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THE PRINCIPLES OF SCIENTIFIC RESEARCH

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By

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To
MY WIFE

AUTHOR'S PREFACE

THE main reason for writing this book has been the author's belief that young men and women embarking on research would find their task easier if they could have at their disposal a book exclusively concerned with research and dealing with it as a whole. Thirty years' experience of industrial research have convinced him that such persons, whatever their previous education, inevitably discover that classes, textbooks, lectures and practical laboratory work, which have been the basis of their scientific knowledge, have not given them a master-key for opening the door to undiscovered knowledge. They have still to learn how research should be done. This stage of their learning is a groping process, popularly known as "learning by experience". It is the object of the present book to help them in this stage of their learning.

The author is only too conscious of omissions and inadequacies in his treatment. He would plead in justification that he was primarily concerned with meeting the requirements of young people just entering the field of research, and that while some of them might have an honours degree and an experience of some post-graduate work, others might have lower academic qualifications, yet possess a desire and a capacity to do fine research in the future. For these reasons the author has endeavoured to be brief, to present the matter as clearly and directly as possible, to keep mathematics to the barest minimum, and to give, whenever possible, simple examples illustrating the principles expounded.

It is hoped that the readers will find the book useful. They may not find it excessively cheerful. But the author believes that a realistic view of things can never be full of unqualified optimism this side of the millennium, and that no enthusiastic young scientist, determined to succeed, will be discouraged by a few difficulties.

The author sincerely thanks his daughter, Miss Ruth Freedman, for her meticulous checking of the manuscript, his colleague, Mr. Arthur Serner, for his help with the drawings, Messrs. Crompton Parkinson Ltd., for facilities for the typing of the manuscript, and his publishers for editorial assistance.

P. F.

London, *November*, 1948

FOREWORD

By JOHN W. T. WALSH, M.A., D.Sc.
Vice-President of the International Commission on Illumination

IT MAY well be that some of the older scientific generation will find it strange that a book dealing with the principles of scientific research should be needed at all. "What is its aim?" they will ask, and "for whom is it intended?" Such as these are still living in the days when scientific research was, almost without exception, taken up "for the love of it" and all other considerations were entirely secondary. They do not realise that to-day natural science is generally regarded, like medical science, as a career; at its highest, no doubt, a vocation, but nevertheless not to be divorced from all the other matters that a young man (or woman) takes into consideration when choosing his life's work.

Over and over again, a grammar school boy who has shown some aptitude in one or more of the science subjects in the general or higher schools examination decides that he would like to "go in for science" as a career, and in most cases further enquiry shows that to him the word "science" is synonymous with scientific research. Even the student at college is more than likely to fall into the tragic error of assuming that if he obtains a good honours degree he is, *ipso facto*, well qualified for a career as a research worker.

Sometimes, but all too seldom, a kindly senior points out the vast difference between ability to learn and those much rarer qualities which are needful for a successful research scientist. If only every student could be shown this distinction, in how many cases might he be saved from the ultimate disillusionment which so frequently results in adding one more to the ranks of those who, although capable and valuable members of the scientific community, nevertheless pass their lives under a perpetual sense of disappointment and frustration?

The prevention of such real tragedies is, I believe, the greatest function which this book can perform for the rising generation of scientific workers, and so I can only hope most earnestly that it will find its way into the hands of everyone to whom a career in science offers an attraction.

There is, however, a second important and growing class to whom the book is addressed; those who, while not themselves engaged in scientific research, have, nevertheless, such a profound influence on an individual or a group of research workers that they have it in their power completely to paralyse the efforts of even the most brilliant among them—until he decides to seek a more congenial atmosphere. The author has written so feelingly and so convincingly on this matter that, again, it is to be hoped that every business executive who is in any way influentially connected with a research organisation will read this book from cover to cover, will mark the particular passages applying to his case, and will learn the essential lesson that scientific research is as far removed from the ordinary run of business as business is far removed from, let us say, the arts or letters.

It is not only the business man, however, who needs to learn this lesson. To-day a large number of the nation's scientific workers are in the service of the State, and it therefore not infrequently happens that a considerable measure of responsibility for the work of a research group may happen to lie with a highly placed civil servant who, although of more than average mental ability and a trained administrator, is without first-hand knowledge of the essential qualities of scientific research or the vital needs of the research worker. His need, although not precisely the same, is in many ways analogous to that of the business man with a research team in his organisation. Both require and, it should be said, in most cases welcome, initiation into the principles of scientific research so that they may help and not hinder the work of those upon whose efforts, in the long run, any improvement in the national economy must largely depend.

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CHAPTER I

THE NATURE OF RESEARCH AND ITS HISTORY

THE object of this book is to deal not with the entire field of research, but only with research of one particular kind, that associated with science. Scientific research is, of course, not the only possible kind of research; there are many other kinds, as old and older, such as literary, theological and linguistic, and a few more recent, such as market research. All such non-scientific kinds of research are beyond the scope of this book. Indeed, the scope of this book must be further limited by the exact meaning which is to be attached to the terms "science" and "research".

It may appear at first sight that the meaning of the terms "science" and "research" are sufficiently plain to everyone to need no definition. On closer examination it can be seen, however, that the matter is not so simple and obvious. The term "science" has been defined many times, and the definitions are not identical.

Sir William Dampier defines science as "Ordered knowledge of natural phenomena and the rational study of the relations between the concepts in which those phenomena are expressed."

Bertrand Russell gives a wider definition: "Science, as its name implies, is primarily knowledge; by convention it is knowledge of a certain kind, namely, which seeks general laws connecting a number of particular facts. Gradually, however, the aspect of science as knowledge is being thrust into the back-ground by the aspect of science as the power to manipulate nature."

Of the two definitions, Bertrand Russell's is the more satisfactory. Sir William Dampier's definition relates only to scientific knowledge, and does not take into account either the application of such knowledge, or the power to apply it, towards control and change of man's environment. But though wider than Sir William Dampier's definition, Bertrand Russell's

definition is also open to a serious objection. It presents science as static, whereas it is intensely dynamic. The most important attribute of science is not knowledge, but its capacity for acquisition of knowledge. Knowledge which science contains is limited, frequently fragmentary and inaccurate, always liable to revision. The capacity of science to acquire knowledge is infinite.

A very different conception of science is presented by J. G. Crowther's definition. According to this, "Science is a system of behaviour by which man acquires mastery of his environment". This definition does emphasise the dynamic aspect of science. But it is open to objection that it is at once far too wide and far too narrow. It is wide enough to include the behaviour of peasants beating out a forest fire within the realms of scientific activity, which is certainly not Mr. Crowther's intention, and it is narrow enough to relegate the theoretical work of Democritus and the experimental work on electrostatics of Gilbert and of Dufay to a level below the performance of a competent designer-draughtsman.

Thus it may be seen that an adequate definition of science is not only necessary, but also difficult to frame. A perfect definition of science is, indeed, an impossibility, since an understanding of the nature of science, like science itself, changing with the passage of time, can only gradually approach to truth.

An adequate definition of science must be wide enough to include all its aspects and, at the same time, rigid enough to exclude all that is non-scientific in reasoning, knowledge, experience and action. It must, while excluding activities which (like the culinary art) are merely a haphazard accumulation of empirical knowledge and practices, include not only all the pure but also all the applied branches of science. As Joseph Needham says, there is no sharp distinction between "pure" and "applied" science—"There is really only science with long term promise of application and science with short term promise of application. True knowledge emerges from both kinds of science." An adequate definition of science, while excluding all practices of essentially magical nature, must include all genuine science even in its very early stages, however elementary and naïve. It must include a yardstick by which the magnitude of scientific achievement can be measured irrespective of the stage of development of the particular branch

of science and of science in general during the particular period. It must not only present science as dynamic, but take into account the fact that nature itself is not static, and that its laws are not immutable but change with time.

A definition which appears to satisfy the above conditions and to be a reasonable approximation to truth is:

Science is a form of human activity through pursuit of which mankind acquires an increasingly fuller and more accurate knowledge and understanding of nature, past, present and future, and an increasing capacity to adapt itself to and to change its environment and to modify its own characteristics.

Brevity is essential to any definition. Consequently no definition can give an exhaustive presentation of that which it defines. Its essential brevity is achieved at the cost of omission. It is well worth while to ponder over the views on science expressed by two of the greatest modern physicists. These views were not presented as definitions, but they convey the spirit that has animated true scientists throughout the ages.

"When you can measure what you are speaking about and express it in numbers," says Lord Kelvin, "you know something about it, and when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind. It may be the beginning of knowledge, but you have scarcely in your thought advanced to the stage of a science." These words reveal to us both the uncompromising exactitude of a master of the scientific method, and the humility of a great man.

The next quotations are from Millikan who, in his description of the development of science, and in particular physics, speaks of "This perpetual effort to reduce the complexities of the world to simple terms, and to build up the infinite variety of objects which present themselves to our senses out of different arrangements or motions of the least possible number of elementary substances." These words were written by Millikan when he was an old man: they have all the quality of the zest of perpetual youth. The second quotation is from the same work: "In popular writing it seems necessary to link every great discovery, every new theory, every important principle, with the name of a single individual. But it is an almost universal rule that developments in physics actually come about in a very different way. A

science, like a plant, grows in the main by a process of infinitesimal accretion." These words should be a deterrent to any who, having established themselves in the sphere of science, tend to develop a smug sense of superiority; even more, these words should be a rallying call to serious young scientists to whom the paths of science are still strange.

"Research" in all fields of human activity means continued search for knowledge and understanding. *Scientific research differs from other kinds of research in that it is a continued search for scientific knowledge and understanding by scientific methods.* This dual determination of the scientific nature of a research—determination by objective and by method—is of fundamental importance. Not all knowledge and understanding is scientific and if any one were foolish enough to search for the best spinet music or for understanding of a poem by scientific methods, he would not, in any sense, be engaged in scientific research. Knowledge and understanding of movements of heavenly bodies would, on the other hand, be scientific knowledge, but any one searching for such knowledge by unscientific methods, for example by study of theological works, would, most certainly, not be engaged in scientific research.

