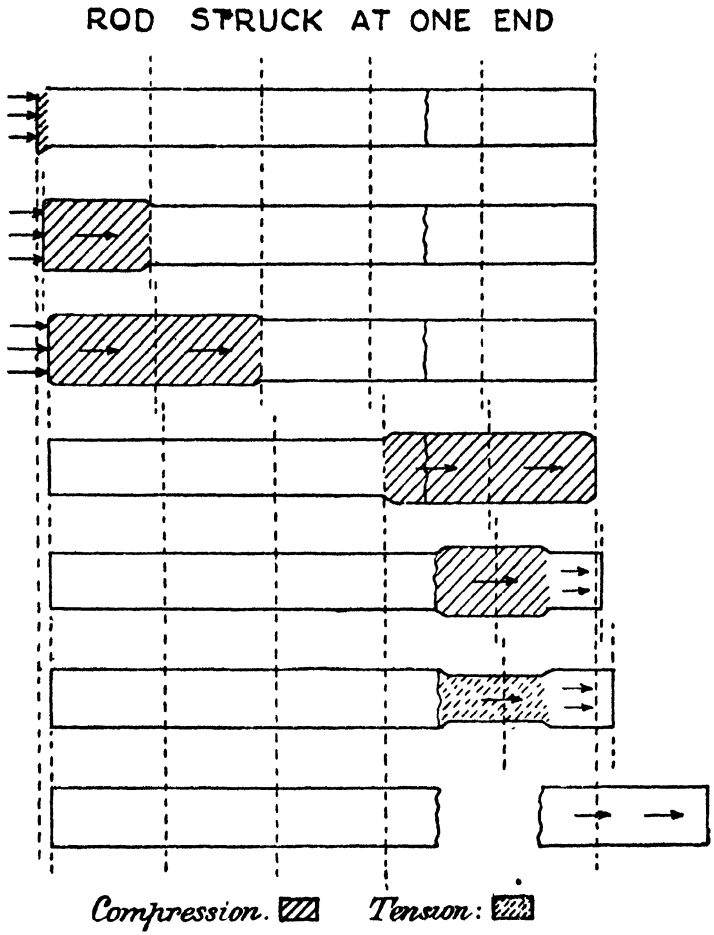


Fig. 3.

The fact that a blow involving only pressure may, by the effects of wave action and reflection, give rise to tensions equal to or greater than the pressure applied, often produces curious effects. I shall choose by way of illustration some observations which I have been making recently, and which I think are new. I have here a small cylinder of gun-cotton. By the use of a small quantity of fulminate in the hole provided for the purpose it is possible to detonate the gun-cotton, which means that in an excessively

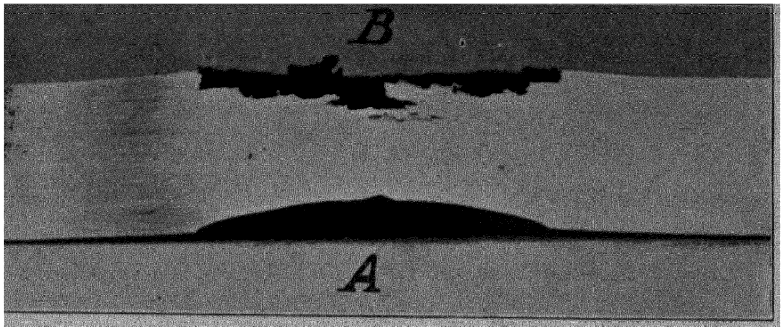


Fig. 4. Section of mild steel plate, $\frac{3}{4}$ inch thick, after detonation of gun-cotton in contact with the side *A*. Scab torn off from *B*.

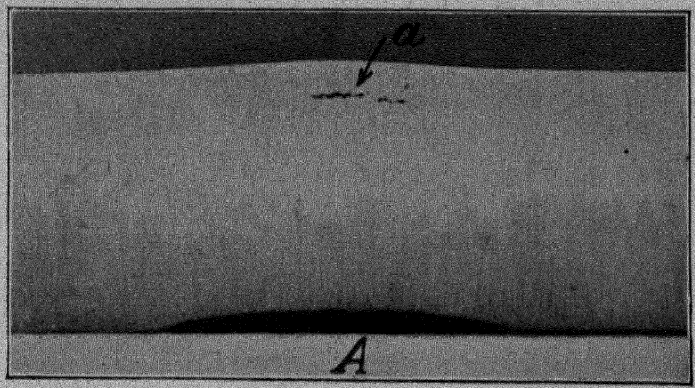


Fig. 5. Section of mild steel plate, $1\frac{1}{4}$ inch thick, after detonation of gun-cotton in contact with the side *A*. Crack formed at *a*.

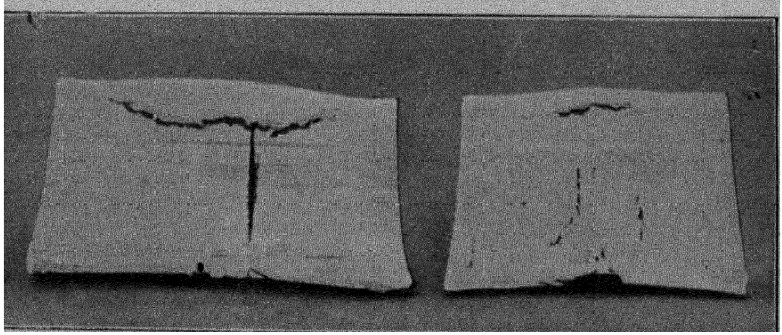


Fig. 6. Axial sections of steel cylinders, after the detonation of gun-cotton in contact with one end.

short time it is converted into gas at a very high temperature. The time required is probably only 3 or 4 millionths of a second, and is so excessively short that the gas does not during the process expand appreciably into the surrounding atmosphere.

Thus the gas generated, which, when completely expanded, will fill a space several thousand times as great, is for a minute fraction of time confined within the volume of this small fragment of gun-cotton. This confinement implies great pressure, how much is at present a matter of doubt. I understand that Sir Andrew Noble estimates it at 120 tons per square inch. The only thing which restrains the expansion of the gas is the inertia of the surrounding air, and the pressure accordingly drops with very great rapidity. It is probable that the pressure is practically gone after 1/30,000 second. The same pressure is, of course, exerted by the gas upon any surface with which the gun-cotton is in contact, and it will be seen that the force so produced has the characteristics of a blow, namely, great intensity and short duration. If such a cylinder of gun-cotton weighing one or two ounces be placed in contact with a mild steel plate, the effect, if the plate be half an inch thick or less, will be simply to punch out a hole of approximately the same diameter as the gun-cotton, just as though it had been struck by a projectile of that diameter. But if the plate be three-quarters of an inch thick, the curious result which I exhibit here is obtained (Plate I, Fig. 4). Instead of a complete hole being made, a depression is formed on the gun-cotton side of the plate, while on the other side a scab of metal of corresponding diameter is torn off, and projected away with a velocity sufficient to enable it to penetrate a thick wooden plank, or to kill anyone who stands in its path. The velocity in fact corresponds to a large fraction of the whole momentum of the blow. The scab behaves much in the same way as the piece which we saw would be shot off the end of a rod struck at the other end if the rod were divided or weakened, so as to be unable to sustain the reflected tension wave. The separation of the metal implies, of course, a very large tension, which can only result from some kind of reflection of the original applied pressure, but the high velocity shows that this tension must have been preceded by pressure over the same surface, acting for a time sufficient to give its momentum to the scab.

Wishing to ascertain how and where the separation originates, I caused a two-ounce cylinder of gun-cotton to be detonated in contact with a somewhat thicker plate. In this case no separation of metal was visible; the only apparent effects being a dint on one side and a corresponding bulge on the other. On sawing the plate in half, however, I was gratified to find an internal crack, obviously the beginning of that separation which in the thinner plate was completed (Plate I, Fig. 5).

The pressure exerted by the gun-cotton in the experiments which I have just described is practically confined to the circular area of contact

between it and the metal, as is shown by the accurate agreement of the print on the plate with that circle. The effects of that pressure must, however, be largely conditioned by the fact that the metal upon which it acts is attached to the surrounding portions of the plate, and is held back by them. In order to get an idea of the effect of this factor, I have tried the experiment of removing this outside metal, leaving the steel cylinder opposed to the gun-cotton. If such a short cylinder of steel be placed in contact with a gun-cotton cylinder of equal diameter, the result of detonation was at first sight merely to flatten it out slightly, and to produce a depression on one side with something of a bulge on the other. No external crack was visible. But on sawing the piece in half a remarkable system of cracks was disclosed (Plate 1, Fig. 6); the cracks spread in all directions, as though tension had been acting in every direction; in fact, it appeared as though the steel cylinder had begun to burst. The tension necessary to produce these cracks, which, as you will see, must have radial as well as axial components, must originate in some kind of wave action which follows the blow. The problem is very complicated, and I have not yet succeeded in finding a full explanation of the phenomenon; but there cannot be much doubt that the longitudinal tensions are due to a wave generally similar to that which we have been discussing in connection with the rod. To account for the radial tensions which the cracks show also to have been present, it is to be observed that the shortening of the cylinder in the direction of its axis, which is the immediate effect of the blow, must be accompanied by a corresponding increase in diameter. This increase takes place very rapidly, and implies that at first the metal is moving out in a radial direction with a high velocity. The stoppage of this radial motion requires radial tension, and probably this is greater at points near the axis for much the same reason that when a stone is dropped into a pond the circular waves which it causes have their greatest amplitude at points near the centre of disturbance. In the case of the steel cylinder the radial tension wave travels inwards from the surface, and its amplitude increases as it goes in.

I have recently been attempting to measure the duration of the pressures produced by the detonation of gun-cotton. The principle of the method may be made clear from the diagram showing the effect of a blow on one end of a rod (Fig. 3). A wave of compression travels along the rod, the length of the wave corresponding to the time during which the pressure has acted; that is, it is equal to the velocity of sound, multiplied by that time. We may assume that the time was 1/20,000 second, which would give a wave just 10 inches long. This wave travels to the end of the rod, is there reflected as a wave of tension, and comes back. If the rod be cut across, the surfaces of the junction being accurately faced and in firm contact, the pressure wave will pass the joint without change, but on the arrival of the head of the tension wave at the joint, the parts will

separate and the end-piece will fly off. If the tail of the pressure wave has then cleared the joint, the separated end-piece will have trapped within it the whole momentum of the blow, and the part left behind will remain at rest and unstrained. In the case supposed things will happen in this way if the end-piece is more than 5 inches long. If it be less than 5 inches long, say 4 inches, there will, on the arrival of the reflected wave at the joint, be still 2 inches of pressure wave in the other part of the rod, and the corresponding quantity of momentum. In this case, therefore, only a portion of the whole momentum is trapped in the piece, the balance being left in the other part of the rod, which moves forward with the corresponding velocity. In order to discover how long the pressure lasts it is only necessary to try a series of experiments with the joint at different

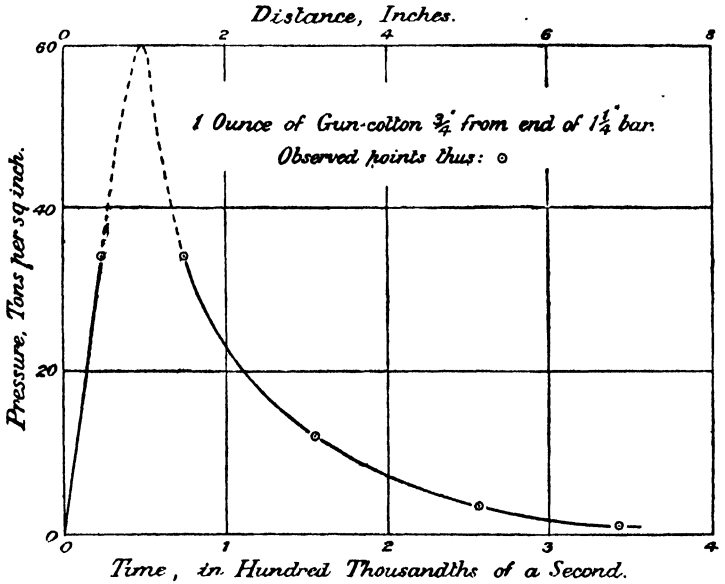


Fig. 7.

distances from the free end. It will be found that if that distance exceeds a certain amount, the rod which was originally struck remains at rest, the whole momentum being transferred to the free end-piece. If the distance be less, only a fraction of the momentum is so transferred, and the balance remains in the struck rod, which accordingly moves forward.

I have applied this method to investigating the curve of pressure developed by the detonation of gun-cotton. I will not weary you with the details, but the results, so far as I have obtained them, are shown in the curve (Fig. 7). This exhibits the pressure produced by the detonation of one ounce of dry gun-cotton on the end of a steel rod, distant three-quarters of an inch from it. With the gun-cotton hard up against the steel the pressures are probably about twice as great, and the curve similar in form. It is, of course, impossible to do this experiment within the walls

of this lecture room, the explosion, while not very violent, is sufficiently so to necessitate its being carried out in the open. But I can readily show you a very similar experiment, in which the pressure is produced, not by the detonation of gun-cotton, but by the impact of a small rifle bullet. [Experiment shown.]

I have on the table some specimens to show the effects of detonating larger quantities of gun-cotton. Here is a steel plate which has been broken by firing a charge of about 1 lb. in contact with it. It is interesting to note the character the fracture produced. This plate is a good quality of mild steel, such as is used for making boilers. It would be possible by a steadily applied pressure to bend it double without fracture, yet as the effect of the blow delivered by the gun-cotton it is broken with very little bending, almost as though it were cast iron or very hard steel. Time will not permit of my going further into the interesting question, of course a very important one in connection with our subject, of the effect on the character of the fracture produced of very big stresses lasting for a very short time. This case of the fracture of mild steel by gun-cotton shows, however, that one result may be that the property of ductility largely disappears under the action of a sufficiently violent blow. The mild steel, in fact, behaves very much like sealing-wax, or pitch. The stick of sealing-wax which I hold in my hand has been bent by the continued action of a small force acting for several days, and there can be no doubt that the same force, had it continued to act, would ultimately have bent it double without breaking it. Yet under the application of a force many times as great, it snaps like a piece of glass.

The pressures produced by the detonation of gun-cotton are of the same order of intensity as those developed in ordinary blows. We saw that in the impact of billiard balls the average pressure over the area of contact may reach a value of 27 tons per square inch, and with steel balls moving at quite small velocities, such as 2 or 3 feet per second, it is easy to get pressures of 100 tons per square inch or more. These pressures, however, are very local, the area over which they act being a few hundredths of an inch in diameter only. By means of gun-cotton similar pressures may be applied over any desired area; but the intensity is no greater. About 120 tons per square inch is probably the limit of simple static gaseous pressures produced by known practical explosives. Probably greater pressures are produced with fulminate, but that cannot be used except on a very small scale. For the production of destructive effects on hard steel greater pressures than this are required, and in order to develop them on any considerable scale we must again have recourse to the dynamic action of collision.

We have already seen that a lead bullet moving at 1800 feet per second probably generates a pressure of 200 tons per square inch or more. We went on to consider the impact of rods of hard metal, and it appeared that

two rods of steel colliding end on with a relative velocity of 34 feet per second would develop a pressure of about 13 tons per square inch over the whole section of either. The theory on which that conclusion is based has been subjected to experimental test—indirect, it is true, but sufficiently searching—and is certainly correct for velocities and pressures of that order. According to the theory the pressure is simply proportional to the relative velocity of the two rods, so that if they collided at 2000 feet per second, that is sixty times as fast, the pressure would be 780 tons per square inch, assuming that the theory continues to hold under these very different conditions.

One of the fundamental assumptions on which the theory is based, however, would certainly break down long before such a velocity was reached. That assumption is that the pressure leaves no permanent effect on the material. I do not know what is the strongest steel for this purpose which has been produced, but I think it may safely be asserted that no known substance would stand an end compression, such as results from the blow of the colliding rods, of more than 300 tons per square inch. If it were ductile it would flow so rapidly under this pressure that there would be appreciable deformation even in the very short time during which the pressure lasts. If it were very hard it would be instantly shattered. In both cases the circumstances of pressure transmission would be completely altered. It is, however, fairly certain that in neither would the pressure exceed that calculated on the hypothesis of perfect elasticity, and that in both it would be greater than that calculated (as for the lead rifle bullet) on the hypothesis of no elasticity.

I am afraid, therefore, that at present our theories can throw but little light on the interesting question of the pressure developed when a hard steel armour-piercing shell strikes a hard steel plate with a velocity of 2000 feet per second. But a consideration of the visible effects of such a blow is suggestive in many ways, and by the kindness of Sir R. Hadfield I am able to describe and show some of them to you to-night.

You see before you specimens of modern armour-piercing shot, and their essential features are shown on the drawing. The shell is made of a special steel of great strength and considerable ductility, and after manufacture the point is hardened by thermal treatment, the base and most of the body of the shell remaining more or less ductile. In recent years it has become the practice to fit a cap of soft steel over the hardened point. I will speak of the functions of this cap later, and for the present we will consider the shell without it.

I first show the effect of firing an uncapped shell at a plate of wrought iron or mild steel (Plate II, Fig. 8). In this case the metal of the plate is so soft that pressures that are quite without effect on the hardened point of the shell are able to make it flow very rapidly. The shell simply ploughs its way through, pushing out the wrought iron before it, and emerges quite

unscathed. It will be noticed that on the striking side there is a rim or lip of wrought iron which has been squeezed out in a direction opposite to the movement of the shell. A similar lip is formed if a hole is blown in a lead plate by means of a gun-cotton primer, and there seems to be a good deal of analogy between the two cases.

Completely to stop a 14 inch shell, such as that which you see before you, would require a thickness of at least 2½ feet of wrought iron, and almost as great a thickness of mild steel. I believe that some ships twenty-five years ago were fitted with armour of this sort of thickness, but, of course, the weight is almost prohibitive. Modern improvements in armour, whereby the same effective resistance is obtained with less than half the thickness, are based on the use of special steel having sufficient ductility to enable it to be worked and fixed in place on the ship while possessing greater strength than wrought iron or ordinary structural steel. Even such a special steel, however, is handicapped as against the shell by the hard point of the latter, which is able to force the softer material aside though itself undamaged. This disability has been overcome by hardening the face of the plate so that it now possesses the structure indicated in the drawing, the back being tough and ductile, but the face as hard as it is possible to make it. When such a plate is struck by the shell it is a case of Greek meeting Greek, and this is the result (Plate III, Fig. 9, rounds 583 and 588). Both the shell and the hardened face of the plate are shattered by the pressure, sufficient of which is transmitted through the substance of the plate to crack it right through, though, of course, none of the shell has penetrated it.

It would seem that when it acquired the hard face the armour plate more than overtook the shell in the race. Though the shell might by sheer energy pierce a somewhat thinner plate, I am told that it was apt to be smashed to pieces in the process. The balance has of recent years been more than restored by the addition to the shell of the soft steel cap. I have already shown you the effect of firing an uncapped shell; I will now draw your attention to that of firing the same shell with cap at the same plate (Fig. 9, round 587). The shell goes through minus its cap, but otherwise so completely uninjured, that I am told it might in many cases be used again. It punches a clean hole in the plate. The fate of the cap is interesting. The shell punches a hole in it, as of course it must do before it reaches the plate, and the cap forms a ring, which is held up by the plate and through which the shell passes. The fragments of the cap are found on the front side of the plate, and in some instances they have been collected and put together, forming a ring. I have one such ring here. Its largest diameter is that of the shell, its smallest about an inch less, and it looks as though the ring had got intact as far as the shoulder of the projectile, but had then burst into several pieces.

The usual explanation of this remarkable effect of a soft steel cap is

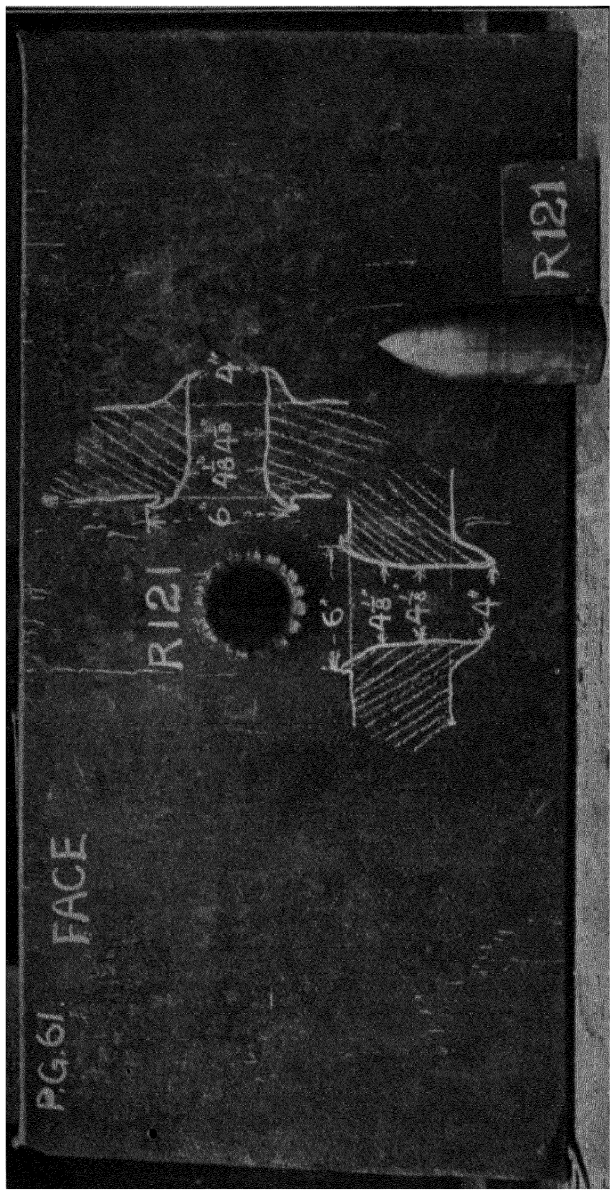
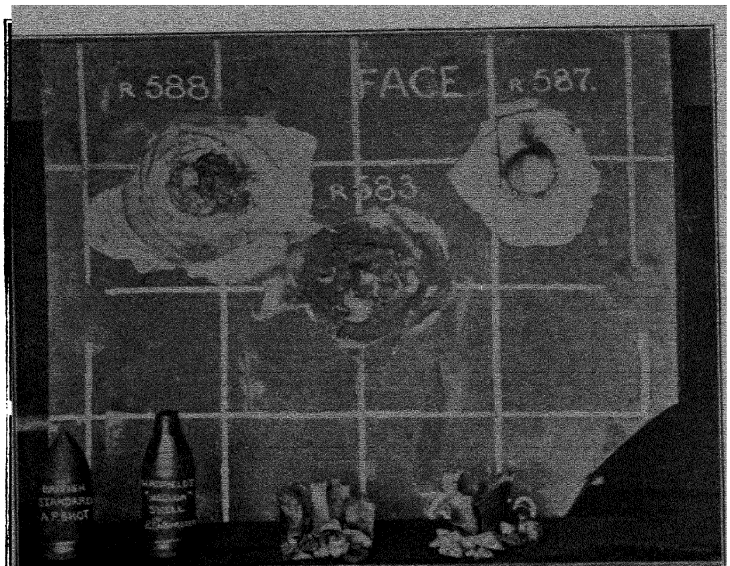


Fig. 8

2. TRIAL OF HADFIELD'S "HECLON" 10·5 C.M. (4¹ IN.) ARMOUR-PIERCING SHELL OF 32¹ LBS.



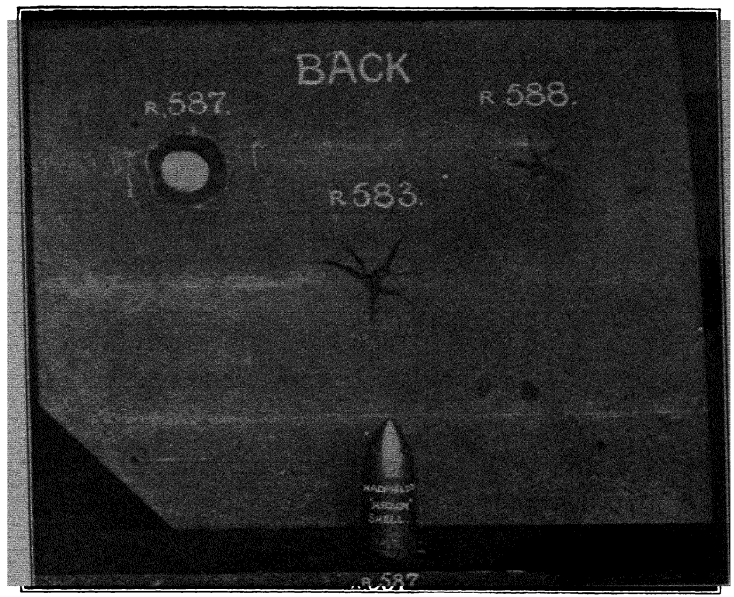
TEST OF PLATE.

1. Round 583. Hadfield's Cast Steel A.P. Shot (British Standard) Uncapped.

Striking Velocity	Energy—Foot-tons.	Factor of Penetration
2010	920	2.32

Round 588. Hadfield's Cast Steel A.P. Shot (British Standard) Uncapped.

Striking Velocity.	Energy—Foot-tons.	Factor of Penetration
1920	840	2.18



TEST OF PROJECTILE

2. Round 587. Hadfield's "HECLON" Cast Steel A.P. Shell, Capped.

Striking Velocity.	Energy Foot-tons	Factor of Penetration
1920	840	2.18

No. 872.

Fig. 9.

that it supports the point of the projectile. As I pointed out in connection with billiard balls, the destructive effect of pressure depends on the difference of pressures in different directions, and not on their absolute amounts, and it is obvious that by the exercise of a sufficient lateral pressure the point might be completely protected. The difficulty is to see how the comparatively weak material of which the cap is made can exert the very large pressures which are necessary for effective support. It seems hardly possible that such pressures could be generated by the mere act of stretching or expanding the cap over the end of the shell. If this be so, the inertia of the metal in the cap must play an important part. At the critical moment when the hard point of the shell meets the plate, there is a sudden distortion of the shell and plate near the point of contact. This distortion is the cause of breakage. One can see that the mass of mild steel surrounding the point of the shell, and pressed into firm contact with it, might by its inertia oppose a powerful resistance to this sudden change of form, and so support the shell during the minute fraction of time which determines whether it or the plate shall go.

27

A METHOD OF MEASURING THE PRESSURE PRODUCED IN THE DETONATION OF HIGH EXPLOSIVES OR BY THE IMPACT OF BULLETS.

(Abstract: from "Proc. Roy. Soc.")

If a rifle bullet be fired against the end of a cylindrical steel rod, or some gun-cotton be detonated in its neighbourhood, a wave of pressure is transmitted along the rod with the velocity of sound. If the pressure in different sections of the rod be plotted at any instant of time, the abscissae being distances along the rod, then at a later time the same curve shifted through a distance proportional to the time will represent the then distribution of pressure. Also the same curve represents the relation between the pressure across any section of the rod and the time, the scale of time being approximately 2 inches for 10^{-5} seconds. In particular it represents the relation between the total pressure applied to the end of the rod and the time, and the length of the curve represents the total duration of the blow.

If the rod be divided at a point a few inches from the far end, the opposed surfaces of the cut being in firm contact and carefully faced, the wave of pressure travels practically unchanged through the joint. At the free end it is reflected as a wave of tension, and the pressure at any section is then to be obtained by adding the effects of the pressure wave and the tension wave. At the joint the pressure continues to act until the head of the reflected tension wave arrives there. If the tail of the pressure wave has then passed the joint the end-piece flies off, having trapped within it the whole of the momentum of the blow, and the rest of the rod is left completely at rest. The length of end-piece which is just sufficient completely to stop the rod is half the length of the pressure wave, and the duration of the blow is twice the time taken by the pressure wave to travel the length of the end-piece. Further, it is easy to see, as is proved in detail in the paper, that the momentum trapped in quite short end-pieces will be equal to the maximum pressure multiplied by twice the time taken by the wave in traversing the end-piece. Thus by experimenting with different lengths of end-pieces and determining the momentum with which each flies off the rod as the result of the blow it is possible to measure both the duration of the blow and the maximum pressure developed by it. This is the basis of the experimental method described in the paper. A steel rod is hung up as a ballistic pendulum, and the piece is held on to the end by magnetic attraction. A bullet is fired at the other end, and the end-piece is caught in a ballistic pendulum and its momentum measured. The momentum of the rod is also measured.

Most of the experiments described in the paper were made with lead bullets with the object of checking the accuracy of the method. On the assumption that a lead bullet behaves on impact as a fluid the time taken completely to stop it, which is the duration of the blow, is equal to the time which it takes to

travel its own length, and the maximum pressure is equal to the mass per unit of length in the section of greatest area multiplied by the square of the velocity. The experiments showed good agreement between the observed and calculated values of the maximum pressure as is shown in the following table:

Velocity of bullet	Maximum pressure	
	Calculated	Observed
ft./sec.	lbs.	lbs.
2000	43,500	42,600
1240	15,700	16,700
700	5,450	5,320

The observed duration of the blow is in the case of the highest velocity about 6 per cent. greater than the time taken by the bullet to travel its own length. This discrepancy is to be accounted for partly by the fact that the bullet is really not absolutely fluid, but is also in part due to the non-fulfilment of some of the conditions postulated in the simple theory of the method. It seems probable that the principal source of error of the latter kind is that the pressure applied by the bullet is not uniformly distributed over the end. Experiments with rods of different diameter show that the larger ones give larger estimates of the duration of the impact.

Having established by experiments on lead bullets that the method of experiment is capable of giving within a few per cent. both the maximum pressure and the duration of very violent blows, experiments were next made on the detonation of gun-cotton. Cylinders of dry gun-cotton $1\frac{1}{4}$ inch $\times$ $1\frac{1}{4}$ inch and weighing about 1 oz. were detonated with fulminate at a distance of about $\frac{3}{4}$ inch from the end of the steel rod. The results may be expressed by saying that the average value of the pressure during a period of 10^{-5} seconds in the neighbourhood of the maximum is about thirty tons per square inch. The absolute maximum is of course considerably higher. The pressure has practically disappeared in 1/50,000 second, that is at least 80 per cent. of the impulse of the blow has been delivered within that time. Experiments were also made with gun-cotton in contact with the rod, but owing to the permanent deformation of the steel, which would have the effect of deadening the blow, the results in this case cannot claim to be precise. They lead, however, to the conclusion that the maximum pressure at the surface of contact is at least double what it is when an air-space $\frac{3}{4}$ inch thick is interposed.

The results obtained for gun-cotton, though lacking in precision, throw some light on the nature of the fracture which is produced by the detonation of this explosive in contact with a mild steel plate. They show that the pressure of the gun-cotton may be regarded as an impulsive force in the sense that only very small displacement of the steel occurs during its action. Its effect is to give velocity to the parts of the plate with which it is in contact, the remainder being left at rest. In a plate 1 inch thick the velocity given by a slab of gun-cotton of about the same thickness is roughly 200 feet per second. The resulting strain depends upon the ratio of this velocity to the velocity of propagation of waves of stress into the material, and, assuming perfect elasticity, shearing stresses of the order of 100 tons per square inch may be produced in a plate of this

thickness. In static tests on mild steel the metal flows when the shearing stress is of the order of 10 tons per square inch, and no materially greater stress can exist. But if the rate of straining is sufficient, the viscosity of the flowing metal becomes important, and the shearing stress may approximate to the value corresponding to perfect elasticity. The shearing stress is accompanied by tension, which under such circumstances may be sufficient to break down the forces of cohesion. Thus the steel is cracked in spite of its ductility, just as pitch may be cracked by the blow of a hammer. From the measured duration of the pressure produced by gun-cotton it may be inferred that the velocity of shear required to crack mild steel is of the order of 1000 radians per second.

The shattering of the plate by the gun-cotton probably occurs during the time that the pressure is acting—that is within two or three hundred-thousandths of a second—and before the plate has had time to be sensibly deformed. The bending of the broken pieces, which is always found when mild steel is broken in this way occurs subsequently, and is due to the relative velocities which remain in the different parts of each piece of the plate after the plate has been broken and the pressure has ceased to act.

["Philosophical Transactions," 1913.]