The meaning of the expression "scientific knowledge and understanding" follows naturally from the definition of science. The nature of "scientific method" in research is the matter with which this books is primarily concerned.

From the definition of science which has been advanced here it follows that scientific research is by far the most important constituent of science. This view is supported not only by the words of Kelvin and Millikan quoted above, but by the history of every science and the biography of every great scientist.

Yet scientific research has been slow in winning its rightful place, not only in society but even in academic circles. This has been partly due to insufficient appreciation of the dynamic nature of science. At all times there has been a tendency to value learning more than the capacity to discover new knowledge and achieve a new understanding. A man of learning was sure of respect, but a man with a capacity to discover new knowledge and achieve a new understanding could gain the respect of society and the recognition of academic circles only

if he was already a man of learning or if his discoveries reached the stage of established importance which could not be ignored.

Sir Humphrey Davy recognised Michael Faraday's power to discover new knowledge when the latter still had to acquire his learning. But Davy's action was not typical either of society or of the academic world, whose normal behaviour is more aptly illustrated by the treatment accorded to Waterston's kinetic theory of gases and to the early discoveries of Pasteur.

Even to-day, when the dynamic element of science is becoming increasingly evident, a university course in pure or applied science is a course of instruction in existing knowledge and not an education for discovering new knowledge. A student may graduate in a branch of science having had only a meagre taste of scientific research at the end of his course, or possibly none at all, and it is possible for a student to graduate with high honours without having the slightest capacity for original experimentation or thought.

Still, to-day those embarking on scientific research may console themselves by looking at the history of scientific research and thanking the good fortune which did not cause them to be born at the time when scientific discovery might be rewarded by death at the stake—to-day they have nothing worse to fear than poverty, obscurity, ridicule and frustration.

The history of scientific research is interwoven with the history of society. Exerting as it has done the greatest influence on the development of science and, through that, a profound influence on the development of society, it has itself been conditioned by the form and degree of development of society in which it existed. At any moment in history it has been conditioned by the degree of development of arts and crafts, of industry, of commerce, of means of communication; by social relationships; by philosophy; by religion—in fact by all the social forces of that period. Scientific research is not as old as science, since the scientific method of searching for scientific knowledge and understanding was impossible until science had reached a stage of development which made the conception of scientific method possible. Only when society reached a certain stage of development, and when science had reached a certain level, could scientific research be born.

Scientific research is essentially compounded of two elements—

observation, by which knowledge of certain facts is obtained through sense-perceptions, and reasoning by which the meaning of these facts, their interrelation, and their relation to the existing body of scientific knowledge, are ascertained as far as the existing state of knowledge and the investigator's ability permit.

Emile Boirac, who courageously devoted his energies to research in a most difficult and thankless field which has been the graveyard of scientific reputations—the field of psychic phenomena—expresses with great lucidity both the relationship of scientific research to modern science, and its essential nature.

"Mathematics appears to many the perfect type of established science, forever immobilized in the possession of eternal truths. Yet do we not behold, with each successive generation, a host of new thinkers, conquering new fields in the domain of thought? For a mathematician of genius, such as Descartes, Liebniz, Cauchy, Poincaré, for example, is not the real science that which he invents and to which he gives life?"

"Let us say the more complex a science is, the more difficult, the more recent its constitution, the greater is the part played by *researches* than by *knowledge.*"

Describing his research technique he says:

"The experimental method . . . consists essentially of four processes, disposed in the following order:

(1) Observation
(2) Hypothesis
(3) Experimentation
(4) Induction."

Boirac explains that observation may be either that actually made by the person conducting the research or by others whose evidence he accepts as sufficiently reliable; that hypothesis is a provisional interpretation of the observations; that experimentation is intended to verify the hypothesis; and that the final step, induction, is a definitive interpretation.

While Boirac's delineation of the essence of experimental method was intended to explain his own technique in the particular research he was conducting, it is equally applicable to any kind of modern scientific research. It is true even of mathematical research, in which mathematical symbols are the only experimental material.

It may be observed that for success in any piece of scientific research all four steps must be correct to the degree of approximation necessary for the solution of the problem. Greater accuracy is not necessary, and absolute accuracy impossible. If any step is incorrect, however, all succeeding steps will be incorrect. Thus, if the observation is incorrect, the hypothesis will be incorrect, the experiment intended to verify the hypothesis will be pointless, and the final conclusion valueless. If the observation is correct, but the hypothesis wrong, the third and fourth steps will fail. And if both observation and hypothesis are correct but the experiment is not, the final result will still be wrong.

Ancient science originated in observations and grew out of their accumulation. At this stage science consisted of the recording of observations and their co-ordination and of evolution of certain arbitrary standards.

In Babylon, about 2500 B.C., there were already standard units of length, volume and weight. These units, while arbitrary, were based on what appeared to be the most reliable and readily repeatable observations. Thus the unit of length was the finger, equal to about $\frac{2}{3}$ in.[1]. Twenty fingers were equal to 1 ft., and 30 fingers to 1 cubit. The pole and the cord were multiples of the cubit, the pole being equal to 12 cubits and the cord, which was the surveyor's cord, to 120 cubits. The league was equal to 180 cords. Similarly, the unit of weight was the grain, equal to 0·046 gramme.

Observations and their co-ordination enabled Babylon to evolve elementary mathematics, including multiplication tables, tables of squares and cubes, a duodecimal and a decimal system; they also enabled Babylon to develop astronomy to a degree which made possible the prediction of the relative positions of the sun and moon, as early as 600 B.C. Similarly observations and their co-ordination produced in Egypt, about 2500 B.C., elementary anatomy, surgery and pharmacology.

But during this period of history, the second of the four steps constituting scientific research did not exist; hypothesis had not yet made its appearance. This absence of hypothesis was not due to absence of effort on the part of Babylonian and Egyptian priest-scientists to interpret their observations, but

[1] Taken as the breadth of the finger.

to their mode of interpretation, which was not scientific but religious and mythological.

Christopher Caudwell defines the religious and mythological approach to interpretation of observations as "the belief that scientific phenomena are adequately explained by any symbolisation which includes and accounts for the phenomena." Caudwell finds that this faulty approach to interpretation of observations was not eradicated from scientific circles even in the eighteenth and nineteenth centuries, when it passed for good scientific reasoning. "Thus 'caloric' accounts for temperature phenomena. None the less no such mysterious stuff exists." Even to-day, Caudwell points out, this fallacious reasoning has not been entirely banished—it has found its way into the psychologist's study of the subconscious mind and its relation to the conscious. The defect of this form of reasoning is that it is capable of giving an indefinite number of interpretations, all of which are equally convincing to their votaries, and none of which is capable of experimental confirmation.

Caudwell's criticism of Joseph Black's caloric fluid theory implies that it is no more scientific than the Babylonian theories about the universe. Such criticism may appear far too harsh, but a closer examination of Black's caloric fluid theory reveals that it cannot be classed as a scientific hypothesis, which it merely resembles in form. The concepts of an imponderable caloric fluid, and of quasi-chemical compounds made by it with ice, water and steam, were merely arbitrary postulations added to previous scientific knowledge based on observation, and were unsusceptible to experimental verification, since the only facts that could be experimentally established about them were the facts they were designed to explain. This is not different in essence, but merely in form, from the Babylonian theory of the universe which merely added to previous scientific knowledge based on observation, an arbitrary story of Marduk which is unsusceptible to any experimental confirmation.

The penalty of religious and mythological interpretation of observations in science is not the stagnation of science, but its deterioration. In Babylon, applied to astronomy, it converted it to astrology. In Egypt it led to the countenancing in medicine of amulets and charms.

It was not till the sixth century B.C. that Greek science added hypothesis to observation. Thales of Miletus was the first great figure in the Ionian nature philosophy. The fact that his hypothesis, his theory of the universe and its manner of evolution, is unacceptable in the light of modern scientific knowledge, is of minor importance compared with the fact that he substituted rational reasoning for mythology in explanation of natural phenomena.

Following Thales, three other Ionian philosophers, Anaximander, Anaximenes, and Heraclitus, by their interpretations of the universe and its origin, reaffirmed the position of the scientific hypothesis as an integral and essential part of scientific investigation. The amount of scientific knowledge available at the time was too small to impose a degree of limitation on an hypothesis and permit it a degree of precision customary in modern science. The hypotheses of these four Greek philosophers were only qualitative in content. There were considerable differences between the four hypotheses, and no scientific data was available to justify any rational preference for any one of them. Furthermore, they were not the only hypotheses possible at the time concerning the nature and origin of the universe, as other Greek philosophers quickly showed. Finally, no means were available, or could at the time be devised, for any experimental test of these hypotheses. It is a proof of the value of scientific hypothesis, as a step in scientific investigation, that, in spite of all this, each of these four Ionian philosophers' hypotheses contributed something of great value to science—Thales, the idea of a cycle of change; Anaximander the ideas of the earth unsupported in space, of a common indeterminate substance, and of land life evolving from sea life; Anaximenes the ideas of rarefaction and condensation; Heraclitus the ideas of universal flow and change, and of opposite tension.

The next, possibly the most important, advance in the history of scientific research was due to the work of Pythagoras and his disciples. The quality of a scientist's work is not dependent on his religious beliefs, but on whether he does or does not allow the religious and mythological approach to determine and become an integral part of his interpretations of observations. Pythagoras was a religious reformer. He was

also one of the greatest Greek scientists. The fame of Pythagoras now chiefly rests on his great discoveries in geometry and on his theory of numbers. His contribution to development of scientific research is, however, probably of even greater importance.