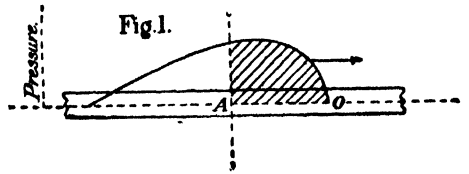
The determination of the actual pressures produced by a blow such as that of a rifle bullet or by the detonation of high explosives is a problem of much scientific and practical interest but of considerable difficulty. It is easy to measure the transfer of momentum associated with the blow, which is equal to the average pressure developed, multiplied by the time during which it acts, but the separation of these two factors has not hitherto been effected. The direct determination of a force acting for a few hundred-thousandths of a second presents difficulties which may perhaps be called insuperable, but the measurement of the other factor, the duration of the blow, is more feasible. In the case of impacts such as those of spheres or rods moving at moderate velocities the time of contact can be determined electrically with considerable accuracy*. The present paper contains an account of a method of analysing experimentally more violent blows and of measuring their duration and the pressures developed.

If a rifle bullet be fired against the end of a cylindrical steel rod there is a definite pressure applied on the end of the rod at each instant of time during the period of impact and the pressure can be plotted as a function of the time. The pressure-time curve is a perfectly definite thing, though the ordinates are expressed in tons and the abscissae in millionths of a second; the pressure starts when the nose of the bullet first strikes the end of the rod and it continues until the bullet has been completely set up or stopped by the impact. Subject to qualifications, which will be considered later, the result of applying this varying pressure to the end is to send along the rod a wave of pressure which, so long as the elasticity is perfect, travels without change of type. If the pressure in different

* Sears, *Proc. Camb. Phil. Soc.*, vol. 14 (1917), p. 257, and references there given.

sections of the rod be plotted at any instant (Fig. 1) then at a later time the same curve shifted to the right by a distance proportional to the time will represent the then distribution of pressure. The velocity with which the wave travels in steel is approximately 17,000 feet per second. As the wave travels over any section of the rod, that section successively experiences pressures represented by the successive ordinates of the curve as they pass over it. Thus the curve also represents the relation between the pressure at any point of the rod and the time, the scale being such that one inch represents the time taken by the wave to travel that distance which is very nearly 1/200,000 second. In particular the curve giving the distribution of pressure in the rod along its length is, assuming perfect elasticity, the same as the curve connecting the pressure applied at the end and the time, the scale of time being that just given.

Fig. 1.



The progress of the wave of stress along the rod is accompanied by corresponding strain and therefore by movement. It is easy to show that the same curve which represents the distribution of pressure at any moment also represents the distribution of velocity in the rod, the scale being such that one ton per square inch of pressure corresponds to about 1·3 feet per second of velocity. Until the wave reaches any section of the rod that section is at rest. It is then, as the wave passes over it, accelerated more or less rapidly to a maximum velocity, then retarded, and finally left at rest with some forward displacement. In this manner the momentum given to the rod by the application of pressure at its end is transferred by wave action along it, the whole of such momentum being at any instant concentrated in a length of the rod which corresponds, on the scale above stated (one inch = 1/200,000 second), to the time taken to stop the bullet completely. Consider a portion of the rod to the right of any section A (Fig. 1) which lies within the wave at the moment under consideration. The pressure has been acting on this portion since the wave first reached it, that is for a time represented by the length OA and equal to $\frac{OA}{V}$ where V is the velocity of propagation. The momentum which has been communicated to the part under consideration is equal to the time integral of the pressure which has acted across the section A, that is to the shaded area of the curve in the figure. The portion of the rod to the right of the section is continually gaining momentum at the expense of the portion to the left while the wave is passing, the rate of transfer at any instant being equal to the pressure.

When the wave reaches the free end of the rod it is reflected as a wave of tension which comes back with the same velocity as the pressure wave,

and the state of stress in the rod subsequently is to be determined by adding the effects of the direct and of the reflected waves. Now suppose that the rod is divided at some section, B, near the free end (Fig. 2), the opposed surfaces of the cut being in firm contact and carefully faced. The wave of pressure travels over the point practically unchanged and pressure continues to act between the faces until the reflected tension wave arrives at the joint. The pressure is then reduced by the amount of the tension due to the reflected wave and as soon as this overbalances at section B the pressure of the direct wave (which is the moment shown in the figure) the rod, being unable to withstand tension at the joint, parts there and the end flies off. The end piece has then acquired the quantity of momentum represented by the shaded area in the figure, equal to the time-integral of the pressure curve from O to B, less that of the tension wave during the time for which it has been acting, that is from O' to B. The piece flies off with this amount of momentum trapped, so to speak, within it. If it be caught in a ballistic pendulum and its momentum thus measured we have the time integral of the pressure curve between the points B and B' on the pressure-time curve which are such that they correspond to equal pressures on the rising and falling parts of the curve, while the time-interval between them is equal to that required for a wave to travel twice the length of the end-piece. By taking end-pieces of different lengths and measuring the momentum so trapped in each the area of the pressure-time curve over corresponding intervals can be obtained. In general the precise form of the curve itself cannot be deduced because the points of commencement of the several intervals are not known. Thus a given set of observations would be consistent with any one of the three forms shown in Fig. 3 which can be derived from one another by shearing parallel to the base so that the intercept of any line such as AA' is the same on all. But the maximum pressure and the total duration of the impact can always be obtained, and these are the most important elements. The maximum pressure is the limiting value of the average acting on a piece when the piece is very short, and the duration corresponds to twice that length of piece which just catches the whole of the momentum leaving the rod at rest. If the circumstances of the impact are such that the pressure is known to rise or to fall with great suddenness, the curve assumes the form I or III and its form may be determined completely from the observations.

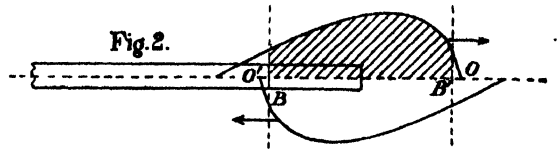


Fig. 2.

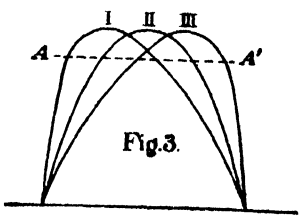


Fig. 3.

This is the basis of the method described in the present paper. A

cylindrical rod or shaft of steel is hung up horizontally by four equal threads so that it can swing in a vertical plane remaining parallel to itself. A short piece of rod of the same diameter is butted up against one end being held on by magnetic attraction but otherwise free. A rifle bullet is fired at, or gun-cotton is detonated near, the other end; the short piece flies off and is caught in a box suspended in a similar manner to the long rod. Suitable recording arrangements register the movement both of the long rod and of the box, and the momentum in each is calculated in the usual way as for a ballistic pendulum. Sufficient magnetic force to hold the end-piece in position is provided by putting a solenoid round the rod in the neighbourhood of the joint. The slight force required to separate the piece from the rod under these conditions may be neglected in comparison with the pressures and tensions set up, since these amount to several tons on the square inch, and, practically speaking, the joint will transmit the pressure wave unchanged but will sustain no tension.

Pressure Produced by the Impact of Lead Bullets.

The pressure which should be produced by the impact of a lead bullet can be predicted theoretically, and the study of this pressure was made rather with a view to checking the method than in the hope of discovering any new facts. At velocities exceeding 1000 feet per second lead behaves on impact against a hard surface practically as a perfect fluid.

The course of the impact is shown in Fig. 4. The base of the bullet at the moment of striking is at A; a little later it is at B. Assuming perfect fluidity the base of the bullet knows nothing of the impact at the nose and continues to move forward with unimpaired velocity. Hence the time elapsing between the two positions shown in the figure is $\frac{AB}{V}$. The momentum which has been destroyed up to this time is to a first approximation that of the portion of the bullet which has been flattened out, namely that portion shown shaded in the dotted figure. Knowing the distribution of mass along the length this is easily calculated. This simple theory is subject to some qualifications due partly to want of perfect fluidity, and partly to the fact that the sections of the bullet are not brought right up to the face and there stopped dead, as is assumed in the theory, but are more or less gradually retarded or deflected in the region of curved steam-lines at C. These corrections are, however, most conveniently introduced when comparing the theory with the experimental results.

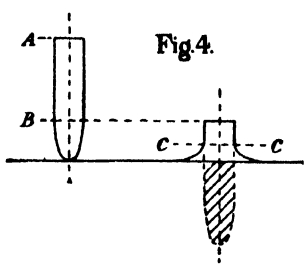


Fig. 4.

The bullets used were of two patterns, one the ordinary service form (Mark VI) and the other a soft-nosed bullet supplied on the market for sporting purposes. Both are of lead, encased in nickel. Sections of the

bullets are shown in Fig. 5*. Sample bullets were sawn into sections, and the sections weighed. The distribution of weight along the length thus determined is shown in the curve Fig. 5. The bullets were almost precisely alike both in regard to total weight (0·0306 lb.) and distribution of weight along the length.

Most of the experiments were made with the service cartridge, in the service rifle, giving an average velocity of 2000 feet per second. These

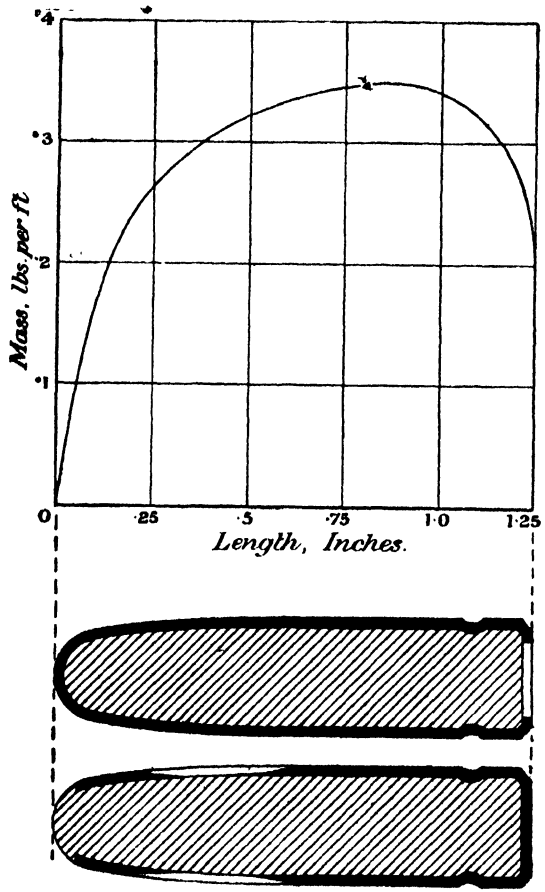


Fig. 5.

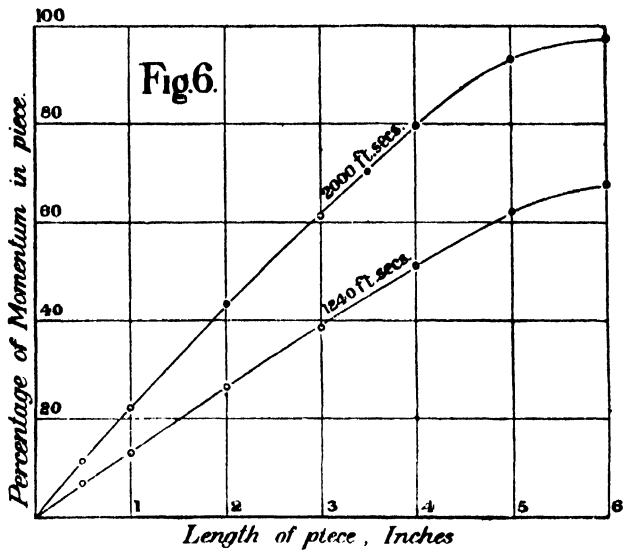
cartridges were very uniform, the range of variation in velocity being under 1 per cent. Some experiments were also made with cartridges giving velocities of about 1240 feet per second and 700 feet per second respectively.

The rod against which the bullet was fired was in most cases of steel containing C, 0·4 per cent.; Mn, 1·05 per cent. Its breaking strength was 37 tons per square inch with 24 per cent. elongation over 8 inches. The

* The soft-nosed bullet (lower figure) has four longitudinal saw-cuts in the nickel casing; the section is taken through two of these cuts.

end of the rod was heated to a white heat in the forge and quenched and would then stand a large number of shots without serious damage. In some cases tool steel hardened, and tempered blue, was used, but it was found difficult to get the temper exactly right. The pieces butted to the end of the rod were usually of mild steel. For recording the movement of the rod and of the box in which the piece was caught each was fitted with a pencil which moved over a horizontal sheet of paper and the length of the mark was measured.

Assuming that the bullet strikes the rod fairly in the centre, and that the fragments are shot out radially, the total momentum recorded in rod and piece should be equal to the momentum of the bullet, which at 2000 feet per second is 61·2 lbs. feet per second units. In fact, considerable



variations were found in the total momentum. For instance, in 110 shots fired at a 1 inch rod, the maximum total recorded was 76, the minimum 50, and the average 63. With a rod of $1\frac{1}{2}$ inches diameter, the variation was less; 61 shots showed a minimum of 59, a maximum of 70, and a mean of 62·5. High values are probably due to fragments being thrown back by irregularities in the surface of the rod, low values to slight errors in aiming. It was found, however, that with a piece of given length, the total momentum was shared between the piece and the rod in a nearly constant proportion, though the absolute values might vary widely. This is to be expected if the explanation just given of the irregularities is correct. For instance a cup-shaped cavity in the rod such as is formed after a large number of shots will give a high value for the momentum, but if not too pronounced it will not seriously affect the form of the relation between pressure and time.

The results have accordingly been reduced by taking in every case the percentages of the total momentum found in the piece. The following table gives details of one set of experiments. It was found that there was no systematic difference between the service bullets and the soft bullets, and the results for both types are included in the table:

Rod, 1 inch diameter, 43 inches to 50 inches long. 2000 feet per second.

Length of piece	Number of shots	Percentage of total in piece			Total momentum in rod and piece		
		Maximum	Minimum	Mean	Maximum	Minimum	Mean
inches							
0·5	19	11·6	9·8	10·9	63	58	60
1·0	25	24·0	20·4	22·1	66	58	62
2·0	8	46·0	40·6	43·2	73	60	65
3·0	26	63·0	58·0	61·0	66	59	62
3·5	6	71·0	69·0	70·4	71	65	67
4·0	6	82·0	79·0	79·7	67	50	62
5·0	11	93·0	94·5	93·5	76	59	67
6·0	9	99	—	97·6	69	63	66

The mean percentages given in the third column of the table are plotted against length of piece in Fig. 6. As the wave travels 2·04 inches in 10^{-5} seconds, 1 inch length of piece represents $0{\cdot}98 \times 10^{-5}$ seconds*. The slope of this curve represents pressure, and as already explained the maximum pressure is represented by the slope at the origin. This is 22 per inch, and assuming an average total momentum of 61·2 units the corresponding pressure is

$$\frac{0{\cdot}22 \times 61{\cdot}2 \times 10^5}{32{\cdot}2 \times 0{\cdot}98} = 42{,}600 \text{ lbs. or } 19{\cdot}0 \text{ tons.}$$

It will also be noticed that the impact is practically complete in 6×10^{-5} seconds, 97½ per cent. of the total being then accounted for in the piece.

According to the simple theory, which regards each element of the bullet as coming up to the end of the rod with its velocity v_0 unimpaired and there suffering instant stoppage, the pressure at any time is λv_0^2 where λ is the mass per unit length at the section which is undergoing stoppage at the time. The pressure-time curve, calculated in this way, is shown in Fig. 7, in which the ordinates are proportional to the values of λ. This is the same curve as that giving the distribution of mass along the length of the bullet, the abscissa scale being such that the length OF within which the impact is complete is equivalent to the time required by the bullet

* The value of E for the mild steel of which the pieces were made was found to be $3{\cdot}00 \times 10^7$ lbs. per square inch. The density was 482 lbs. per cubic foot. Both determinations are probably right within 1 per cent. The velocity of propagation $\sqrt{\frac{E}{\rho}}$ is 17,000 feet per second.

to travel its own length (1·25 inches) at a velocity of 2000 feet per second. This is $5{\cdot}2 \times 10^{-5}$ seconds. The maximum pressure corresponds to the maximum value of λ (0·35 lb. per foot) and is

$$\frac{0{\cdot}35 \times 2000 \times 2000}{32{\cdot}2} = 43{,}500 \text{ lbs.}$$

which is $2\frac{1}{2}$ per cent. in excess of the value found by experiment. This difference is no more than can be accounted for by errors of observation.