Pythagoras developed the quantitative content of the hypothesis which, added to the already existing knowledge of possibilities of a hypothesis with a purely qualitative content, produced the true scientific hypothesis not essentially different from the type of hypothesis which constitutes the second of the four steps of modern scientific research. He also developed the third and fourth steps of scientific research—experimentation and induction.

The meaning and value which Pythagoras attached to hypothesis and to experimentation were, however, different from those now accepted in modern science. To Pythagoras the content of a hypothesis was a much more certain way to scientific knowledge and understanding than either the observation which preceded it or the experimentation which followed it. He saw the function of experimentation as that of assistance to a hypothesis based on *a priori* mathematical reasoning, not as a test of the validity of such a hypothesis. This implied that the final conclusion had always to be in agreement with the hypothesis which was perfect, and if experimental evidence did not agree with the hypothesis, the final conclusion should explain this away by an appropriate interpretation of the experimental evidence. This point of view could not have any serious adverse effect on discoveries in geometry and in the theory of numbers, but it could and did have a serious adverse effect on Pythagorean cosmology, in which an invisible "counter-earth" was included in the final induction because repeated observations revealed one planet less than was postulated in the hypothesis. It is probably this view of experiment and hypothesis which explains why Pythagorean acoustics, a scientific achievement of the highest order, contain no reference to the vibration frequency of a string being proportional to the square root of stretching weight, although the effect of variation of weight was apparently investigated: had the square root entered into these acoustics, it would have brought in with it the irrational numbers. The modern

conception of the hypothesis as something quite tentative, which must stand or fall by the evidence of experiment, did not emerge until a later period of history.

The preference of Pythagoras for mathematics as against experiment as a means of acquiring new knowledge and understanding is quite comprehensible, since at the time progress in mathematics and the certainty that each step forward was not only correct but opening wider vistas was already possible, while experimental science was still in its infancy, and could grow only through development of devices for translating sense-perceptions into scientific information. To-day the mathematician needs nothing for his research except pencil and paper, some books of reference and drawing instruments; the experimentalist needs a well-equipped laboratory, and has frequently to add to it new devices of his own invention, before he can hope to get results. Hence even to-day there are scientists whose attitude to mathematics and experiment, though different from that of Pythagoras in form, is essentially the same in substance. Young scientists seeking to master the right technique of research, and overawed by modern advocates of the Pythagorean attitude, should remember the warning of Lord Kelvin: "Nothing can be more fatal to progress than a too confident reliance on mathematical symbols; for the student is only too apt to take the easier course, and consider the formula, and not the fact, as the physical reality."

It is because experimental science could grow only through development of means of translating sense perceptions into scientific information, at least qualitative, but preferably quantitative, concerning natural phenomena, in circumstances when unaided senses could at best give only vague qualitative information and at worst give no indication of the existence of the phenomena, that the discovery by Empedocles of means of demonstrating that the invisible air was something material, and capable of exerting a force, marked another important stage in the development of scientific research. Democritus, without devising any apparatus for experimentation, made a much more fundamental contribution to development of scientific research by his realisation that sense perceptions were merely man's reactions to physical reality and therefore,

unaided, inadequate to give complete knowledge of that reality.

Not all branches of science needed special devices before experimentation could be carried out on an ambitious scale. In such cases the experimental method was applied in the early stage of development of the particular science without any *a priori* mathematical reasoning, and indeed with a minimum amount of hypothesis. This was the case in physiology, whose beginnings may be credited to Alcmaeon, a contemporary of Pythagoras. But branches of science which appear to be exceptionally easy to explore by the experimental method in their early stages, in their more advanced stage of development make demands on the experimental technique just as exacting as any other science. Alcmaeon discovered the optic nerve in the sixth century B.C., using the elementary experimental technique then available; the physiology of the eye related to the phenomena of colour vision is still inadequately explored, in spite of very great improvements in the experimental technique.

The modern conceptions of the hypothesis as a provisional interpretation leading to and verified by the experiment, and of the planned experiment as the decisive test of the validity of theory, first emerged in the works of Archimedes, the founder of the science of statics and the first, as well as one of the greatest physicists. Archimedes not only appreciated the importance of combining mathematics with experiment, the necessity of unity of theory and practice in scientific research, but also the importance of proceeding to acquisition of new scientific knowledge and understanding, not by striving to grasp the whole universe in a single attempt, but by pursuing a succession of limited and clearly defined objectives.

The influence of society on the scientist, on development of science and on scientific research, is well illustrated by the history of Ancient Greece. The economy of Ancient Greece was based on slavery, and while it gave Greek gentlemen the leisure and material security which enabled those among them who combined intellectual power with a desire for knowledge to engage in contemplation and reasoning, it made them despise the effort and skill of a man's hands. Such conditions were the greatest hindrance to experimentation, and made

development of sciences originating in pottery, metal work, dyeing and other crafts, such as the science of chemistry, completely impossible. Physics failed to develop except in the sphere of acoustics, which were connected with music and therefore not contemptible. Knowledge was prized, but the acquisition of new knowledge through scientific research was rejected in favour of acquisition of new knowledge through philosophic discussion. The great intellectual figures in Greece after Pythagoras and before Archimedes—Socrates, Plato and Aristotle—were not experimentalists but philosophers.

Socrates thought that philosophy should not concern itself with natural science but with ethics. Plato condemned experimentation either as contemptible, being based on crafts of slaves, or as irreligious, and developed theories about the universe in which—to quote Plutarch—"he made natural laws subordinate to the authority of divine principles." Aristotle was the only one of the three who was not only a philosopher but also a great scientist.

Plato's attitude was antagonistic to scientific research, and, because he was a great man, the harm he did to scientific research was grievous and long-enduring. Aristotle's influence was quite different. He was not only a great philosopher, but also the most widely and deeply learned of ancient scientists. No other scientist in ancient history approached his encyclopaedic knowledge and his systematised presentation of that knowledge, to which he contributed the results of numerous personal investigations. He developed the technique of classification of phenomena. He created formal logic, with its syllogisms and its apparently incontrovertible conclusions. One might have expected to find that such a combination of intellectual power and scientific knowledge would have resulted in most valuable contributions to development of scientific research. But it did not.

Alcmaeon concluded that the brain was the organ of intelligence, but Aristotle returned to an earlier view that the organ of intelligence was the heart. Aristotle rejected entirely the atomic theory of Democritus, his conception of vacuum, his teaching that there was no absolute "up" or "down", "lightness" and "heaviness", and that the qualities attributed to matter—colour, taste, feel and smell—were only sense-

perceptions, engendered in the observer by physical reality. Astronomy, in Aristotle's hands, became theological, and was responsible for the long neglect subsequently meted out to the brilliant theories of Aristarchus. Finally, Aristotle's formal logic proved a grave hindrance to scientific progress, because it was essentially a technique for obtaining the only possible conclusions from unquestionably true premises, whereas in science no premises can ever be absolutely true, all premises are open at all times to a test of their validity by new evidence, and no conclusions can be regarded as final and unalterable. The influence of Aristotle upon science survived into the Middle Ages, and it is not too harsh to say that, because of his mistakes and his great authority, this influence did a great deal of harm to scientific research.

The influence of Aristotle on scientific research deserves the closest examination because it leads to a number of important conclusions. One of these is that formal logic, when applied to scientific research, is a fatal form of reasoning. Another conclusion is that it is possible for a man to possess the highest intellectual power and greatest scientific learning without possessing a corresponding gift to discover, or even to appreciate the discovery, of new knowledge and new understanding. The third conclusion is that a man with little scientific knowledge may make scientific discoveries which may not be appreciated by a learned authority. And the final conclusion is that in scientific research, whose very nature consists in discovering that which is not yet known, authority must be a modest guide, and not a judge from whom there is no appeal.

It is a strange irony of history that Socrates, who has not been classed as a scientist, who did not attempt to probe the mysteries of the universe, but was concerned only with ethics, should have made two of the greatest contributions to scientific research. His first great contribution was his practice of formulating universal definitions. Without precise definitions, scientific research could not have reached anything like its present stage. His second great contribution was his method of reasoning. The importance of this to scientific research is not yet fully realised. Its value as a technique in scientific research is so high that it will be considered more fully in another chapter.

CHAPTER II

RESEARCH AND SOCIETY

IN THE previous chapter we have examined the nature of science and of scientific research, and briefly reviewed the development of scientific research in Ancient Society. We have seen that while scientific research is by far the most important constituent of science, and essential to acquisition of new scientific knowledge, scientific research and scientific knowledge are by no means synonymous, and learning and capacity for scientific research are not only not synonymous but need not be simultaneously manifested.

Indeed, history since Archimedes shows that there were periods when the value of science was recognised, but scientific research languished, and other periods when science suffered an eclipse, yet certain brilliant solitary scientists made most important contributions to the methods of scientific research, though it was long before their contributions were accepted.

This book is concerned not with science as a whole, but with scientific research; not with scientific discoveries, but with principles of scientific research. For these reasons history of scientific research, as here presented, must of necessity differ considerably from a history of science. Many names and achievements, given legitimate prominence in the history of science, must be omitted as not relevant to development of methods of scientific research, while other names and achievements, less conspicuous in the history of science, must be given prominence because of their importance in the development of methods of scientific research which made possible the great discoveries of later scientists.

The most important contribution to methods of scientific research following Archimedes's death was made in the second century B.C. by Hipparchus, who invented plane and spherical trigonometry, and devised various instruments for astronomic observations. Aristotle's influence was still dominant. Long

after his death his hand of authority lay heavily on scientific progress. Ecphantus advanced the theory of the rotation of the earth about its axis, and Aristarchus, the theory that the earth rotated round the sun and that the stars appeared motionless because of their great distance from us. But these theories were rejected until their revival by Copernicus. Ptolemy, whose researches on optics were of the highest order, succumbed to mythological and religious influences to the extent of writing an astrological work. Plato's philosophy concerning the nature of matter prevented the development of chemistry, and led instead to the appearance of alchemy. When the Romans became the dominant world power, and Greek science had to shelter under the Roman wing, matters became worse. Romans recognised and encouraged science, but only in so far as it could give immediate results of practical value. This policy resulted in various successful applications of science, but quickly proved fatal to scientific progress.

Nevertheless, there were two important contributions to the method of scientific research about the third century A.D. The first of these was the work of Hero, who was the pioneer of scientific research in engineering and whose steam turbine anticipated Sir Charles Parsons's rediscovery of the principle by some 1,500 years. The second was that of Diophantus.

Diophantus was the creator of algebra in the modern sense. He introduced symbols for quantities, and operations, which were previously handled either by geometry or by verbal argument. This was an innovation of the highest importance. Previously mathematics could not be separated from material objects, or at any rate from geometrical forms visible to the mind's eye. Diophantus made mathematics a science of pure quantities, independent of any objects. By so doing he made it possible for mathematics to grow to a great stature, and develop into a powerful instrument for scientific research. But by the same act he made possible the severance of mathematics from the physical reality which gave them birth. Henceforth mathematical processes could have a life of their own, divorced from natural phenomena. This two-fold importance of mathematics as a science of pure quantities did not become apparent until much later—until the nineteenth century, when both mathematics and scientific research had

advanced far beyond anything imaginable in the third century B.C.

With Diophantus the progress of science in ancient society came to an end. Gradually the ancient civilisation decayed and disintegrated, and the world entered the long period of the Dark Ages. In Europe theology took the place of ancient science and philosophy. This theology was a compound of beliefs and arguments of the Fathers of the Christian Church, some Plato and less Aristotle. At the beginning of the Dark Ages, Aristotle's influence was still important, but faded by the sixth century, and for the next seven centuries only commentaries on his Logic were not entirely forgotten. The circumstance that Europe, which had forgotten science, still retained a lingering regard for Aristotle's Logic, is not strange, for his Logic, as shown in the previous chapter, was an obstacle to scientific enquiry. The Christian Church was antagonistic to objective study of natural phenomena and to manifestations of spirit of critical enquiry. It rejected understanding and knowledge based on observation and experiment, and insisted on acceptance of its doctrines and an unquestioning belief in all pronouncements of the Fathers of the Church—even when these pronouncements were as fantastic as the declaration that lion cubs are invariably born dead and only given life on the third day to symbolise the Resurrection of Christ.

That science did not perish entirely during this eclipse of civilisation was due to the Arabs. Arab science grew simultaneously with Muslim theology. This theology had borrowed certain elements from Buddhism, which preached love of knowledge and respect for reason. Consequently it was not as opposed to science as was the theology of the Christian Church of that period. While it denied both the actual existence of matter and the duration of any phenomena, it at least did so on the basis of a theory of atomicity of space and time, which was on a higher plane than the Fathers of the Church's story of the resurrection of the lion cubs. Thus Muslim theology did not advocate the rejection of science, but merely allocated it a position of secondary importance.

The Arab school of science has much to its credit. Abu-Musa-Jabir-ibn-Haiyan conducted scientific experiments in chemistry,

and was probably the true founder of that science. Abu-Bakr-al-Razi, the greatest physician of the Middle Ages, advanced both medicine and chemistry by interlinking them. Ibn-al-Haitham carried out experimental researches in optics. The most revolutionary advance in the methods of scientific research to the credit of the Arabs was, however, achieved in Spain, in the twelfth century, by Averroes.

Averroes declared that religious belief and scientific knowledge were completely different things, and that a theology compounded of a mixture of the two was injurious to both. Thus Averroes was the first to start the battle for emancipation of science and of scientific research from religion and mythology. This battle was to go on for many centuries. It had to be fought by Copernicus and Galileo for astronomy, by Darwin and Huxley for biology, and Christopher Caudwell has pointed out that before long it will have to be fought for psychology.

Slowly Europe emerged from the Dark Ages into the light of a new civilisation. Before scientific research could be revived, it was necessary first to rediscover some of the lost ancient knowledge, and after that to loosen the bonds of theology and authority upon the enquiring mind. The first step was achieved in the thirteenth century, principally through rediscovery of Aristotle, and, to a lesser extent, of Galen, and through study of Arabic commentaries. The second step was not openly achieved until the sixteenth century; but it was achieved a good deal sooner by a few great scientists who had transcended the limitations of the society in which they lived, but were compelled by it to work in solitude and silence.

One of them was the amazingly versatile genius, Leonardo da Vinci, whose fame for a long time rested solely on his achievements as painter, sculptor and architect, but whose unpublished notebooks have revealed him as also one of the world's greatest scientists. He was an engineer, physicist, biologist and philosopher, in each of which activities he revealed himself as a master. He rejected Aristotle as a scientific authority, and accepted the works of Archimedes as the true starting point. His work reveals his complete emancipation from theology and from the dead hand of authority, and his acceptance of observation and experiment as the only way to scientific knowledge. Could his work have

been published and accepted in his time, science would have at once taken an enormous leap forward. But the time for such publication was not ripe. In spite of the fact that Leonardo da Vinci was famous in his lifetime, and a friend of princes and learned men, his scientific discoveries were doomed to long forgetfulness by a society which was not ready to receive them.

Great as Leonardo da Vinci was, he was not the first great scientist since Averroes. There were certainly others whose scientific achievements were obliterated far more effectively. Roger Bacon, who was himself a champion of the cause of scientific research rather than a scientist, gives glimpses of these shadowy figures. Crying out "Cease to be ruled by dogmas and authorities; look at the world!" and going so far in his rebellion against authority as to say that Aristotle's books should be burned as an obstacle to progress, Bacon speaks with much regard about various contemporary scientists, and with special respect and admiration of Peter de Maharn-Curia, whom he describes as a supreme master both of mathematics and of experimental research. Though, according to Bacon, de Maharn-Curia was the most accomplished mathematician of his time, he regarded "the science of experiment", in which he had no rival, as far above all other sciences. All reasoning, all conclusions, however convincing they might appear, could not be regarded as valid unless and until they had been tested and confirmed by experiment. This conception of science and of scientific research, of the supremacy of experimental evidence over unsupported theory, and of the value of all speculations, calculations, and interpretations, as limited to their guidance to experiments until such experiments have confirmed their validity, was a tremendous advance in scientific thought and method. Had it been accepted, such theories as that of heat as an imponderable fluid, which managed to survive into the nineteenth century, could never have established any hold in the scientific world.

Who was Peter de Maharn-Curia? What were his discoveries? According to Bacon he was indifferent both to fame and to wealth, and published nothing that he discovered. How far was Bacon's scientific outlook due to his inspiring

influence? To what extent was Bacon's faith in the ultimate practicability of power-controlled ships, carriages and flying machines due to the same influence? The questions must remain unanswered. Medieval society has wiped the slate clean of all records of Peter de Maharn-Curia's work; nothing but his shadow remains.

Joseph Needham has said that a scientist's work is inevitably conditioned by the society in which he lives: the nature and the stage of development of the society impose limitations upon his choice of subject of his research, and determine the facilities at his disposal, the knowledge available and a philosophy of the period which he accepts, consciously or unconsciously, and by this determination influence to a high degree his approach to his problems, his technique and his conclusions. Certainly, as far as the very great majority of scientists are concerned, including many of the highest order, history has confirmed the truth of Needham's words. Sometimes, however, there have been isolated geniuses whose intellect could transcend the limitations of the framework of society in which they lived, who refused to be deterred by the limited facilities and knowledge available to them, created their own philosophies, evolved their own technique, and reached conclusions which, had they been accepted at the time, would have enabled science to make an enormous leap forward. But such solitary geniuses did not escape the power of society. Society, which has honoured scientists whose discoveries have been acceptable to it, has been harsh to those whose discoveries have been too far in advance of its stage of development. It has put such premature scientific discoveries aside and ignored them for more than a thousand years, as it has done with Hero's turbine; or it has sought to suppress such discoveries by use or threat of torture and death, as the Inquisition did with Galileo; or it has obliterated them, as the thirteenth century world did with the work of Peter de Maharn-Curia.

Christopher Caudwell defines a scientist as one who can teach men how to control events if they wish, but cannot teach them what to wish. In formulating this definition, Caudwell was not dealing with all scientists, but only with a particular kind of scientist. Taking the term "scientist" in

its widest sense, one might say that a scientist is one who can teach men concerning phenomena if they wish, but cannot compel them to wish to be taught—the wish must come from themselves. When a society is ripe for certain discoveries, the scientist who achieves them can have a profound effect upon the course of science, and perhaps of society itself. When a scientist makes discoveries for which the society is not ripe, his discoveries remain socially valueless until they are rediscovered by someone more in harmony with the needs of his period.

It is the practice of society to honour the rediscoverer more than the premature discoverer, not only after acceptance of rediscovery, but more or less permanently: this is no doubt due to the fact that society feels gratitude to the man whose work has benefited it, and cannot feel a similar gratitude to a man whose work, in spite of, or perhaps more accurately because of, its brilliance, has been of no help. Copernicus is more honoured than Aristarchus, and Sir Charles Parsons more than Hero. In this there may be some measure of justice, just as there is a measure of justice in students' preference for a good teacher who is not a great scientist as against a great scientist who is a bad teacher.

But is there any such element of justice in the case of scientists or scientific bodies, who, enmeshed in the ideology of their society, follow, and by following tend to perpetuate, this society's practice of paying greater regard to rediscoverers than to discoverers of scientific truths? This is more than doubtful. Such an attitude on the part of scientists, or of scientific bodies towards discoverers, is not only antagonistic to the discoverer, but to the discovery also, and, by inference, to all discoveries too revolutionary for ready acceptance, and therefore to the cause of science of which they should be champions. Such failure to champion the cause of science is not, however, due to any conscious treachery to the cause—it is merely another illustration of how completely the thoughts and actions of scientists may be conditioned by the society.