The momenta which should according to theory be taken up by various lengths of piece are readily calculated from this curve. For instance, that corresponding to a 3 inch piece is the area *ABCDE*. The following table

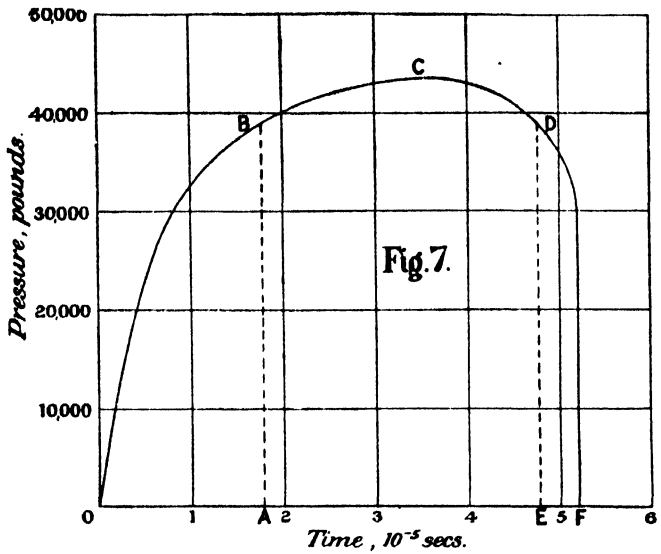


Fig. 7.

shows the results so obtained with the corresponding observed values. The momenta are reckoned as percentages of the total:

Length of piece	Percentage momentum in piece	
	Calculated	Observed
inches		
3	65	61
4	84	80
5	98·5	93·5
6	100	97·5

The differences between the calculated and observed figures in this table are probably rather outside experimental errors. Especially is this the case as regards the 5 inch and 6 inch pieces. The impact seems to last appreciably longer than it ought.

The Effect of the Rigidity of the Bullet.

In the simple theory it is assumed that the bullet is absolutely fluid. In fact, it possesses a certain rigidity, partly because of the nickel casing and partly because of the viscosity of the lead the effects of which may be quite appreciable at such high speeds of deformation. The general effect of rigidity may be represented by saying that any section of the bullet requires to be subjected to an end-pressure P before it begins to deform at all, and this pressure must act across the section CC (Fig. 4) where deformation is just beginning and where, if the bullet were really fluid, there would be no pressure. To a first approximation, P will be proportional to the area of the cross-section of the bullet which is undergoing deformation, that is to λ the mass per unit length in the plane CC. The pressure P is added to that due to the destruction of momentum, making a total pressure $P + \lambda v^2$ where λ is the mass per foot of the section of the bullet in the plane CC, v the velocity of that section. Further, the part of the bullet behind CC is being continually retarded by the pressure P, with the result that the hinder parts do not come up with unimpaired velocity v_0, as they would if the bullet were quite fluid, but with a diminishing velocity.

The general effect of this is obvious. In the early stages of the impact there has not been time for much retardation, and the pressure will be increased above the theoretical value by nearly the amount P. As the hinder parts come up, however, with less and less velocity, the fluid pressure term diminishes until the pressure falls below the theoretical value in spite of the rigidity term P. Applying this correction to a pressure curve such as that in Fig. 7 in which the maximum pressure occurs somewhat late in the impact, it will be seen that the general effect will be to reduce that maximum, and also to make it flatter. Furthermore, since the tail of the bullet takes longer to reach the end of the rod, the impact will be prolonged beyond the theoretical time.

It is easy to get a rough idea of the magnitude of these effects. Assume that the bullet is cylindrical and of mass λ per unit length and that the deforming pressure is constant. Let x be the length of the bullet behind the plane CC (Fig. 4). This portion is moving as a rigid body with acceleration $\ddot{x}$ and its equation of motion is

$$\lambda x \ddot{x} = P,$$

which integrates in the form

$$\tfrac{1}{2}\dot{x}^2 = \frac{P}{\lambda} \log x + \text{const.}$$

If l be the length of the bullet and v_0 its velocity on striking, and if we neglect the small distance between the plane CC and the end of the rod, the constant of integration is

$$\tfrac{1}{2}v_0^2 - \frac{P}{\lambda} \log l,$$

and we have

$$1 - \frac{\dot{x}^2}{v_0^2} = \frac{2P}{\lambda v_0^2} \log \frac{l}{x}.$$

From this $\dot{x}$ can be plotted in terms of x, and thence in terms of t. The total pressure $P + \lambda \dot{x}^2$ is then plotted in terms of the time.

As an example, take $\lambda = 0{\cdot}35$ lb. per foot, $l = 1{\cdot}05$ inches which correspond to a bullet having the same mean density diameter and total mass as those used in the experiments. The pressure required to stop such a bullet at 2000 feet per second, if fluid, would be constant and equal to 43,500 lbs. If P be taken as $\frac{1}{20}$ of this, or 2170 lbs., and the curve plotted as described, it will be found that when $x = 0{\cdot}3l$ the hydrodynamical pressure λv^2 has dropped 12 per cent. making, after allowing the addition of 5 per cent. for the rigidity, a nett drop of 7 per cent. Furthermore, the momentum still left after a fluid bullet would have been completely set up is about 4 per cent. of the whole.

If corrections of this amount were applied to the calculated figures in the last section, the effect would be to make the observed maximum pressure about 4 per cent. too high, while the observed time of impact would be still slightly too long. It was found that to crush the cylindrical part of the service bullet in a testing machine required an end pressure of about 1800 lbs., but the nickel casing failed by buckling, whereas in the impact it apparently bursts and is torn into strips along the length of the bullet. The pressure required to deform the bullet in the latter case, after rupture is once started, is probably less than 2000 lbs. Thus, while the difference between the observed and calculated times of impact may undoubtedly be referred in part to rigidity, it is unlikely that the whole can be accounted for in this way.

Discussion of Errors Inherent in the Method of Experiment.

In calculating the pressure from the momentum in the piece which is thrown off the end of the rod it is assumed that the pressure wave transmitted along the rod represents exactly the sequence of pressures applied at the end, that it travels along the rod and through the joint without change of type, and that it is perfectly reflected at the other end. These assumptions are correct if the wave is long compared with the diameter of the rod, and if the pressure is uniformly distributed over the end, but are subject to certain qualifications in so far as these conditions are not fulfilled.

(*a*) *Effect of Length of the Rod.* The mathematical theory of the longitudinal oscillations of a cylinder shows that a pressure wave of simple harmonic type is propagated without change, but the velocity of propagation depends on the wave-length. Because of the kinetic energy involved in the radial displacements, which is negligible when the wave is long

compared with the diameter, the velocity diminishes with the wave-length. If the wave-length be $\frac{2\pi}{\gamma}$, and if the radius of the cylinder be a, the velocity is $\sqrt{\frac{E}{\rho}}(1 - \frac{1}{4}\sigma^2\gamma^2a^2)$ correct to the square of γa*. In a wave of any form, the simple harmonic components move with different velocities, and the wave accordingly changes its form as it progresses.

Rough calculation of this effect on waves generally similar in form to that produced by the blow of the bullet, but of periodic character, showed that the change should not be very serious with rods of the lengths and diameters used in these experiments. It was, however, thought advisable to check this inference by direct experiment, and trials were therefore made with a rod 15 inches long and 1 inch diameter. The small mass of this rod precluded its use as a ballistic pendulum suspended in the ordinary way, it was therefore arranged to slide in bearings and to compress a spring buffer. Difficult questions arose as to the precise allowance which should be made for the kinetic energy given to the spring (which was of considerable mass) by the rod, and no attempt was therefore made to get an accurate measure of the total momentum. Instead of taking the fraction of this total which was trapped in the piece, the absolute values of the momenta so trapped were taken in a series of shots, in each of which, from the accuracy of the aiming and the absence of cupping in the end, it might be assumed that the total momentum was approximately equal to the average. The results are shown in the following table and are compared with the corresponding figures obtained with the long rod:

Rod, 1 inch diameter. 2000 feet per second.

Length of piece	Number of shots	Momentum given to piece			
		Short rod (15 inches)			Long rod
		Mean	Maximum	Minimum	Mean
inches					
$\frac{1}{2}$	7	6·5	6·8	6·4	6·7
1	5	13·3	13·9	12·8	13·5
2	2	26·5	26·8	26·2	26·4
4	6	49·3	51·2	48·6	48·8
5	2	60·2	61·3	59·1	57·2

It is clear from these figures that there is no systematic difference between the results obtained with the two rods. The change, if any, between the forms of the wave when at 15 inches and at 45 inches from

* Love, *Mathematical Theory of Elasticity*, 2nd edition, p. 277.

the end consists in a shearing of the whole curve as in the manner illustrated in Fig. 3. Such a change of form—analogous to the change preparatory to breaking which a wave experiences as it advances into shallower water—would not be detected by these experiments, and it is not impossible that it occurs to some extent.

(*b*) *Reflection and Effect of the Joint.* The simple harmonic pressure wave which is propagated without change of type is accompanied by a distribution of shearing-stress across the section. This shearing-stress depends on the square of the ratio γa, and is small. That it plays no important part in these experiments is shown by the fact that if there be a joint in the long rod the results are unaltered. Such a joint transmits the pressure, but stops the shearing-stress part of the wave. As might be expected, it was found that the faces of the joint must be a carefully scraped fit if the wave is to pass it unaltered.

The small magnitude of the shearing-stress is the foundation of the assumption that the wave is perfectly reflected at the free end. Strictly accurate reflection is not possible. A reflected wave which is exactly the same as the incident wave, except that the signs of all the stresses are reversed, will when combined with the incident wave give no normal force over the free end. The shearing-stresses corresponding to the two waves do not, however, neutralize each other, but are added, hence accurate reflection can only be brought about by the application of a distribution of shear over the free end. The shear required is, however, of the order $\gamma^2 a^2$ and the experiment with the joint shows that its effects may be neglected.

2000 feet per second.

Length of piece	Percentage of momentum in piece		
	¾ inch	1 inch	1½ inches
inches			
0·5	10·8	10·9	10·35
1·0	21·1	22·1	22·0
2·0	—	42·2	40·5
3·0	61·3	61·2	60·2
4·0	79·5	79·7	78
5·0	92·5	93·5	88
6·0	—	97·5	89

(*c*) *Effect of the Diameter of the Rod.* The pressure exerted by the bullet is confined to a comparatively small area in the centre of the end; whereas the pressure wave travelling without change of type implies a nearly uniform distribution of pressure over the section. The question of the nature of the wave developed under such conditions seemed quite intractable mathematically, but from general considerations it appeared

probable that it would not differ greatly from that of the wave originated by a uniform pressure distribution. In order to test this point comparative tests were made with rods of $\frac{3}{4}$ inch, 1 inch, and $1\frac{1}{2}$ inches diameter. The lengths of the rods were roughly 48 inches, 43 inches, and 30 inches, respectively. The results are exhibited in the table on p. 451, in which the figures for the 1 inch rod are the same as those already given.

It will be seen that the diameter of the rod has no appreciable effect up to a length of 4 inches, but that for greater lengths the large rod gives appreciably lower values. In other words the apparent maximum pressure is not much affected by the diameter, and is presumably correctly given by all three rods, while the duration of the blow is largely overestimated by the $1\frac{1}{2}$ inch rod, and presumably somewhat overestimated by the other two, though as they are in substantial agreement on this point the error cannot be very large. It may be surmised that some at any rate of the difference between the observed and calculated times of impact is due to this cause, though, as already pointed out, the rigidity of the bullet is competent to account for part of it.

Experiments at Lower Velocities.

Measurements were also made with cartridges giving velocities of about 1240 feet per second and 700 feet per second respectively, the same types of bullet being used. The results for the 1240 feet per second cartridges are exhibited in the following table, which corresponds to that already given on p. 446 for the 2000 feet per second cartridges:

Rod, 1 inch diameter, about 40 inches long. Velocity of bullets 1240 feet per second. (Mean of 5 shots: maximum 1257, minimum 1229.)

Length of piece	Number of shots	Percentage of total in piece			Total momentum in rod and piece		
		Maximum	Minimum	Mean	Maximum	Minimum	Mean
inches							
0·5	1	—	—	6·5	—	—	37·9
1	8	12·9	12·3	12·7	38·5	36·8	37·7
2	7	26·7	25·8	26·5	35·9	31·5	34·0
3	4	38·4	37·5	38·1	39·4	37·7	38·4
4	5	51·6	50·6	51·1	39·1	38·3	38·6
5	3	63·0	61·5	62·1	40·4	39·1	39·7
6	4	67·7	67·5	67·6	37·2	35·9	36·6
9	5	89	81·5	85·8	39·0	35·8	37·0

The mean total momentum registered (37 shots) is 37·7 units; the calculated total is $1240 \times 0{\cdot}0306 = 38$ units.

The percentage figures are plotted in Fig. 6 (curve marked "1240 feet per second"). The percentage of momentum trapped by short pieces is

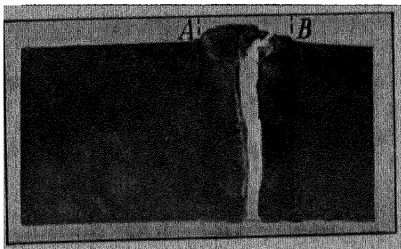


Fig. 8.

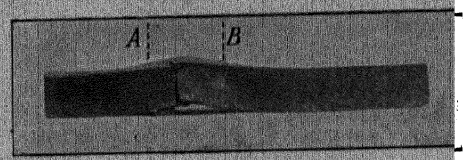


Fig. 9.

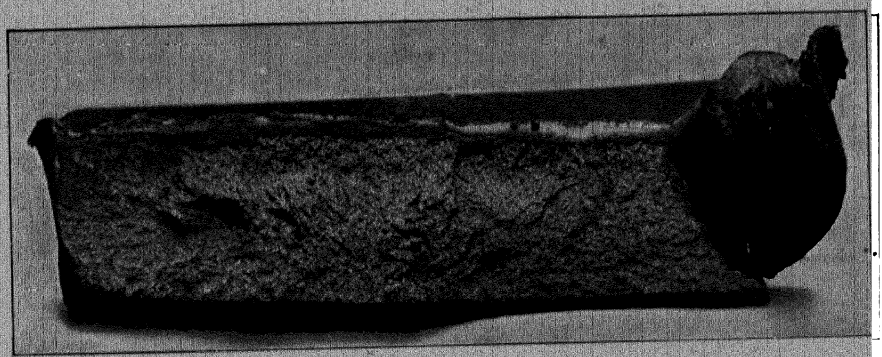


Fig. 10.

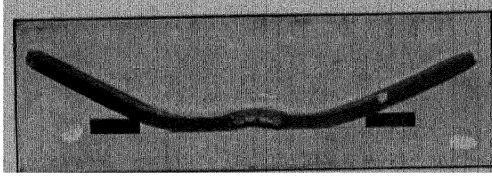


Fig. 11.

13 per inch, and the corresponding maximum pressure for the normal velocity of 1240 feet per second is

$$\frac{0{\cdot}13 \times 38}{32{\cdot}2 \times 0{\cdot}98 \times 10^{-5}} = 15{,}700 \text{ lbs.}$$

The maximum pressure which should be exerted by a perfectly fluid bullet having the same mass and velocity is

$$\frac{0{\cdot}35 \times (1240)^2}{32{\cdot}2} = 16{,}700 \text{ lbs.}$$

The time taken by the bullet to travel its own length is $8{\cdot}4 \times 10^{-5}$ seconds. Thus if the bullet were perfectly fluid, the whole momentum should be trapped in a piece 9 inches long, whereas in fact only 86 per cent. is so trapped. The errors inherent in the method of experiment, which have been discussed in the last section, will all be less at the lower velocity. On the other hand the rigidity of the bullet will be relatively more important and probably suffices to account for much of the difference between the theoretical and observed times of impact.