The sixteenth century, particularly towards its end, witnessed the first signs of the upsurge of the spirit of scientific enquiry and the rebirth of science, or rather the birth of modern science, which during succeeding centuries rapidly attained a knowledge

and understanding of natural phenomena far exceeding those of ancient science. Apart from Copernicus there were many other harbingers of the scientific age. Agricola's work on mineralogy formed a prelude to geology; Paracelsus experimented with chemicals for medicinal purposes; Valerius Cordus in the course of his medical work definitely passed from the quasi-alchemic chemistry to true chemistry; Fernel and Vesalius carried out important experiments in anatomy, and Fernel initiated both anatomy and physiology in the modern sense; Janssen invented the compound miscroscope; Gilbert carried out both qualitative and quantitative experiments in magnetism and electrostatics. The most important contributors to development of methods of scientific research were, however, three men whose work extended from the later part of the sixteenth century into the seventeenth century: these were Francis Bacon, Sanctorius, and, by far the most important, Galileo.

Francis Bacon realised that new scientific knowledge could only be attained by way of experimentation, and that for this purpose Aristotle's deductive logic was not an aid, but an obstacle. This led him to attempt to evolve a different type of logic, more suitable for the advancement of science. Bacon's inductive logic was the result. Inductive reasoning is implicit in all scientific reasoning, for even if the reasoning is in the form of establishing the applicability of a general law to a particular instance, which may appear as an example of deductive logic, there is the implicit assumption of the validity of the general law, which could only have been established with the aid of inductive reasoning from the particular to the general. But in believing that his inductive logic would prove a master-key to all doors leading to nature's secrets, Bacon went too far. His belief that application of his inductive logic would automatically give the right answer to a scientist possessed of factual information through observation and experiment, involved three fallacious suppositions. In the first place, it implied that the sum total of observations and experiments would be finite, and therefore that their meaning could be fully grasped by a man of finite capacity, whereas scientific knowledge is, by its very nature, capable of infinite extension, so that a scientist aiming to unravel the secrets of nature would, according to Bacon's method, have to acquire an infinite

amount of observational and experimental information and then display an infinite capacity for grasping it and drawing his conclusions. In the second place, both observational knowledge and experimental knowledge can be acquired only gradually, in steps, as each discovery makes the next discovery possible. Finally, Bacon assumed that it would be possible for a scientist to obtain an absolutely correct answer to his question, whereas in science all answers which can be obtained, however convincing they might appear at the time, can only be tentative approximations to truth, and may at any time be shown untenable in the light of fresh evidence.

Nevertheless, Bacon's inductive logic, while not a master-key to scientific problems, was of positive value to scientific research because it did lay emphasis upon observation and experiment, and did provide an antidote to the poison of *a priori* reasoning in thin air as a guide to scientific knowledge.

Sanctorius, whose scientific discoveries were not as important as those of Vesalius or Gilbert, made a much more important contribution than either of them to the methods of scientific research. He was the first scientist to realise the importance of applying the knowledge in one branch of science to scientific research in another. He applied the methods of physics—the thermometer, weighing, and a method of measuring the pulse —to his medical investigations. At the time, with various branches of science still in their infancy, this new method of scientific research could not have the profound influence on the course of science which it became capable of exerting later. Even so, it bore good fruit at once, and its value has continued to increase with the passage of time.

The most important scientist of the period, and one who contributed most at the time to development of scientific research, was Galileo. He is now generally regarded as the first of modern scientists. He was also the first to conduct scientific research in the modern sense. In the history of science his lasting fame is chiefly based on his work in astronomy, and perhaps even more on his initiation of the science of dynamics. In the history of scientific research he stands out not only as a great research worker in the modern sense, but also as the founder of several most important principles, to which the achievements of modern scientific research are largely due.

The first great principle he introduced into the technique of research was that before one could attempt to formulate the causes of phenomena one had to ascertain as correctly as possible what the phenomena actually were, and how they were related with certain other phenomena. Thus, his discoveries in dynamics were largely due to the fact that instead of aiming to elucidate why bodies moved in various ways, he concentrated his effort in finding how they moved in various circumstances. Gravity itself he never attempted to explain. He did not, in the course of his researches, reject all attempts to explain the causes, but as such attempts were made at the end of a particular research, and not at the beginning or the middle of it, they were not only more likely to be approximations to truth, but if they were incorrect the mistake could not have the same serious consequences because the main body of the scientific work performed still remained sound. This is well illustrated by his work on the height of liquid columns, the bulk of which remained sound in spite of the fact that his final speculation about the limiting height of such columns was incorrect.

His second great contribution to the methods of scientific research was his insistence on objectivity in observation. He did not merely rediscover the theories of Democritus; he transferred them from the sphere of philosophy to the sphere of experimental investigation. He differentiated the properties of a body not as primary and secondary, as did Kepler, but as characteristic of the body and characteristic of the observer. Thus number, size, shape, relative position, state of rest or motion, and, a most important conception, position in time, were attributes of the bodies; colour, taste, sound and smell associated with these bodies were not attributes of the bodies, but characteristic of the observer, being sensations produced in the observer by the action of these bodies upon him by virtue of their number, size, shape, weight, spatial relationships and manner of motion. Experimental investigations must therefore involve the translation of sense perceptions, in part characteristic of the observer and in part characteristic of that which is observed, into objective information concerning that which is observed.

His third great contribution to scientific research was in the

manner of interpretation of observations and experiments. One of the principal features of this manner of interpretation was the allocation to space and time of importance which they still retain in modern science, and the other a rule that it is far better to attempt no interpretation at all, and to say frankly that one does not know the answer, than to advance a speculative explanation for which there is no basis of scientific knowledge.

A fourth great contribution to methods of scientific research made by Galileo was in devising simple experiments, involving the use of readily available experimental means, from which important theoretical conclusions could be drawn. His experiments with bodies rolling down and up inclined planes are an example of this.

One more, and by no means the least important contribution to methods of scientific research made by him, was the devising and construction of scientific instruments and their application to scientific research. His invention of the thermometer and his application of the telescope to astronomy were events of the highest importance to science, not only in the immediate results they enabled Galileo to achieve, but also in the establishment of the principle that scientific research can conquer new fields only by continued development of new devices to aid man's senses in perception and study of natural phenomena. The champion of vested interests in ignorance, a Professor of Philosophy who refused to look through Galileo's telescope, exhibited an unerring instinctive fear of the methods by which scientific research was going to achieve its victories.

With Galileo modern science and scientific research at last enter into their own in spite of the hostility of social forces still in power. The Inquisition tried to suppress Galileo's cosmology, but though its threats of torture and death extorted from Galileo a formal recantation, it could neither obliterate the scientific truths he had discovered nor make him forget them. The inquisitors could not destroy the march of history, and history was not with them but with Galileo. "And still it moves!" has echoed down the ages to make all men remember that a ruling minority which seeks to reverse the course of social development, and an authority which seeks to perpetuate

discredited beliefs, are alike doomed to failure, and that all their ruthlessness can achieve nothing but to earn them the hatred and contempt of their ultimate judges.

Before proceeding with an examination of the development of scientific research since Galileo, it is necessary to explain why practically nothing has been said of Kepler, who has a very high place in the history of science. The reason is that this book, as already explained, is concerned not with science as a whole but with scientific research, and not so much with actual discoveries as with methods of scientific research. To methods of scientific research Kepler contributed virtually nothing. His guiding principles were the Pythagorean theory of perfect numbers as a basis of cosmogony, and a revival of Aristotle's aether, which, in various guises, has plagued science to this very day. The reason why Kepler's work was of value to science is that no method, however old, and no hypothesis, however faulty, provided they be not devoid of genuine scientific content, can be dismissed as absolutely valueless to scientific research, just as no scientific method or hypothesis can ever be regarded as free from imperfections and fallacies. The theory of perfect numbers has proved an invaluable guide to most fruitful action, and even to-day Aston's work on isotopes shows that its potentialities are far from exhausted. Even aether, which is now no longer accepted as real, has, in its various modifications, played a part in the development of science. But, inasmuch as Kepler did not bring anything new to the methods of scientific research, he can have no place in the history of development of such methods. For the same reason the names of many great scientists and discoverers must be omitted from subsequent pages, while the names of less famous persons must be included.

The history of science from the seventeenth century up to the end of the nineteenth shows an increasing tempo of development through discoveries, each of which has paved the way to more discoveries in a manner that resembles the spreading of a luxurious vegetation through seeds giving rise to plants which sow more seeds around them.

The reason for this remarkable development was not a spontaneous appearance of a new human race, endowed with scientific abilities far surpassing those of human beings who had

populated Europe during the Middle Ages, but the changed attitude of society towards science. Feudalism, with its system of power and authority based upon land ownership, its serfdom and its doctrine of the fundamental inequality of man, was displaced, after a bitter struggle, by a bourgeois society in which industry and commerce were of paramount importance. Science, showing its capacity to further both industry and the means by which commerce could thrive, such as means of communication and transport, quickly secured an increasing support from sections of the population enjoying the privileges of wealth and power and desirous of increasing them. This tendency was greatly augumented by the circumstance that slavery and serfdom, which alike provided the ruling classes with the ownership of large numbers of most ingeniously-fashioned mechanisms—the supposedly inferior human beings —was replaced by a system of society in which no man of wealth could buy a poor man, but could only buy from that poor man the use of his power to labour on tasks assigned to him for a certain short period, at so many hours a day. It therefore became necessary for anyone engaged in industry and commerce to consider carefully whether a new machine, or a new process, might not be a better way to success than the purchase of large quantities of labour power which had to be paid for continuously.

No doubt, in the majority of cases, those who have advanced financial support for scientific work would have preferred that such work should prove of direct and immediate practical benefit, but for a long time, up to the end of the nineteenth century, no method had been evolved of keeping the work of scientists within narrow bounds of exclusive usefulness to their particular patrons. Scientists, unfortunately for the more avaricious patrons, have proved themselves to be capable of scientific achievements only in the proportion in which they were devoted more to science as such than to the special desires and ambitions of their patrons. Furthermore, while various applied sciences have developed in increasing numbers, their development did not and could not insulate them from pure sciences. The belief, at first held, that applied sciences gave results of practical benefit, while pure sciences did not, proved to be unfounded. All that could be said definitely was

that while applied sciences gave results immediately beneficial in certain restricted fields, pure science gave results whose practical benefits came somewhat later and over a wider field, after these results had been absorbed and digested by various applied sciences. For all these reasons science and scientific research have found, from the seventeenth century onwards, an increasing, though not unconditional, support in a society based upon "private enterprise". We shall consider the nature and effect of the conditions attached to such support later.

The amazing development of science during this period has been due almost entirely to the successes achieved in scientific research. These successes of scientific research, though broadly speaking due to the favourable attitude of society (favourable, that is to say, in comparison with that of the Middle Ages or of Ancient Greece and Rome), could be ascribed on a more detailed examination to operation of eight distinct factors, each of which was itself associated with social development.

These eight factors, which are given below in an order which is not to be regarded as the order of their importance, are:

1. Further developments in general methods of conducting scientific research.
2. Increasing sum total of information in various fields of science, making possible the use of various forms of investigation which may be broadly described as "statistical", or "classifying".
3. Development and application to research of new and highly important devices, such as the microscope and the spectroscope.
4. Development of the science of mathematics.
5. Increasing general availability of new materials, such as refractories, rubber, various hitherto unavailable metals, alloys and chemicals.
6. Increasing general availability of certain services and facilities, such as gas, electricity, and means of providing low temperatures.
7. Interaction of various sciences, and the occasional creation of new sciences as a result of such interaction.
8. The impact of new philosophies.

It is extremely difficult to say, having regard to the vast number of scientists whose work during this period was of the highest order, where discovery by application of already established methods of research ceases, and modification of methods of research begins. The difference is probably most easily evident in the development of the science of mathematics, examined not from the point of view of that science but from the point of view of its effects on methods of research in other fields. In mathematics the greatest contributions to methods of scientific research were made by Napier's logarithms; by Descartes and Fermat in developing co-ordinate geometry; by Fermat and Pascal in developing the theory of probability; by Leibniz's infinitesimal calculus; by Fourier in the development of his series and in establishing that all derived quantities must have dimensions which may be expressed in terms of units of space, mass and time; by Lagrange's differential equations; by the theory of errors developed by Gauss; by Hamilton's differential equations; and by Riemann's geometry of non-Euclidean space.

When we pass from mathematics to other branches of science, the task of singling out scientists who introduced important modifications into methods of research becomes much more difficult. In physics, for example, with such scientists as Snell, Torricelli, Boyle, Huygens, Newton, Hooke, Black, Michell, Cavendish, Avogadro, Gay-Lussac, Franklin, Coulomb, Volta, Gauss, Joule, Oersted, Seebeck, Ampère, Ohm, Wallaston, Erman, Bernouilli, Rumford, Carnot, Arago, Fresnel, Young, Weber, Rowland, Faraday, Herschel, Ritter, Clausius, Graham, Kohlrausch, Maxwell, Boltzmann, von Bunsen, Melloni, Stefan, Kelvin, Stokes, Dewar, Ramsay, Foucault and Röntgen, one is tempted to say that each and every one of these has contributed to methods of scientific research. If, however, one confines one's choice to these who either adopted new methods in their own investigations, or made contributions to science which resulted in a new technique of research becoming available to subsequent investigators, one may hazard a limited choice.

While the contributions of the physicists of this period were numerous, the ones which probably resulted in the most important changes of research methods of subsequent investigators were those of von Guericke and Torricelli, initiating the

vacuum technique, Michell's torsion balance, Volta's electric pile, Oersted's discovery of the magnetic effects of electric current, Faraday's discovery of magnetic induction, Seebeck's discovery of thermo-electric effect, the work of Arago and Fresnel on polarised light, Herschel's discovery of infra-red, and Ritter's discovery of ultra-violet radiation, Joule's transformation of energy, Carnot's cycle, Graham's discovery of colloids, Dewar's work on low temperatures, and Röntgen's discovery of X-rays.

Deferring for the moment consideration of new methods of scientific research adopted by several great physicists of the period in conducting their own investigations, and passing to contributions in chemistry during these three centuries which exerted the maximum influence upon the technique of research of subsequent investigators, one may, as in the case of physics, hazard a limited choice. In chemistry, the contributions which probably exerted the greatest influence upon subsequent research technique were the classification by Sylvius of chemicals into acids, alkalis, and salts produced by the union of these two; Boyle's methods of qualitative analysis, use of litmus, and definition of elements; Mayow's introduction of weighing as an essential technique; Lavoisier's introduction of quantitative methods of investigation, his principle of the conservation of mass, and his discovery of isomerism of carbon; Scheele's discovery of the photographic action of light on nitrate of silver, the law of constant proportions established by Proust and by Richter, Wenzel's study of rates of reaction, the introduction by Berzelius of letter symbols to represent relative mass of elements corresponding to their atomic weights, Nicholson's and Carlisle's discovery of the electrolytic effect, Davy's recognition of importance of chemical contamination, spectrum analysis introduced by von Bunsen and Kirchhoff, Thomsen's application of principle of conservation of energy to chemistry, Avogadro's number, Williamson's discovery of dynamic equilibrium, Kirchhoff's discovery of catalytic effect, Wöhler's synthesis of urea, Gibbs's phase rule, Kekulé's constitutional formulae, and Liebig's and Wöhler's work on radicles. This, as in the case of preceding résumé of achievements in physics, does not include the work of scientists who adopted new methods of scientific research in their own investigations.

In consideration of scientists who, in their own investigations, introduced new fundamental modifications into principles of scientific research, it is preferable to consider simultaneously the work both of physicists and of chemists, partly because these two sciences are closely related, partly because many of the scientists of this small group of initiators of new principles in research were concerned both with physics and with chemistry.

The first of this small, select group was Newton. He introduced into scientific research the principle of the use of mathematics strictly as an instrument of investigation. While he did not rely on this instrument to the exclusion of experiment, and indeed performed very many experiments, his comparative lack of success in chemistry shows that he had a far greater mastery of the instrument of mathematics than of the experimental method.

The possibilities of mathematics as an instrument of research were even more convincingly demonstrated by Clerk Maxwell, whose amazing achievements were solely due to this method of approach and to the fact that he combined a mastery of mathematics with the keenest awareness of the natural phenomena, to the investigations of which his mathematics were applied.

In contrast to these two great scientists, Faraday relied exclusively on experiment, and his contribution to the methods of scientific research is his brilliant proof that the greatest discoveries are possible through the use of this method, without mathematical aid, without elaborate apparatus, without accumulation of endless observations, if the scientists adopting this method combine the qualities of daring imagination, acuteness of observation, clarity of thought and a power of simplification. Today it has become fashionable among professional scientists of a certain type to minimise the value of Faraday's methods of research, while admitting (as everyone must) the merits of his discoveries. Faraday, say such critics, was only able to achieve so much by his methods because he was exploring virgin soil. Today, they say, such methods would be unsuccessful because science has progressed too far, complex apparatus and complex mathematics have become inseparable from all important discoveries, and no virgin soil remains for

another Faraday to explore. Faradays, they say, will never again be born because the time for them is past. It is, of course, true that Faradays are not born frequently—as it is no doubt true that not one of the modern critics of his methods has a spark of his divine fire. But the universe is too great to have no virgin soil left to explore—and the soil still virgin will never fail to be at hand for those fit to follow in Faraday's footsteps.

A different kind of contribution to methods of research was made by Henry Cavendish. This was the demonstration, for the first time, of the possibility of discoveries of the highest importance being made through experiment, not because of ingenuity in planning, brilliance in execution, or imagination and profundity in interpretation, but entirely through care to achieve the highest possible degree of accuracy. This new principle in methods of research was established by Cavendish in his experiments on the composition of air, when he was able to say with complete conviction that the $\frac{1}{120}$th part of the air which refused to combine with alkali on sparking was not an experimental error, but a new gas of unknown properties. A century later this unknown gas was identified as argon. This principle of accuracy in experiment was destined to bring rich rewards, which have been most spectacular in the twentieth century, during which discoveries in radio activity, radiation, and many other phenomena, may be largely attributed to its use in the hands of gifted men.

An entirely different type of contribution to methods of scientific research was made by Mendeléeff by his Periodic Table of elements. This was the principle of possibility of discovery through classification. Newlands, in his discovery of the Law of Octaves, which was promptly rejected with foolish mirth by the well-established professional scientists to whom he presented it, largely anticipated Mendeléeff's discovery of the periodicity of elements. But Newlands did not have Mendeléeff's supreme confidence in the power of classification as a technique of research. It was Mendeléeff's faith that classification could be more than a mere method of tidily arranging previously known facts; that it could give positive, new knowledge, and new understanding, of phenomena; that not to see this was blind prejudice; which enabled him to establish his Periodic Table and the fame to which he is so justly entitled.

One more great scientist of the period, and one of the noblest of men, who made two great contributions to the methods of scientific research, was Pasteur. Pasteur belongs both to chemistry and to biology, and while his fame rests more upon his work in the latter field, he may equally fairly be included among the greatest chemists. His first great contribution to methods of scientific research was in micro-manipulation, by which he separated the dextra- and laevo-rotary racemate crystals. This method has since been developed to a degree when it has become of the greatest importance, particularly in biology. His second, and greater contribution to methods of research, was his triumphant establishment of the importance of persistent seeking of experimental confirmation of an hypothesis which appears sound, in spite of repeated negative results. In this he did not merely emphasise the virtues of courage and persistence. He established the importance of an hypothesis as a guide to action.