The 700 feet per second bullets showed a maximum pressure of 5450 lbs., as compared with 5320 lbs. calculated. 54½ per cent. of the momentum was trapped by a 9 inch piece. It was not possible to experiment with longer pieces, so that the time of impact in this case could not be determined.

It should be observed here that just after the piece has been shot off it tends to pull the rod after it by magnetic attraction, which of course still continues after the joint is broken, though it diminishes rapidly as the distance between piece and rod widens. The effect of this is to give more momentum to the rod and less to the piece than they would respectively possess as the effect of the blow alone. By measuring the amount of the magnetic pull when the piece is held at different distances from the rod, the current in the solenoid being the same as that used in the impact experiment, it is possible to estimate the amount of this effect. With 2000 feet per second bullets it is quite negligible, but when the velocities are lower particularly with long pieces, it necessitates a correction. This correction has been applied in the figures given above for the 1240 feet per second and 700 feet per second bullets.

Detonation of Gun-Cotton.

It is well known that a charge of 1 lb. gun-cotton will shatter a mild steel plate 1 inch thick or more, if it be detonated in firm contact with it. The fracture is quite "short," like that of cast iron, though the broken pieces are usually more or less deformed. Typical fractures of this kind obtained on plates of very good mild steel are illustrated in Plate I, Figs. 8, 9, 10, and 11. Figs. 8 and 9 are photographs of a plate 1¼ inches thick originally quite flat. It was broken by a slab of gun-cotton weighing 1 lb. which

covered the section of the plate AB and was detonated in contact with that which became the convex face (lower face in Fig. 9). Fig. 10 is a view of the broken edge of one of the two fragments. The plate shown in Fig. 11 was a flat piece of boiler plate $1\frac{1}{4}$ inches thick. A slab of 1 lb. of gun-cotton was detonated against that which is the under side in the figure and the two pieces subsequently fitted together again and photographed. Thinner

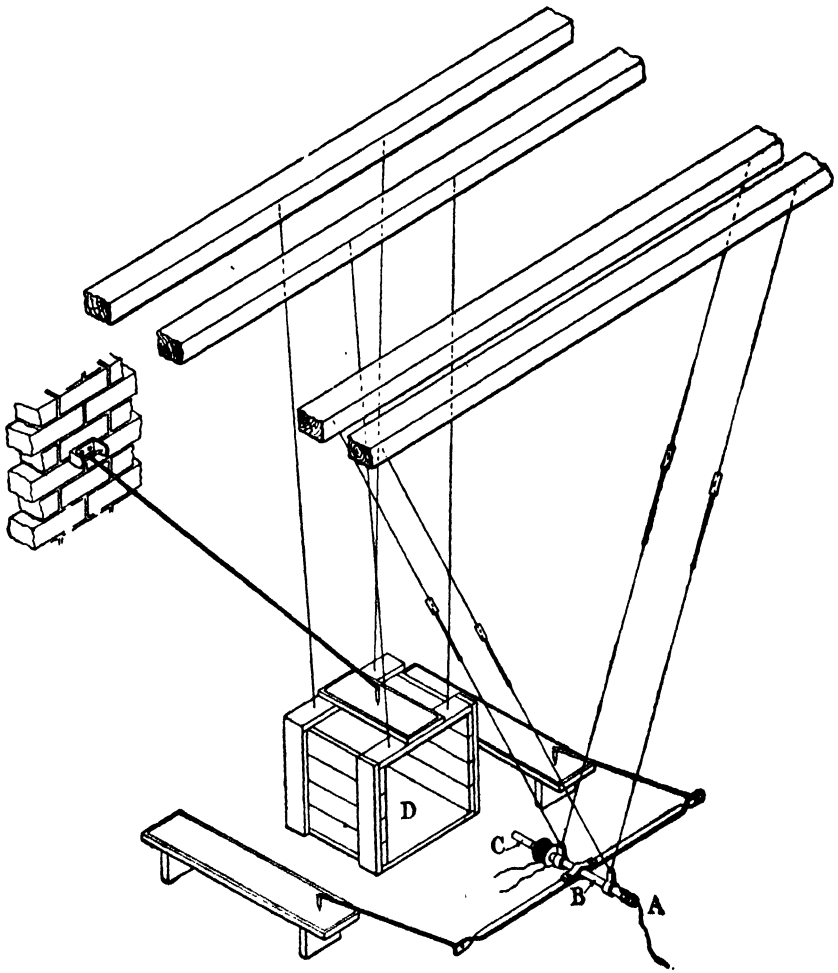


Fig. 12.

plates—*e.g.*, 1 inch thick—are usually cracked in two places, one at each edge of the gun-cotton slab, and the portion covered by the slab is blown out of the plate, sometimes whole and sometimes shattered into pieces. The fact that no tamping is necessary suggests that the duration of the process of detonation is of the same order as the time taken by sound to travel an inch or less in air, so that during the conversion of the cotton

into gas there is not time for much expansion*. If this be so, the maximum pressure developed must be that which would be reached if the cotton were fired in a closed chamber of a volume not greatly exceeding that of the slab. The pressure is then dissipated with great rapidity by the expansion of the gas, which is resisted only by its own inertia and that of the surrounding air.

Experiments on the detonation of gun-cotton have been made by the method described in this paper. It has only been possible hitherto to use quite small charges and the results are a very rough approximation, but as they throw light on a matter of which little is known I have thought it worth while to give them. Briefly, the conclusion is that the pressure at a point distant $\frac{3}{4}$ of an inch from the surface of one ounce of dry gun-cotton (a cylindrical "dry primer" about $1\frac{1}{4}$ inches diameter and $1\frac{1}{4}$ inches long), when detonated with fulminate, has fallen to less than $\frac{1}{5}$ of the maximum value within 2×10^{-5} seconds. At least, 80 per cent. of the blow has been delivered within that time. Over an interval of 10^{-5} seconds round about the time of maximum pressure the average pressure is about 30 tons per square inch, and the actual maximum is probably of the order of 40 tons per square inch. At a point on the surface the maximum pressure is at least twice as great, 80 tons per square inch†.

The arrangements are shown in Fig. 12.

The gun-cotton cylinder A is fixed by short splints of wood opposite the end of the shaft B, which is of mild steel $1\frac{1}{4}$ inches diameter and from 15 to 30 inches long. This shaft is suspended as a ballistic pendulum with a pencil and paper for recording its movement. The end-piece C, from $\frac{1}{2}$ to 6 inches long, is held on by magnetic attraction. The faces of the joint are a scraped fit. In line with the shaft is the box D, which is also suspended as a pendulum and provided with a recording pencil. Some part of the momentum given to the box is due to the blast from the gun-cotton; this was estimated from experiments in which there was no piece on the end of the shaft. Separate experiments were also made to determine the effect of the blast on the supports of the shaft. The momentum accounted for by the blast is in each case deducted from the total recorded momentum to get the nett momentum due to the blow on the end of the shaft. This correction in the case of the box amounted to about 8·3 units with a 15 inch shaft, and 1·2 units with a 30 inch shaft. The correction for the blast on the supports of the shaft was 5 units.

* The velocity of detonation of long trains of gun-cotton has often been measured and is variously estimated at 18,000 to 20,000 feet per second. If the same velocity obtained in the small primers they would be completely converted into gas in about 2×10^{-6} secs.

† The pressure developed by the explosion of gun-cotton in a vessel which it completely fills does not appear to have been measured. From measurements made with charges of lower density Sir Andrew Noble estimates that it would be about 120 tons per square inch (*Artillery and Explosives*, p. 345). Allowing for the partial expansion during the process of detonation, this agrees fairly well with the pressure here determined.

The following table gives the results of all the trials made with the gun-cotton about $\frac{3}{4}$ inch from the end of the shaft.

Length of piece	Total nett momentum shaft and piece	Percentage of total in piece	Average percentage in piece
inches			
	40·7	86·6	
	46·2	93	
	38·8	93	
3·85	50·0	92	90
	41·4	90	
	*31·2	90	
	*45·6	88	
	60·0	89	
3	57·3	88	89
	50·9	89	
	74·3	90	
	36·7	81	
2	38·7	87	83
	42·9	84	
	42·0	79	
	40·8	57	
	42·3	57	
	44·1	55	
0·95	55·4	60	57
	*44·6	57	
	*50·8	58	

* In these cases the air space between the gun-cotton and the end of the shaft was 1 inch. In all the others it was $\frac{3}{4}$ inch.

The total impulse of the blow when the air-space is $\frac{3}{4}$ inch varies from about 35 to 70 units, the average being about 46 units. The percentages absorbed by the different end-pieces are, however, more nearly constant, and from them a rough approximation to the pressure wave transmitted by the rod in an average case may be constructed. As already explained the precise form of this curve depends on the way in which the pressure rises, but it may be assumed in this case that the pressure reaches its maximum in a time that is short even in comparison with the duration of the blow. Assuming an average total momentum of 45 units, Fig. 13 has been constructed. The area of the parallelogram marked 1 represents the momentum given to a 1 inch piece, the width of this parallelogram is 10^{-5} seconds and the height is the average pressure acting during the first 10^{-5} seconds. The parallelogram marked 2 represents the excess of the momentum given to the 2 inch piece over that given to the 1 inch piece and its height is the average pressure acting during the second 10^{-5} seconds. The dotted curve gives the same average pressures over the successive intervals of time. It is obviously largely conjectural, but it gives a rough idea both of the maximum pressure and of the duration of the blow.

The chief difficulty experienced hitherto in measuring by this method the pressures developed in the detonation of gun-cotton has been the permanent deformation of the end of the rod by the blow. No steel has yet been discovered which will stand, without flowing or cracking, the detonation of gun-cotton in contact with it, and even when a cushion of air

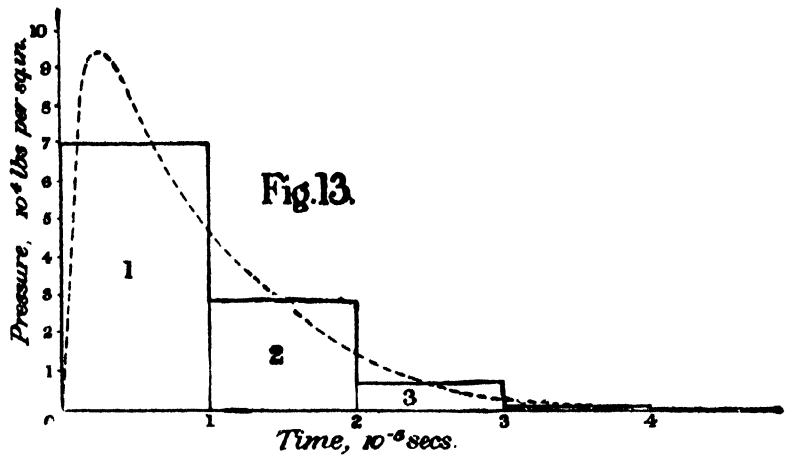


Fig. 13.

Pressure at a distance of ¾ inch from surface of one ounce "dry primer."

¾ inch thick is interposed some flow takes place*. In consequence of this, the pressure wave which emerges and is propagated elastically cannot be quite the same as the wave of applied pressure. It is easy to see that the general effect of the setting up of the end must be to deaden the blow, that is to reduce the maximum pressure and prolong its duration. In Fig. 14,

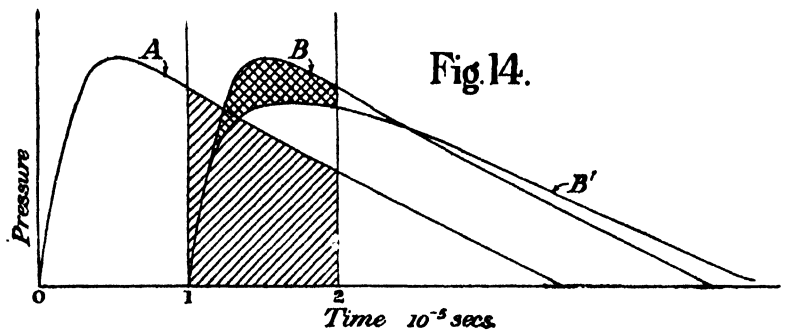


Fig. 14.

A is the (conjectural) curve representing the pressure applied to the end of the rod. If the rod were perfectly elastic, the pressure across a section 2 inches from the end would be represented on the same time base by the curve B, which is the same as A, but moved 10^{-5} seconds to the right. The momentum in the end 2 inches at any time is the difference between the

* This is when the steel is in the form of a shaft, so that there is no lateral support of the part subjected to pressure. It is, of course, possible to make a plate with hardened face which will withstand the attack of gun-cotton on a portion of the face.

areas of the curves up to that time. For instance at 2×10^{-5} seconds it is represented by the shaded area under curve A. But if the end be not completely elastic, the higher pressures developed over section B will be less than those acting on the end at corresponding times. Thus the record of pressure over the section 2 inches from the end will be a curve such as B' and the momentum in the end 2 inches at any time will be greater than it would be if the end were elastic by the difference between the areas of curves B and B' which is double shaded in the figure. This extra momentum is transferred to the remainder of the rod later on, causing the curve B' to rise above B. The curve B' represents the wave of pressure actually sent along the rod. It is this curve which is determined by the method which has been described, and it is evident that that method underestimates the maximum pressure and over-estimates the duration of the blow.

A few experiments were made with the gun-cotton touching the end of the shaft. The average total momentum given to the shaft and piece in this case is about 90 units or roughly twice as great as that transmitted through $\frac{3}{4}$ inch air-space. Of this total about 80 per cent. is caught in a piece 4 inches long, and about 50 per cent. in a piece 1 inch long. When the gun-cotton is at a distance of $\frac{3}{4}$ inch these figures are 90 and 60 respectively. The apparent duration of the pressure is therefore rather greater at the surface of the explosive. The setting up of the end of the shaft is, however, much more marked when the gun-cotton is in contact and it may be that the distribution of the pressure in time is not materially different in the two cases. If that were so, the maximum pressure developed on the surface of the gun-cotton would be 80 or 100 tons per square inch.

It is hoped that by the use of special steels it may be possible to give greater precision to these estimates of the amount and duration of the pressure produced by the detonation of gun-cotton in the open. Meanwhile the information already obtained as to the order of magnitude of these quantities is sufficient to throw some light on the nature of the fractures produced. The general result obtained may be expressed by saying that a gun-cotton cylinder $1\frac{1}{4}$ inches $\times$ $1\frac{1}{4}$ inches produces at its surface, when detonated, pressure of the average value of 100,000 lbs. per square inch lasting for 1/50,000 second. Probably figures of the same sort of magnitude will describe the blow produced by the detonation of a slab $1\frac{1}{4}$ inches thick, one of whose faces is in contact with a steel plate. It may be that the pressure is greater and the duration correspondingly less, but this does not affect the point that the pressure is an impulsive one in its effect on the plate. That is, the effect of the pressure is to give velocity to the parts of the plate with which the gun-cotton is in contact but the pressure disappears before there has been time for much movement to take place. For instance, if the plate be 1 inch thick (mass 0·28 lb. per square inch) a pressure of 100,000 lbs. per square inch acting on it for 1/50,000 second will

give a velocity of about 230 feet per second, and while the pressure is being applied it will move 0·028 inches.