During the two thousand years intervening between Pythagoras and Pasteur, the conceptions of the nature and purpose of hypothesis and experiment and of their relationship had undergone many changes. To Pythagoras the hypothesis was perfect, and the function of the experiment was a demonstration of this perfection rather than a test of validity of a tentative theory. Later, the hypothesis, if blessed by the support of theology or authority, was regarded as not only perfect, but sufficient also for purposes of knowledge, and experiment was entirely rejected. Still later the pendulum swung to the other, though to a scientist far more tolerable, extreme, and Francis Bacon virtually denied all merit to initial hypothesis, and argued in favour of unrestricted observations and experiments as the starting point of any investigation. In the twelfth century de Maharn-Curia had conceptions of the nature and purpose of hypothesis and experiment and of their interrelation, which were not merely in advance of his time but which were not generally accepted in Pasteur's time, and which even in the twentieth century have not been properly grasped by many scientific investigators. But Pasteur was the first to prove that not only was it necessary to conduct experiments according to a plan based on an hypothesis, but that no amount of experimental failures could deprive an hypothesis of its value

as a guide to action—an hypothesis could be overthrown only by positive experimental evidence of its invalidity.

During these three centuries there were enormous developments in what may be broadly described as biological sciences. Here, as in the case of physics and chemistry, one may, leaving aside for the moment consideration of scientists, who introduced new principles of research into their own investigations, hazard a choice of contributions which exerted the greatest influence upon the research technique of subsequent investigators. Probably the most important of such contributions were Harvey's experiments on artificial insemination, the classification system of Linnaeus, Malpighi's microscopic examination of tissues, the chemical approach to physiology of Sylvius, Redi's and, still more conclusively, Spallanzani's evidence of non-existence of spontaneous generation, Gall's work on the brain and the nervous system, the work of Helmholtz, of Müller and of Weber and Fechner on sensations, Bell's distinction between sensory and motor nerves, Marshall Hall's distinction between volitional and reflex actions, Claude Bernard's discoveries of bodily functions, the Brownian movement, Schwann's discovery that processes of ferment and of putrefication were due to living organism, Schleiden's proof of development of plant tissues from division of single nuclear cell, Pasteur's discoveries on germs, Büchner's proof that fermentation could be produced by an enzyme extracted from yeast cells, Metschnikoff's discovery of phagocytes, von Benenden's observations of reduction division of chromosomes, Löffler and Frosch's discovery of ultra-microscopic filter-passing viruses, discovery of nitrogen fixation by bacteria made by Hellriegel and Wilfarth and by Vinogradsky, Darwin's theory of evolution (independently developed by Wallace), Mendel's laws of inheritance, Lloyd Morgan's theory of conditional reflexes, and Landsteiner's discovery of the blood groups.

Contributions of the highest importance to principles of scientific research were made by a number of scientists working in this field in their application of new technique in their own investigations. The most important of these were those due to Helmholtz, Müller, Weber and Fechner, Darwin, Mendel, Quetelet and Galton.

Helmholtz, Müller, Weber and Fechner investigated the sense perceptions as functions both of the stimulus and of the observer. Their researches involved development of a technique which would not only differentiate between the effects characteristic of the observer and those characteristic of the stimulus, but also show the inter-relationship between the two. This work not only added new knowledge and understanding of the highest importance to science, but established a new principle of scientific research. It established both the necessity and the possibility, in scientific observations, all of which can only be made through the medium of sense perceptions, of translating the sense perceptions, characteristic both of the stimulus and of the observer, into stimulus, characteristic only of the observed phenomena.

The work of Darwin on evolution, of epoch-making importance not only in biology but in the philosophic outlook also, involved the application of another new principle in scientific research—that of value of accumulated mass of systematised information as a means of establishing the validity of a new hypothesis beyond the powers of demolition by hostile criticism. The difference between the theory of evolution due to Darwin and that due to Wallace was not in the theories themselves, but in the mass of systematised supporting evidence. The mass of evidence offered by Darwin represented the results of twenty years' hard work: this made it impossible for intelligent critics of the period to dispute its validity with any hope of success.

Darwin's method has the elements both of weakness and of strength. Its weakness resides in its insatiable demands on the time and labour of anyone who adopts it. It compelled Darwin to devote twenty years to a discovery for which Wallace found a few months sufficient. It imposes a very great limitation on the number of discoveries which even the most gifted and indefatigable scientist may hope to achieve in the whole of his active life, and will not add much to the contents which his discoveries would have had if he had achieved them by other methods in one-twentieth, or even one-fourtieth, of the time.

The strength of Darwin's method rests in being the only method which robs the critics of new theories and discoveries

of their power. Critics of new scientific discoveries fall roughly into two groups—those who have alternative new theories and those who may be described as well-established opponents of rapid scientific progress. The first of these are frequently amenable to new evidence even when it is hostile to their previous theories, while the second are generally dumbfounded by it. Established authorities of the kind who derided Newlands, and would have accorded a similar treatment to Wallace if they could, have only one technique—to the genius of the discoverer they oppose authority, to the new knowledge and understanding offered by the discovery they oppose learning. Their technique of criticism becomes impossible when they are faced by a discoverer who, by his mass of systematised evidence, shows that he is a greater authority than they and has more learning than they can claim. Their ignorance is revealed, and they have nothing left but their wits with which to provide an adequate opposition. Thrown back upon their wits, they naturally can but lapse into a sulky silence. The strength of Darwin's method is therefore its ability of ensuring that a scientist's discovery, however revolutionary, is accepted, at least by the most progressive scientific elements, in his own lifetime.

A scientist wishing to adopt Darwin's method must ask himself whether he would sooner secure the acceptance of one or two discoveries in his lifetime, or make many more discoveries, but have them rejected and perhaps rediscovered by others many years later. Only the greatest discoveries justify Darwin's method. It is the refuge of many pedestrian scientists who amass a vast quantity of data with no conclusion at their end. But in the case of really great discoveries it is justified because it is the discoverer's sure shield against well-established stupidity.

Mendel, in his experiments on inheritance, introduced another new method of scientific research—that of combined classification and statistics. His experiments had all the simplicity which a genius for simplification can impart. This has led some of his twentieth-century detractors to say that the experiments were in fact so simple that anyone could have planned them after a little thought, and that his fame was undeserved.

Such an attitude towards Mendel's experiments is confined to critics who have not themselves exhibited any marked experimental ability, and, since Mendel has been dead too long to make envy a reasonable explanation, is no doubt due to entire absence of comprehension on their part as to the essentials of classificatory-statistical method. Such a method can give results of highest importance only in the hands of an experimentalist possessing great powers of simplification; any complexity in the pattern of investigation must lead to an undecipherable result. But to experimentalists with a real gift of seeing their problems clearly and simply, and of devising a simple pattern of experiments, this method can give a ready access to new knowledge and understanding of complex phenomena.

Quetelet, who as an astronomer was familiar with statistics, took the revolutionary step of applying statistics to anthropology. This step represented an introduction into biological science of a new and powerful instrument of investigation which, in astronomy, had not at the time yielded any highly important results. Since Quetelet the statistical method has been modified, improved, and extended into many fields of investigation. Its essence, however, has remained unaltered. Its strength is not confined to its ability to give new knowledge and understanding as a result of mere repetition of observations of similar phenomena: it derives its strength from the pattern of nature itself, because nature's laws, whether these be the laws of behaviour of gases or the laws of behaviour of classes in a human society, are the laws of behaviour of the sum of constituent units.

One more, highly important, addition to methods of scientific research was made by Galton, in the field of experimental psychology. He performed a number of experiments in which he observed his own psychological reactions under various controlled conditions. One of these was an experiment in which he deliberately treated breathing as a volitional act for a long interval of time, and then noted the effect of sudden abandonment of volitional control. Another experiment was his study of psychology of the religious reverence which savages feel for idols they know to be man-made; this experiment consisted in paying a daily homage for a long time to a figure

of Punch and noting how gradually this habit engendered a feeling of respect and finally reverence for an object which intelligence pronounced to be quite undeserving of such feelings. The experiments themselves were far from perfect in the light of present-day knowledge of psychological processes, but from the point of view of scientific research they were of extreme interest, because they represented the creation of a new method of investigation. Hitherto, it had been assumed that the only kind of observations of scientific value in any experimentation were those in which the objective element was dominant, and any subjective element unavoidably present could be eliminated by appropriate corrections. True, long before Galton's time there had been numerous experiments, such as investigations of effects of various chemicals upon human beings, in which investigators made experiments upon themselves: but such experiments were performed not because of, but in spite of, the presence of the subjective element, and, wherever possible, were replaced by experiments on animals. Galton showed for the first time the possibility of experimental investigation, in which the subjective element in observation was a means of acquiring new knowledge and understanding of phenomena which were not amenable to investigation through objective observation.

These three centuries witnessed developments of the highest importance in other pure sciences, such as geology and astronomy, and even more impressive developments in the applied sciences, such as engineering and industrial chemistry. The temptation to deal with them in detail has had to be resisted, since to do so would increase the historical part of this book to excessive proportions. Reluctantly, therefore, this chapter must be closed with a few brief comments of a more general nature.

It has already been mentioned that much of the scientific progress during this period was due to interaction of various sciences. Frequently the importance of a discovery, a technique, or an instrument, originating in one branch of science, had its greatest effect not in development of that particular branch of science, but of some totally different branch. The spectroscope, the development of which was due to the physicists' study of light, gained immensely in usefulness as an aid to

scientific investigations with the introduction of spectrum analysis by von Bunsen and Kirchhoff, and ultimately revealed its maximum potentialities not in chemistry, but in astronomy, after it was found to give the composition of the sun and stars, after Secchi had used it for classification of stars and Pickering detected double stars with its aid. Graham's discovery of the colloids proved to be of maximum value, not in physics, but in biology; Brown's discovery of a type of movement of particles which bears his name did not prove useful to botanical investigations, in the course of which it was discovered, but became of great interest to physicists; Niels Stensen's proposal of study of fossils not so much for themselves as a guide to the history of the Earth, was of the highest importance.