The parts of the plate not covered by the gun-cotton are left behind and the strain set up by the forced relative displacement is the cause of the shattering of the plate. The magnitude of this strain, and of the consequent stress, depends (speaking generally), on the relation between the velocity impressed on the steel by the explosion and the velocity of propagation of waves of stress into the material. For instance, if the section AB (Fig. 15) be given instantaneously a velocity of 200 feet per second and this velocity be maintained, the state of the plate after the lapse of 1/100,000 second will be that represented diagrammatically by Fig. 15. The section AB has moved forward relatively to the remainder by 0·002 foot. As soon as this section started moving a wave of shear stress started out from A into the parts of the plate to the left which had been left at rest by the blow. This wave travels in steel at 11,000 feet per second and will therefore in 1/100,000 second get to C where $AC = 0{\cdot}11$ foot. To the left of C the metal has not moved, the wave not having reached it; therefore the average shear in the section AC is $\frac{0{\cdot}002}{0{\cdot}11} = 0{\cdot}018$. Under forces of this duration even mild steel has nearly perfect elasticity up to very high stresses*. If it maintained its elasticity and continuity the shearing stress would be of the order $0{\cdot}018 \times 1{\cdot}2 \times 10^7$, or say 220,000 lbs. or 100 tons per square inch. This illustration is of course very far from representing the actual effect of suddenly giving velocity to a portion of a plate, the real distribution of stress would be far more complicated, but it gives an idea of the magnitude of the stresses which may be expected to arise. In static tests on mild steel, the material begins to flow as soon as the shearing stress exceeds about 10 tons per square inch and no stress materially greater than this can exist. But when the metal is forcibly deformed at a sufficiently high speed the shearing stress is increased by something analogous to viscosity and the tensile stress which accompanies it may be sufficient to break down the forces of cohesion and tear the molecules apart. Thus the steel is cracked, though in ordinary static tests it can stretch 20–30 per cent. without rupture, just as pitch, which can flow indefinitely if given time, is cracked by the blow of a hammer. The essence of the matter is the forcible straining of the substance at a velocity so high that it behaves as an elastic solid rather than as a fluid, thus experiencing stresses which are measured by the strain multiplied by the modulus of elasticity. The effect of gun-

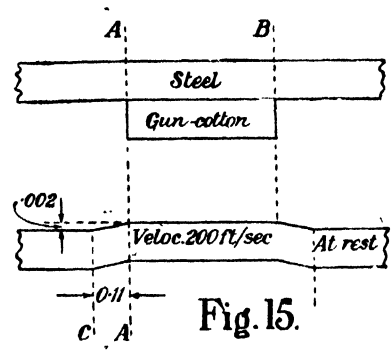


Fig. 15.

* Hopkinson, *Proc. Roy. Soc.*, vol. 74, p. 498.

cotton on mild steel shows that in this material a rate of shear of the order 1000 radians per second is sufficient to cause cracking.

The most probable account of the smashing of a mild steel plate by gun-cotton is, then, that the plate is cracked before it has appreciably deformed, the cracks being caused by relative velocity given impulsively to different parts of the plate. Bending of the broken pieces occurs after the plate has cracked and the pieces have separated from one another. It is due to relative velocity in different portions of each piece which still persists after the initial fracture, and is taken up as a permanent set in each piece. In this connection the fracture shown in Fig. 11, Plate I, is instructive. It will be noticed that the general bend of the plate, after the pieces have been fitted together, is *opposite* to that which might at first sight be expected as the result of the blow in the middle. Inspection of such fractures leads to the conclusion just stated as to their history. The experiments on gun-cotton pressures described in this paper, though lacking in precision, supply I think the missing link in an explanation which is otherwise probable, namely, sufficient evidence that the blow may be regarded as an impulsive force communicating velocity instantaneously.

Most of the experimental work described in this paper was done by my assistant, Mr H. Quinney. I also received valuable help in the earlier stages from Mr A. D. Browne, of Queens' College, and from my brother Mr R. C. Hopkinson, Trinity College. To these gentlemen I wish to express my obligation for aid without which it would hardly have been possible to carry out a research of this character. I have also to thank Sir Robert Hadfield, Mr W. H. Ellis, and Major Strange for providing steel plates and shafts.

Note added November 1920. In this connection the editors have received permission to publish the following letter:—

Research Department, Royal Arsenal, Woolwich.
April, 1918.

Maj. B. Hopkinson, C.M.G., F.R.S.

Sir,

It may be of interest to you to know that the application of the principles of the pressure produced by blows, first enunciated by you, has led to much fruitful result at the Research Department.

Applications have been made to the testing of detonators, gaines, fuzes, general explosives and shell, against bars varying from 0·7″ to 4″ in diameter. Some of these have become specification tests and all have helped greatly in elucidating principles and assisting design. As an example of some of the work a copy of a recent report "Experimental Study of Gaines" is forwarded to you.

The Superintendent of Research also feels indebted for the transfer of your assistant Mr H. Quinney, who has worked indefatigably on these and other questions. If you should care to see the arrangement at Woolwich, a cordial invitation is extended to you to visit the Research Department.

Yours sincerely,

(Sgd.) A. C. Currie, Brig.-General,
Controller, Munitions Design.

28

THE EFFECTS OF THE DETONATION OF GUN-COTTON.

["Proc. North-East Coast Institution of Engineers and Shipbuilders," Vol. XXX, 1913–1914.]

Nearly all explosives now used in practice are solid or liquid bodies whose molecules are in an unstable condition, that is they have a tendency when disturbed to break up or decompose, changing from the condensed solid into the gaseous form. The molecule of gun-cotton may be likened to a steel envelope full of water and heated until the envelope is near bursting. A slight additional heat or a mechanical shock sufficient to rupture the envelope results in the rapid conversion of the water into steam. A portion of the energy which was locked up as heat in the water is thus liberated in the form of the pressure or the rapid motion of the steam and it is this which produces the destructive effects of a boiler explosion. So the gun-cotton molecule has locked up within it by some sort of chemical bond a large amount of energy and when the bond, which is not of the firmest, is by suitable means broken down this energy appears as the pressure or motion of the gas into which the explosive is then resolved. There is, however, the difference that whereas the water is cooled when converted into steam, the gas resulting from the gun-cotton is highly heated and this increases its pressure. The gases into which gun-cotton is converted on explosion would occupy, if allowed to expand to atmospheric pressure and cooled down, nearly 1000 times the volume of the solid cotton. If the gases be prevented from expanding, by confining the gun-cotton in an enclosure of sufficient strength, they will exert a pressure in proportion to the reduced volume which they are forced to occupy, and this pressure is still further increased by the high temperature to which they are raised. The smaller the volume of the enclosure the greater the pressure, which reaches a maximum when the gun-cotton just fits the enclosure.

The pressure developed by explosives when confined depends obviously on the volume of gas developed in relation to the volume of the enclosure, and on the temperature of that gas. We owe to Sir Andrew Noble more than to any other man, our knowledge of these factors and of the resulting pressures. For determining the pressures he has used for the most part the well-known crusher gauge which he perfected many years ago. This gauge consists of a cylinder of copper which is compressed by a steel piston acted on by the pressure, just as an indicator piston compresses the spring. The amount by which the cylinder is crushed measures the pressure. Sir

Andrew Noble found in this way that gunpowder confined in a bomb which it just filled gave a pressure of 43 tons per square inch. He did not find it possible to measure the pressures developed by the more powerful explosives such as gun-cotton or cordite when similarly confined; because no measuring apparatus was available which could withstand these pressures without damage. But by using enclosures of greater volume than the solid explosive so that the resulting gases are less compressed, the pressure of cordite and of gun-cotton has been brought within the range of measurement. Thus it was found by Sir Andrew Noble, working with Sir F. Abel, that gun-cotton in an enclosure of about twice its own volume gives a pressure of 50 tons per square inch. They inferred that the gases from this explosive if they could be held within a bomb of the same volume as the solid would give a pressure of about 120 tons per square inch.

The molecular agitation called heat is in most cases sufficient to upset the delicate balance of the solid explosive molecule and to convert it into the gaseous form. If the reaction be initiated by heating one part of a mass of explosive, the hot gases generated by the decomposition of this part warm up the neighbouring still solid molecules and decompose them, and thus the reaction is propagated from point to point until the whole mass is changed into gas. If the explosive be unconfined the gas escapes harmlessly into the air as fast as it is generated, and the explosive merely burns more or less rapidly with no effect other than the production of a good deal of flame, heat, and noxious gas. This is what happens when a train of gunpowder or of loose gun-cotton is fired in the open air. The burning of cordite in a gun is essentially the same, only here the gas is not allowed to escape but is made to exert pressure on the shot, and this pressure accelerates the rate of burning.

The progress of inflammation in loose gun-cotton depends on the conduction of heat from point to point, and the rate at which it goes on is limited by the rate at which heat can be conducted. In many explosive compounds, however, it is possible under certain conditions to cause the molecules to break up by mechanical shock such as a sufficient rise of pressure, and without the communication of heat from outside. The way in which mechanical shock upsets the stability of the molecule is unknown; it is possible that it may be partly a temperature effect due to the heat developed in the material by the sudden application of mechanical stress just as air is heated by rapid compression. If now a small portion of the explosive within a closely compacted mass is ignited in any manner, the gas generated is unable to escape immediately but is confined more or less by the surrounding explosive on which for an instant it presses with a pressure of the same order as it would exert on a steel bomb. In some explosives, for instance in cordite, the explosive would survive the pressure which would be dissipated with great rapidity as the gases escape from their confinement. In such cases the explosive may simply be scattered

unburnt, or if burning continues it will go on by the ordinary slow process of heat conduction. But if the explosive be sensitive to shock the pressure due to the first ignited portion, fleeting though it is, may suffice to fire the adjacent layer on which it acts. This in its turn fires by pressure the next layer, and so the explosion is propagated from point to point not by conduction of heat, but by the far more rapid process of transmission of mechanical pressure. The slow flow of heat in substances like gun-cotton is a matter of common experience; if a slab 1 inch thick be placed on a hot plate some seconds will elapse before the heat will be sensible on the other face. But pressure applied to one side is felt almost instantaneously on the other side; it travels through with the velocity of sound which may be several thousand feet per second.

"Detonation" is the name given to the propagation of an explosion by a mechanical, as distinct from a purely thermal, process. It occurs only in explosive compounds; and never in mixtures such as gunpowder. Not all such compounds, however, will detonate. Evidently, for detonation to take place the explosive must go off under the application of a pressure which does not exceed that generated when the explosive is burnt in a confined space. In cordite, which cannot be detonated, this condition apparently is not fulfilled. Where an explosive has the necessary sensitiveness to pressure for detonation to occur, it is in general also necessary that it should be closely packed so that the pressure may be transmitted without loss from point to point. Gun-cotton in the dry state may be either burnt or detonated. To initiate detonation requires as a rule the production of a very high local pressure, probably of the order of 100 tons per square inch. Local heating does not usually cause gun-cotton to detonate, nor does an ordinary mechanical blow, even that of a bullet; in both cases the explosive merely burns. To produce sufficient pressure with certainty it is necessary to use another explosive of a more sensitive kind which detonates either when struck an ordinary blow, or by the application of heat. Fulminate of Mercury is the detonator generally employed in practice. A few grains are placed in a copper tube, which is inserted in a hole in the gun-cotton. The fulminate is heated by an electric current, or by the flame from a gunpowder fuse, it detonates and hits the gun-cotton in its neighbourhood a violent blow thus initiating the detonation of the gun-cotton. The fact that the copper tube must be in close contact with the cotton surrounding it, shows the purely mechanical origin of detonation.

When an explosive is fired in the open and the inflammation is of the ordinary kind transmitted from point to point by conduction of heat, the gases can escape as fast as they are formed, and there is no appreciable rise of pressure. But since the gases and surrounding air possess inertia and must acquire velocity in order that they may get away, it is obvious the sufficiently rapid inflammation would result in a rise of pressure. Indeed if the whole mass of solid explosive could be converted into gas

absolutely instantaneously, the gas generated would at the instant of its formation fill the space previously occupied by the solid and the pressure would for that moment be the same as if the explosive were confined in a steel bomb which it just fitted. The pressure would, of course, disappear with great rapidity by the expansion of the gas, but the maximum reached would be the same as in the bomb.

The extreme hypothetical case of instantaneous gasification is approached pretty closely, though, of course, it cannot be actually reached, when the explosive is detonated. The velocity with which detonation is propagated along a train of gun-cotton was measured many years ago by Abel. It is about 18,000 feet per second, or more than 200 miles per minute. When the wave travels radially in all directions from the centre of a compact mass the velocity will not necessarily be the same, but is

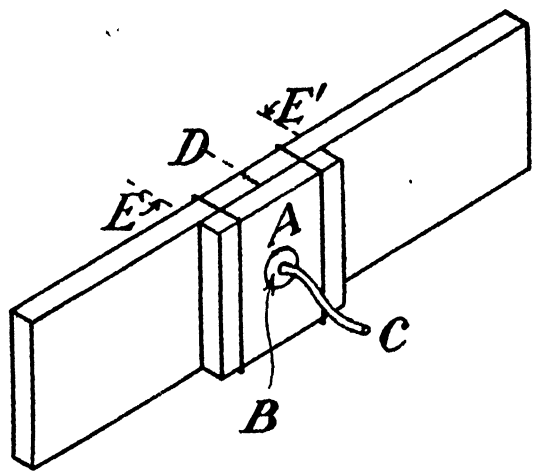


Fig. 1.

probably of the same order of magnitude. If it were the same, a 1 ounce gun-cotton primer such as I have experimented with would be completely converted into gas in 2 or 3 millionths of a second.

Before proceeding to describe the experimental methods which I have developed for measuring the pressure produced by the detonation of explosives, it will be well to say something of the practical effects which that pressure can bring about. Fig. 1 is a sketch of a plate of mild steel about 1 inch thick. Slab *A* of wet gun-cotton, of about the same thickness, is in firm contact with it on one side. A primer *B* of 1 ounce of dry gun-cotton fits in a hole, and within the primer is a fulminate detonator which can be fired by the fuse *C*. The primer is detonated by the fuse, and in its turn detonates the mass of wet gun-cotton. Thus in perhaps the one hundred thousandth part of a second the whole slab is converted into gas. The effect is to smash the plate. If the gun-cotton slab is narrow, the plate will fail by a crack in the middle (*D*), if it covers a greater width

there will be a crack at each edge of the slab (E, E'). In the latter case a piece is blown out of the middle of the plate with a velocity of 200 or 300 feet per second. Sometimes this piece may be recovered whole, but more often it is broken into smaller fragments. Figs. 2 and 3 are drawn from photographs of the fracture of a plate of very good mild steel $1\frac{1}{4}$ inches thick. Fig. 4 is a piece of boiler plate of about the same thickness. The gun-cotton was in each case placed on the underside of the plate in the figure, and was detonated in the open without tamping of any kind. The plate was hung or stood up on edge, as shown in Fig. 1, without other support. It is remarkable that the fractures are quite short without sensible reduction

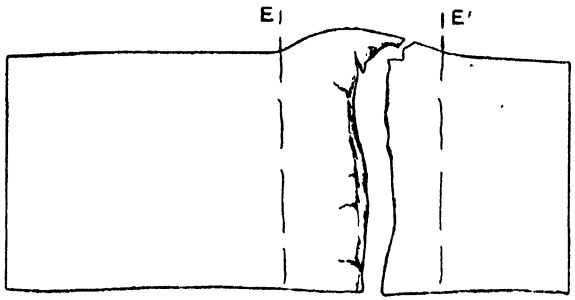


Fig. 2.

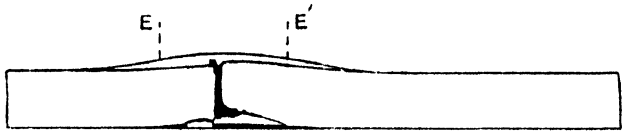


Fig. 3.

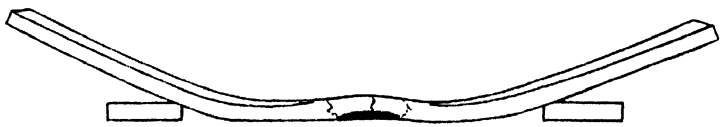


Fig. 4.

of area, so that under the action of this particular type of stress a highly ductile mild steel behaves like a brittle body. There is usually some distortion of the broken pieces of the plate, but there is reason to suppose that this occurs after the fracture, and that the immediate effect of the blow is to shatter as though it were cast iron, a material which in a press can be bent double without showing a crack.

Explosives which detonate readily are of use only as destructive agents. Gun-cotton, apart from its use in the manufacture of other explosives, is chiefly interesting to military engineers, who use it for demolishing rapidly the bridges, rail-roads, etc., which their civil brethren have built. But the fact that it can cause mild steel to break in the curious fashion

which has just been described makes its action well worthy of further study by all engineers, because of the light which such study may throw on the properties of the material with which they work. It was from this point of view that I approached the subject, and tried to devise a means of analysing the blow given by the detonation of gun-cotton.

The effect of any blow is usually defined numerically as the product of the average pressure into the time for which it acts, called the impulse. This product is quite easily measured by causing the pressure to act on a movable body of some kind, and measuring the momentum generated,

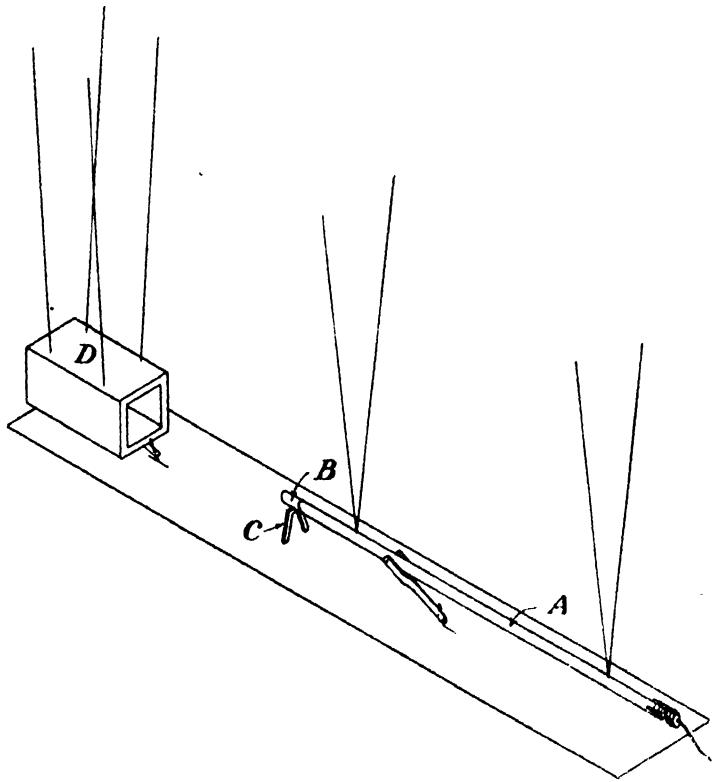


Fig. 5.

which is equal to the impulse of the blow. Thus, a 1 ounce gun-cotton primer, which is a cylinder $1\frac{1}{4}'' \times 1\frac{1}{4}''$, may be fixed by wooden splints to the end of a steel shaft of the same diameter (Fig. 5). The shaft is hung up by four threads so that it can swing parallel to itself, and is provided with a pencil, which marks on a sheet of paper the amplitude of the swing. When the gun-cotton is detonated, the shaft is given velocity impulsively as though it were struck a violent blow with a hammer, and the distance through which it subsequently swings is proportional to the velocity given by the blow. This is the ordinary principle of the ballistic pendulum. If the shaft be 6 feet long (weighing about 25 lbs.), the velocity given to it

by the gun-cotton primer will be about 4 feet per second, and if suspended by strings 50 inches long, it will swing through 17 inches. The same shaft, if struck by a service rifle bullet moving 2000 feet per second, would swing through about two-thirds of the distance.

The problem is to separate the impulse so determined into its two factors of pressure and time. The direct determination of the pressure factor may perhaps be described as impossible, because the pressure amounts to many tons, and lasts only for 1/50,000 second. We must, therefore, find the duration of the blow; then dividing the duration into the impulse, we get the pressure.

The pressure applied at the end of the shaft rises with great rapidity to a maximum as the detonation travels through the gun-cotton, and then falls as the gases escape outwards. The fall of pressure is also very rapid,

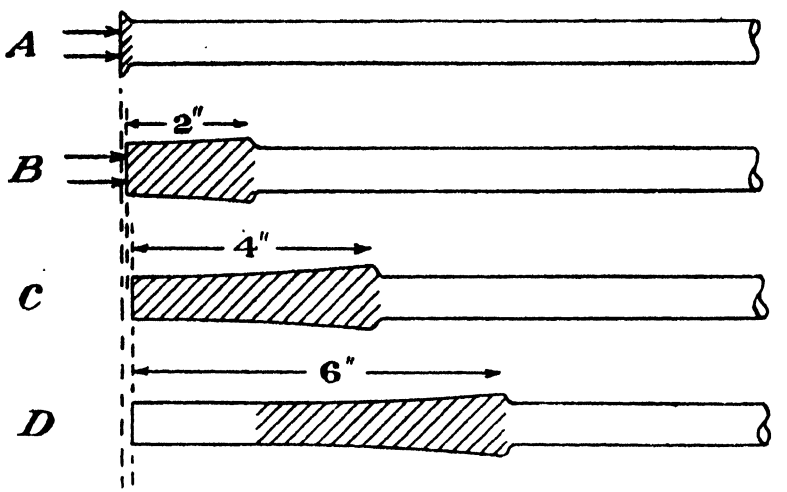


Fig. 6.

but not so rapid as the rise. The pressure may be plotted as a function of the time, and the curve so obtained will be a perfectly definite thing, though the ordinates are expressed in tons and the abscissae in millionths of a second. The result of applying this varying pressure to the end is to send along the rod a wave of pressure which travels without change of type. The progress of such a wave is illustrated diagrammatically in Fig. 6, the state of the shaft being shown at intervals of 1/100,000 second. The compression of the shaft and the lateral expansion are, of course, much exaggerated. In *D* (Fig. 6) the pressure has ceased to act, and its effects are represented by the shaded wave, now advanced some distance into the rod, the parts to right and left of which are unstrained and at rest. If the pressure in different sections of the rod be plotted at any instant (Fig. 7), then at a later time the same curve shifted to the right by a distance proportional to the time, will represent the then distribution of pressure.

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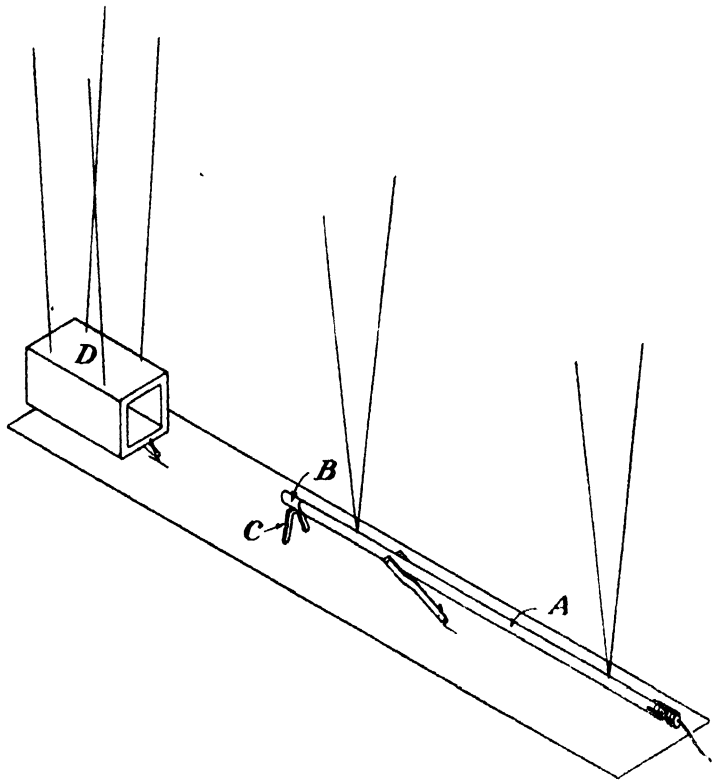


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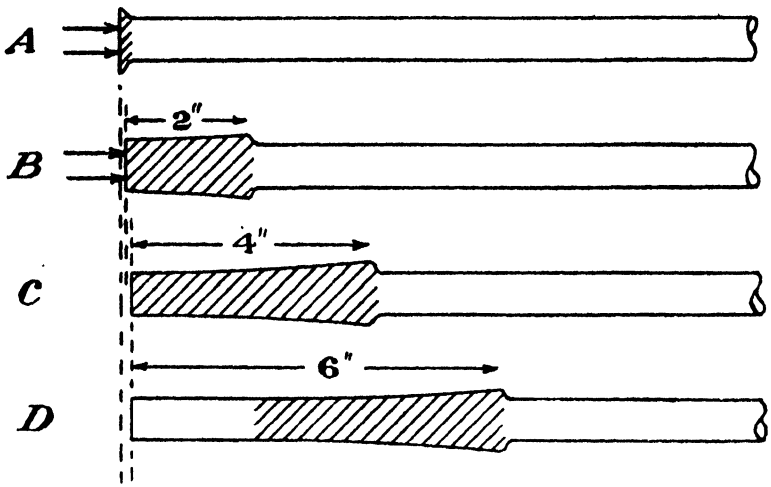


Fig. 6.

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The velocity with which the wave travels in steel is approximately 17,000 feet per second. As the wave travels over any section of the rod, that section successively experiences pressures represented by the successive ordinates of the curve as they pass over it. Thus the curve also represents the relation between the pressure at any point of the rod and the time, the scale being such that one inch represents the time taken by the wave to travel that distance, which is very nearly 1/200,000 second. In particular, the curve giving the distribution of pressure in the rod along its length is, assuming perfect elasticity, the same as the curve connecting the pressure applied at the end and the time, the scale of time being that just given.

The progress of the wave of stress along the rod is accompanied by a corresponding strain, and therefore by movement. It is easy to show that the same curve which represents the distribution of pressure at any moment also represents the distribution of velocity in the rod, the scale being such that one ton per square inch of pressure corresponds to about 1·3 feet per second of velocity. Until the wave reaches any section of

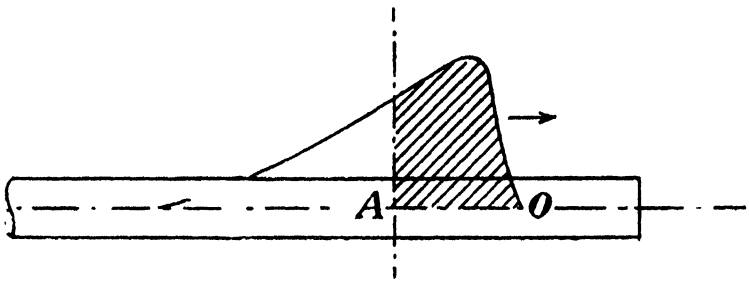


Fig. 7.

the rod, that section is at rest. It is then, as the wave passes over it, accelerated more or less rapidly to a maximum velocity, then retarded, and finally left at rest with some forward displacement. In this manner, the momentum given to the rod by the application of pressure at its end is transferred along it by wave action, the whole of such momentum being at any instant concentrated in a length of the rod which corresponds, on the scale above stated (1 inch = 1/200,000 second), to the total duration of the blow. Consider a portion of the rod to the right of any section A (Fig. 7) which lies within the wave at the moment under consideration. The pressure has been acting on this portion since the wave first reached it, that is for a time represented by the length OA and equal to $\frac{OA}{V}$ where V is the velocity of propagation. The momentum, which has been communicated to the part under consideration, is equal to the time integral of the pressure which has acted across the section A, that is to the shaded area of the curve in the figure. The portion of the rod to the right of the section is continually gaining momentum at the expense of the portion to the left

while the wave is passing, the rate of transfer at any instant being equal to the pressure.

When the wave reaches the free end of the rod, it is reflected as a wave of tension, which comes back with the same velocity as the pressure wave, and the state of stress in the rod subsequently is to be determined by adding the effects of the direct and the reflected waves. Now, suppose that the rod is divided at some section B near the free end (Fig. 8), the opposed surfaces of the cut being in firm contact and carefully faced. The wave of pressure travels over the joint practically unchanged, and pressure continues to act between the faces until the reflected tension wave arrives at the joint. The pressure is then reduced by the amount of the tension due to the reflected wave, and as soon as this overbalances at section B the pressure of the direct wave, the rod being unable to withstand tension at the joint, parts there and the end flies off. With a wave of the shape shown having a very steep front, this happens almost immediately on the arrival of the reflected wave at the joint. The end-piece has then acquired

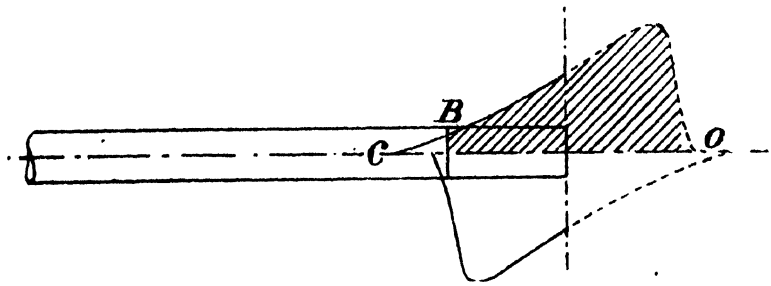


Fig. 8.

the quantity of momentum represented by the shaded area in the figure, equal approximately to the time-integral of the pressure curve from O to B, that is over the period of time required for the wave to travel twice the length of the end-piece. The piece flies off with this amount of momentum, so to speak, trapped within it, and the remainder is left in the shaft. The amount of momentum so trapped can readily be measured by catching the piece in a box suspended so as to form a ballistic pendulum. Dividing this by the time OB, the average pressure which has acted over that interval is obtained, and this is the average pressure exerted by the gun-cotton over the corresponding interval of time. By experimenting with different lengths of piece the area of the pressure-time curve for corresponding intervals can be found, and the curve can thus be mapped. For practical purposes, however, only two lengths of piece are important. First, if the piece be short compared with the length of the wave, the pressure calculated as described is nearly the maximum pressure exerted; as near an approximation as is desired can be obtained by making the piece short enough. This is true whatever the shape of the pressure wave. Second, it is clear that if the tail of the pressure wave has passed the section B

when the reflected wave arrives there, the whole momentum of the blow will have passed out of the shaft into the end-piece, and the shaft is left completely at rest. This will happen if the length of the piece is equal to or greater than half the length of the pressure wave. Thus, the duration of the blow corresponds on the scale of 1 inch = 1/200,000 second to twice that length of piece which just stops the shaft from moving. The general effect of the end-piece and its dependence on the sharpness of the blow may be illustrated from the behaviour of a row of billiard balls touching one another and struck at one end. If the blow be a sharp one, such as is given by another ball, all remain at rest except the ball at the other end, which moves off with the whole momentum of the blow; whereas the dead blow given by a cue causes all to move.

The apparatus as used for measuring gun-cotton pressures is shown in Fig. 5. The shaft *A* is hung up by four equal threads. The end-piece *B*

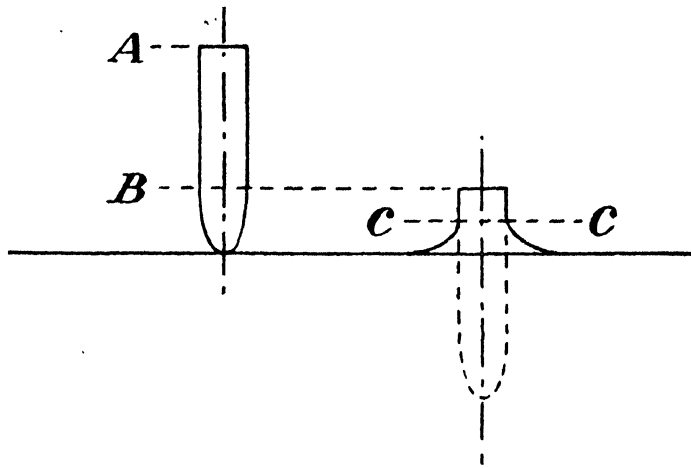


Fig. 9.

rests in contact with the end of the shaft, its weight being taken by the support *C* which falls out of the way when the piece is knocked off. Close contact is ensured by the use of a little thick grease (*e.g.*, vaseline) between the surfaces, or by magnetizing the shaft a little. The piece is caught in a box *D* suspended in a manner similar to the shaft. Both the box and shaft are provided with pencils which record on horizontal sheets of paper the amplitude of the swings. From these records the momentum trapped in the piece and the momentum left in the shaft can be calculated.

In order to test the method it was applied to measuring the pressure produced by the impact of a lead bullet. This can be predicted theoretically, for lead moving with a velocity of 2000 feet per second behaves on impact as though it were perfectly fluid. The bullet is fired at the end of a shaft and then proceeds to deform as shown in Fig. 9, the lead flowing out sideways as though it were a jet of water. The base of the bullet knows nothing of the impact of the nose and continues to move on with unimpaired

velocity as though nothing had happened. Pressure continues until the base has arrived at the shaft, and then it ceases, so that the duration of the blow is equal to the time taken by the bullet to travel its own length. The total pressure in pounds is calculated just as for a jet of water; it is $\frac{\lambda v^2}{g}$ where λ is the mass of the bullet in pounds per foot length and v the velocity.

The service Mark VI bullet is $1\frac{1}{4}$ inches long. At 2000 feet per second the blow should therefore last $5{\cdot}2 \times 10^{-5}$ seconds, and the shaft against which the bullet is fired should just be completely stopped by an end-piece about 5·2 inches long. It was found that in fact a piece 5 inches long traps $93\frac{1}{2}$ per cent. of the whole momentum of the blow, leaving $6\frac{1}{2}$ per cent. in the shaft, while with a piece 6 inches long the figures are $97\frac{1}{2}$ per cent. and $2\frac{1}{2}$ per cent. respectively. The measured duration of the blow is a little longer than it should be according to the simple theory. This is due in part to the fact that the bullet possesses some slight rigidity, so that the base is retarded a little during the impact instead of coming right up with unimpaired velocity. The base arrives therefore a little later than it would if the bullet were quite fluid. In part also it is due to the fact that the pressure is concentrated on a small area in the centre of the end of the rod instead of being uniformly distributed.

Good results were also obtained for the maximum pressure at different velocities, as shown in the following table:

Velocity (Ft. per second)	Calculated Pressure (Pounds)	Observed Pressure (Pounds)
2000	43,500	42,600
1240	16,700	15,700
700	5,320	5,450

The pressure per unit area in the centre of the spot struck by the bullet is (at 2000 feet per second) about 275 tons per square inch.

Having demonstrated by the experiments with bullets that this method is capable of giving within 5 per cent. the duration and maximum pressure of a blow such as that of a rifle bullet, I turned my attention to gun-cotton. The shaft used in most of the experiments was of tool steel, hardened at the end, $1\frac{1}{2}$ inches diameter by 6 feet long. The results are not yet quite ready for publication in detail, but are summarized in Fig. 10. The parallelogram marked 1 represents the momentum given to an end-piece 1 inch long when a 1 ounce primer is detonated in contact with the other end of the shaft. Its base represents the time during which pressure is applied to a piece of this length, viz., 1/100,000 second, and its height is the average value of the pressure exerted by the gun-cotton gases during the first hundred thousandth of a second—58 tons per square inch. The area of parallelogram 2 is the excess of the momentum given to a piece $1\frac{1}{2}$ inches long over that given to a 1 inch piece; its height is the average pressure over the interval of 1/200,000 second following the first hundred thousandth.

The stepped line *ABCDE* is a first approximation to the pressure-time curve of the gun-cotton. Really the pressure varies continuously, more or less in the manner shown by the dotted line. This line is drawn so as to give the correct average pressures, but is of course conjectural as regards the details.

A piece 3 inches long on the end of the shaft stops the shaft practically dead. This is quite a striking experiment. A similar experiment can be done on a smaller scale with a Fulminate of Mercury detonator. The shaft is this case may be ½ inch diameter and about 2 feet long. With 15 grains of fulminate in a copper tube, the shaft is almost stopped by a 1 inch piece, showing that the blow lasts in this case for about 1/100,000 second. The average pressure over that period is about 55 tons per square inch, the

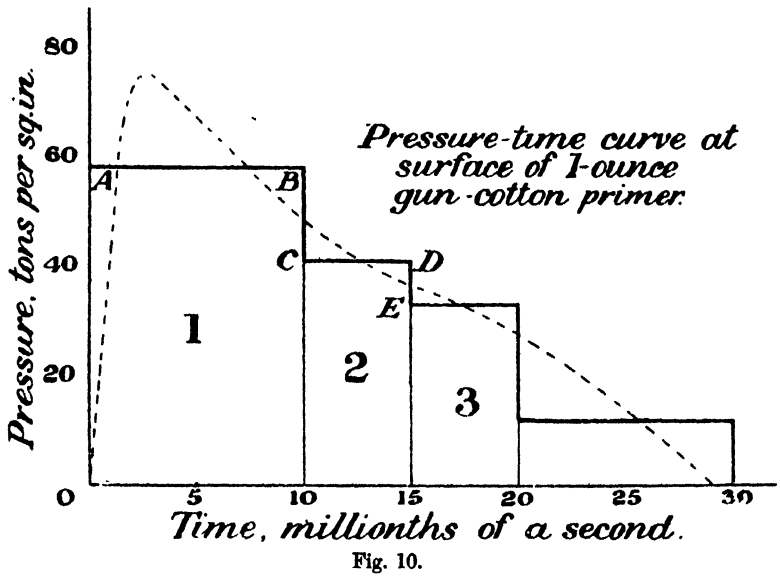


Fig. 10.

maximum at least twice as great. The study of detonators by this method may prove of practical value, because the power of a detonator probably depends mainly on the maximum pressure which it can exert on the surrounding explosive.

Further measurements with pieces of different lengths will enable the details of the pressure-time curve for gun-cotton to be filled in, and in particular will give a closer approximation to the maximum pressure, as to which it can only be asserted at present that it exceeds 60 tons per square inch. Meanwhile, the essential points so far may be expressed by saying that the pressure given by a 1 ounce primer lasts for about 1/50,000 second—nearly 90 per cent. of the blow has been delivered in that time—and that its average value is about 55 tons per square inch. A constant pressure of that amount acting for 1/50,000 second would have the same impulse, and for practical purposes the same effects, as the actual varying pressure of the gun-cotton.

Probably figures of the same order of magnitude describe the blow given by larger slabs of the same thickness (1¼ inches), though as the gas cannot get away quite so easily the pressure will last rather longer.

A more concrete idea of the meaning of these figures is perhaps given by a comparison with the pressure produced by lead bullets. A cylindrical bullet ½ inch long and ¼ inch diameter, striking a steel plate at 2100 feet per second, gives a total pressure of about 15 tons for 1/50,000 second. A number of such bullets distributed with about ½ inch spacing between the centres and striking the plate simultaneously would give a blow of about the same average intensity and duration as that delivered by a slab of gun-cotton 1 inch thick, detonated in contact with the plate and covering the area over which the bullets are distributed.

The resultant effect of applying such a blow to a sufficient area of a steel plate 1 inch thick is, as we have seen, to shatter the plate by developing a crack at the edge of the area to which the pressure is applied. We will now proceed to analyse this effect with the aid of the data which have been obtained as to the intensity and duration of the blow. Fig. 11 represents an edge-view of the plate. The plate is supposed to be a long strip say 8 inches wide by 1 inch thick, and the gun-cotton is a slab 8 inches square, covering the width of the plate. The pressure of the gun-cotton gases is practically confined to the area EE' covered by the slab, and the immediate effect of the pressure is to set this portion in motion, leaving the rest of the plate behind. The relative motion between the portions of the plate on the two sides of the line E gives rise to shearing stress in the neighbourhood of that line, and this is propagated by wave action both into the part EE', which is subject to pressure, and into the outlying parts of the plate to the left of E. The wave of stress set up in this way is rather complicated, but its most important part is a wave of pure distortion, such as is produced in a long cylindrical shaft if one end is suddenly twisted. The rate at which the twist is propagated along the shaft is about 11,000 feet per second, and this is roughly speaking the rate at which the effects of the blow given to the middle of the steel plate travel into the outlying parts.

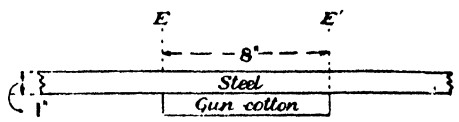


Fig. 11. Before firing.

Now consider the state of the plate 1/50,000 second later, when the pressure has just ceased to act. This is shown in Fig. 12 in which for clearness the deflections are much exaggerated. The waves of distortion which set out from E have got as far as F and G respectively. EF (and also EG) is the distance travelled by the wave in 1/50,000 second, viz., about

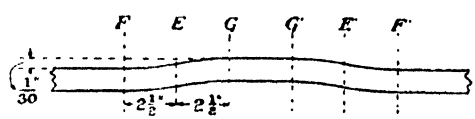


Fig. 12. 1/50,000 sec. after firing. The pressure has ceased to act. Outside FF' the metal is at rest and unstrained; inside GG' it is unstrained but has a velocity of 280 feet per second.

2½ inches. Outside the section FF' the metal has not felt the blow at all, and has not moved; within the section GG' the metal has not felt the drag of the remainder of the plate, and has moved under the influence of the pressure just as though it had been completely isolated by division in the planes GG'. The pressure which has been acting is 55 tons per square inch, and the mass of the plate is 0·28 lb. per square inch. The velocity which the part GG' has acquired is therefore:

$$\frac{55 \times 2{,}240}{0{\cdot}28} \times 32 \times \frac{1}{50{,}000} = 280 \text{ feet per second,}$$

and the distance through which it has been shifted is:

$$\tfrac{1}{2} \times 280 \times \frac{1}{50{,}000} = {\cdot}0028 \text{ foot, or about } 1/30 \text{ inch.}$$

The deformation represented by 1/30 inch on a length of a foot or more would of course have no serious effect upon a plate of mild steel if it were produced by any ordinary mechanical means. The steel having plenty of time to flow prefers to yield in that manner. But under the pressure of the gun-cotton gases, amounting to a total of several thousand tons, the bend is done in 1/50,000 second. At this speed, flowing is not the line of least resistance; instead of flowing, the steel prefers to crack. When a ductile solid is deformed, the stress produced depends among other things on the velocity of deformation, increasing with it. The stress will have as a rule a tensile component, tending to tear the molecules apart, but if the rate of deformation is not too great this component will not be sufficient to overcome the cohesive forces which hold the molecules together. So long as this is the case the material will simply flow without breach of continuity. But if the rate of deformation be increased beyond a certain point the tensile component of stress will surpass the cohesive forces. If this should happen, even for an excessively short time and over a comparatively small area, the material is there torn apart and the crack or tear thus initiated spreads with great rapidity. The material breaks suddenly and with little expenditure of work, like a brittle body. A stick of sealing-wax may be bent slowly through a considerable angle, and the force required to do so is small, but if it be attempted to bend it through the same angle rapidly a much greater resistance is experienced and it snaps. In the same way mild steel cannot be broken short in a testing machine or by the usual kinds of shock test because it cannot be made to flow fast enough to cause the stress to rise to a sufficiently high value. But under the more violent shock of gun-cotton it is shattered.

The subject of this paper is one of the bye-ways of engineering, and is somewhat remote from the more constructive topics which the members of the Institution are accustomed to consider; gun-cotton is an agent of destruction. Nevertheless, I think sometimes one may explore these bye-ways with practical profit because from them one can often get a point of view from which to look at the more straightforward constructional road which most engineers follow.

29

THE POSITION AND USES OF ENGINEERING LABORATORIES IN RELATION TO EDUCATION AT COLLEGE.

["The Institution of Civil Engineers."—Conference on Education and Training of Engineers, 1911. Section II: Scientific Training.]

The classification and analysis of the phenomena presented to the engineer in the practice of his art, conveniently called "Engineering Science," occupies a position intermediate between Biology, which is mainly based on the observation of Nature, and Physics, which is chiefly experimental. It is founded on the laws of motion, of heat, and of electricity, whose operation is investigated in the physical laboratory, but it is concerned with the working of these laws under conditions comparable in their complexity with those under which the same laws work in living things. This is obviously true of the greater natural forces, such as the winds and tides, whose action it is the engineer's business so far as possible to control. It is impossible to bring these things into the laboratory and to subject them to experimental analysis, though experiment may be of great help in interpreting observation of them. In other branches of Engineering Science, for example, in the study of the production and use of power, there is not the same physical limitation on experiment, but there is a commercial limitation which is just as effectual. Complete experiments on a full scale under commercial conditions can rarely be made because no one will pay for them, or allow his business to be disturbed while they are being made. Moreover, the commercial element, and the human element, are the most important among the conditions under which engineering phenomena go on and which determine their course. In a laboratory experiment they are as little reproducible as is a gale of wind. Thus Engineering Science must be based on the intelligent observation of things as they occur in practice. As in biology, experiments in an engineering laboratory may be of great assistance, but in interpreting their results, it must never be forgotten that the artificial control of conditions, which is of the essence of experiment, is of itself a disturbing factor, analogous to that general effect on the subject of a vivisector's operation, which always lessens, and may destroy, the value of his conclusions when applied to the normal animal. On the other hand, it cannot be doubted that the necessary difference between natural conditions and the artificial atmosphere of the laboratory is less far-reaching and its effects are more readily foreseen in engineering than in biology. Experiment plays a part which, though still subsidiary, is relatively more important. Especially is this the case in the applications of electricity.

The position of the engineering laboratory as an instrument of education is not quite the same thing as its position in relation to research, but is closely related to it. In a discussion, some 20 years ago, on the proposal to establish an Engineering School in Cambridge, it was pointed out by way of justifying the innovation to that ancient University, that it was as good an education thoroughly to test a steam-engine as to determine the velocity of light. It might well have been said that it was better, for in the engine-trial the student is introduced to phenomena under conditions which in their complexity resemble those of real life, and which can only partially be controlled. He can vary steam-pressure, load, and perhaps speed, but he cannot within the limits of an ordinary trial control such important factors as the dryness of the steam-supply, or the temperature of the cylinder-walls. Thus he learns in a simple way to use his judgment in interpreting data, and to recognize how in a complex mechanism it is impossible to vary a single element without at the same time affecting a number of others. This training of the faculties of criticism and observation is valuable to anyone, whatever profession he is going to follow, and it cannot be obtained in the same degree from the experiments ordinarily performed by students of similar standing in a physical laboratory. To the engineering student there is the additional advantage that he sees the application of scientific principles to things of the same sort as he will meet with in after life.

These considerations, together with a frank recognition of the fact that by no possibility can the conditions of practical work be completely reproduced in the engineering laboratory, lead to a fairly definite conception of its position and uses. In the first place industrial apparatus, even of a simple character, is too complicated to be used as a means for demonstrating elementary physical laws to students who are quite ignorant of their working. Hence work in the engineering laboratory proper should be preceded by a short course of ordinary physical experiment under the simplest possible conditions. Provision is made for this in nearly all college courses, and it is unnecessary to discuss it in detail. Assuming that he has thus acquired a knowledge of the fundamental scientific principles, the student's work in the engineering laboratory will be directed largely to tracing their operation under more complex circumstances. For this purpose small units of plant of a simple character are quite adequate. Everything that such a student can learn in an engineering laboratory in the time at his disposal he can get from a 10-H.P. engine, from a 5-ton testing-machine, and from plant on a similar scale in the other branches of his study. A triple-expansion engine teaches him no more than a compound; a four-cylinder petrol-motor little, if anything, more than a single-cylinder gas-engine. In fact the smaller and simpler machines are better for his purpose, because they are more easily handled, and he can be left more to himself. The Author remembers seeing in a technical school an

engine of several hundred horse-power, which was being tested by some fifteen or twenty students. They were taking readings according to a set scheme, one reading a thermometer, another a pressure-gauge, and so on, and all the readings were being entered on a beautiful printed sheet. On the same day he happened to speak to a schoolmaster, who described how he was in the habit of setting his boys, two or three at a time, to indicate the laundry-engine at his school, and test its steam-consumption with an improvised condenser. There cannot be much doubt that the schoolboys learnt more than the students, and that the money paid for that large engine would have been better spent on twenty small engines.

But, though small, the machines in the engineering laboratory should be real commercial machines, such as are bought and sold in the ordinary course of business, and the students should use them under ordinary working conditions. This is, of course, quite consistent with the use of the most elaborate measuring arrangements that the ingenuity of the professor or demonstrators can devise, provided that they are such as a commercial user of the machine would tolerate. For instance, a gas-engine may have thermometers stuck all over it, it may suck its air through a hole for the purpose of measurement, and it may discharge its exhaust into a calorimeter, but it is not to be run at less than its normal speed in order to get better indicator diagrams, nor is the valve-setting to be altered for the purpose of improving the measurement of the air. Extreme variation of conditions is necessary for the purpose of research; but the ordinary student is not doing research, nor is it his object to learn in detail about the working of a special machine. His use for the engineering laboratory is to learn generally how to extract from an experiment conducted under severe limitations all the information that it is capable of affording. In doing so he will consolidate the knowledge of natural laws, which he is supposed to have acquired previously in an ordinary physical laboratory, and will learn their limitations; he will also pick up many facts about the working of machines which may or may not be of direct use to him, according as he meets with these particular types in after life or not. But these are by-products, and in the Author's view the engineering laboratory should be designed and organized with a single eye to the primary educational use which has been indicated.

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