A little more may be said about the influence of society upon scientists, and its treatment of their discoveries. This subject has already been touched upon, but it may be added that during these three centuries the powers of a feudal, or semi-feudal, society to check scientific progress were exhibited with particular vividness in China and Russia. During these three centuries China exerted virtually no influence on the progress of world science. Yet such important discoveries as those of paper, the magnetic needle, and gunpowder, had been made and put to use in China before Europe had any ideas about these matters. In Russia, from the seventeenth to the nineteenth century, only Mendeléeff and Metschnikoff made contributions which had a profound effect on world science. Yet there is evidence that during that period there were many other Russian scientists of the first rank. There was Lomonosov, an amazingly versatile genius who was a chemist, physicist, astronomer and mineralogist as well as a master of several applied sciences, and who, among his other achievements, had anticipated much of Lavoisier's work and was the first to establish the presence of an atmosphere on Venus. There was Koolibin, who in 1782 constructed mechanically-propelled carriages and boats. There was Foorvin, who made a balloon ascent in 1731, and was forced by the Church to abandon his work. There was Popov, who anticipated Marconi's principal discoveries. All this work was lost to world science: as far as the progress of science is concerned, these men might have never lived. Feudal Russia had obliterated the efforts of its

discoverers, and semi-feudal Russia frustrated them by its indifference and incompetence.

One more thing must be said before this chapter closes. In the outline of history of scientific research which has here been presented an attempt has been made to show the growth and development of scientific research, and to pay tribute to those to whom such growth and development were principally due. The result has been the presentation of a picture of scientific progress due entirely to the individual achievements of a succession of scientific geniuses—a picture not unlike that given by Carlyle of human history as the consequence of action of a limited number of very great men. It must be said at once that such a picture is highly artificial. No scientist, however great, has ever been free of indebtedness to the work and thoughts of other men. Darwin owned his debt to Lamarck and to Malthus; Leonardo da Vinci his to Archimedes. Even Galileo owed a debt to Johannes Philoponus, who in the first half of the sixth century maintained that bodies acquired their motion from an external force, and continued their motion by virtue of impetus thus received; that all bodies did not endeavour to move to the centre of the earth but to unite into a single aggregate; that the difference in the rate of fall of various bodies was due to air resistance, and that in an empty space all bodies would fall at the same rate. The truth, as Millikan has said, is that "a science, like a plant, grows in the main by a process of infinitesimal accretions".

CHAPTER III

RESEARCH AND PHILOSOPHY

THE progress of science, both pure and applied, was far more rapid during the nineteenth century than during the two preceding centuries. It might be said that if the scientific achievements during these three centuries could be expressed as mathematical terms corresponding to successive equal time intervals, such terms would constitute a diverging geometrical series. The analogy is imperfect, because the discoveries made were of unequal importance, and because delay of some highly important discoveries would have produced at least a temporary convergence of the series, while at the close of the nineteenth century a number of discoveries were made which ushered in a veritable revolution in science. These were Röntgen's discovery of X-rays, J. J. Thomson's determination of the mass and charge of the electron, Wilson's cloud chamber, Becquerel's discovery of radioactivity of uranium, and the discovery of radium by M. and Mme. Curie: they produced a revolution not only in physics, but in chemistry and astronomy also. During the three preceding centuries these branches of science were based upon certain fundamental conceptions, one of which, probably the most important one, was the conception of matter as being composed of indestructible and indivisible atoms. The atomic concept, originating with Democritus, was accepted by Boyle and Newton and supported and developed by Dalton, Berzelius, Avogadro, Cannizzaro, Mendeléeff and many other great scientists. Mendeléeff's periodic table and the increasing use of the spectroscope in astronomy created a picture of the inorganic world as essentially a stable, orderly and familiar entity, where even the undiscovered elements could be predicted, where the same familiar laws held true always and everywhere, where only infinity of space and time were overwhelming to the mind. Even the confidently predicted death of the sun was accepted with philosophic sadness.

Suddenly all was changed. J. J. Thomson's discovery of the electron and the Curies' discovery of radium shattered the old conceptions. The atom was no longer a simple, indivisible, unchanging unit of the Universe.

The remarkable developments in physics, chemistry and astro-physics which followed have, according to some writers, made the world less understandable. Such a view is quite unjustified. Undoubtedly the present day scientific concepts of the universe are more difficult to grasp than the scientific concepts of the nineteenth century appeared to be, just as the nineteenth century scientific concepts of the universe were more difficult to grasp than the ideas propounded by Aristotle. But the fact that increased knowledge shows certain previously held ideas to be untenable, cannot mean that such increase of knowledge produces a decrease of understanding—even if satisfactory new explanations are not yet available. The term "understanding" cannot be attached to erroneous beliefs, however comforting. On the other hand, it does not in the least follow that because new knowledge has made old concepts untenable, the new interpretations must necessarily be correct. On the contrary, quite apart from the fact that a complete, perfect and final understanding of phenomena is unattainable to science except at the end of infinity of time and effort, the mere fact that the new theories have emerged as a consequence of opening up of entirely new vast fields of knowledge by revolutionary discoveries makes it exceedingly probable that at least some of the new theories are destined to be superseded by better ones in the not too distant future. Indeed, the birth and death of several theories of atomic structure bear testimony to the impermanency of some of the new interpretations.

It is quite likely that the reader of this book is, or will be, engaged on some piece of research connected with the physical, chemical or astro-physical discoveries and theories of the twentieth century. Some consideration of these discoveries and theories from the point of view of principles of scientific research is therefore not only justified, but essential.

When we look at the discoveries and theories of twentieth century science, confining ourselves to those whose starting points were located in the field of inorganic phenomena, we may discern a certain broad pattern.

Röntgen's discovery of X-rays, apart from stimulating the study of electrical phenomena in gases, led to Moseley's Law of Atomic Numbers, to Laue's and Braggs's determinations of crystal structure, to measurement of interatomic distances in liquids and gases by Debye and by Wierl, to stereochemistry (which originated in the nineteenth century with Van't Hoff's and Le Bel's theory of tetrahedral carbon atom, but needed X-ray analysis for its development), to study of the structure of fibreous substances such as cellulose, keratin and myosin, to development of a highly important technique in medicine, and to Müller's technique of production of mutations. Here the initial achievement—the discovery of X-rays—was a fortunate accident, an experimental result without preliminary hypothesis. Its fruits have been great discoveries primarily based on acquisition of new knowledge through experiments. With the solitary exception of the Compton Effect, to which we shall return, these discoveries did not play any important part in the conflict of two philosophies which have emerged from the discoveries of the twentieth century.

Becquerel's discovery of radiation emitted by uranium, and the discovery of radium by M. and Mme. Curie, led to Rutherford's work on radioactivity, to his discovery of the proton, to his proof of transmutation of elements and his theory of atomic structure, to the discovery of the neutron by Joliot-Curie and by Chadwick, to atomic disintegration by beams of artificially produced protons by Cockcroft and Walton, atomic transformation by bombardment with deuterons by Lewis, Livingstone and Lawrence, atomic transformation by neutron bombardment by Feather and Harkins, discovery of the hydrogen and helium isotopes with mass number 3, the Joliot-Curie production of artificial radioactivity by α-ray bombardment, Fermi's production of artificial radioactive elements by neutron bombardment, Lawrence's cyclotron, Kerst and Serber's betatron, Hahn and Meitner's discovery of fission of uranium nucleus, liberation of atomic energy, the use of radium in medicine and the use of radioactive isotopes of common elements in biology as a means of studying various processes within living organisms. Here the initial achievement—Becquerel's discovery—was an experimental result which was not unexpected, but was not a consequence of an attempt at

confirmation of any brilliant theory. The fruits were great discoveries, exceeding in importance the discoveries relating to X-rays, based primarily upon experiment, but also involving development of new theories and having an important bearing on two conflicting philosophies.

J. J. Thomson's determination of the mass and electric charge of an electron was an achievement of an order entirely different from both Röntgen's discovery of X-ray and to Becquerel's discovery of uranium radiation. The epoch-making experimental investigations carried out by Thomson in 1897 were not merely carefully planned experiments designed to verify an hypothesis. They involved the application of a new, highly important principle in scientific research—the choice between two alternative hypotheses. The investigation was designed to establish the nature of cathode rays, which, according to one hypothesis, were corpuscles, and, according to another hypothesis, waves in the aether. Thomson decided that the experiments should be designed to test the corpuscle hypothesis, because the aether hypothesis could not be satisfactorily tested, owing to the absence of adequate information about the properties of aether. The principle of choice between alternative hypotheses solely on the grounds of their suitability for verification is of the utmost importance, and cannot be neglected by anyone now engaged in scientific research, for hypotheses to-day tend to be more plentiful than in the nineteenth century, and some of them can only lead to profitless speculation.

Thomson's proof of the existence of the electron corpuscle and measurement of its mass and electric charge led to his discovery of isotopes, followed by brilliant work on these by Aston, to Thomson's and to Lenard's establishment of the nature of the photoelectric effect discovered by Elster and Geitel, to Millikan's oil drop experiments, to Davisson and Germer's electron diffraction, to G. P. Thomson's electron diffraction experiment, to Millikan's work on cosmic rays, discovered by Göckel and confirmed by Hess and Kohlhörster, to Anderson's discovery of the positron, and to the discovery of the meson by Williams and Pickup and by Nishina. While these discoveries were achieved, like those in the field of radioactivity, through experimentation, it can be said that here the relative importance

of the hypothesis was much greater than in researches on radioactivity. It is these discoveries which, more than anything else, determine the victory of one of the two new rival philosophies over the other.

The two new rival philosophies to which several references have already been made emerge from the quantum theory. Before dealing with the quantum theory and the philosophies in question, it is probably best to deal with theories of atomic structure, although these involve quantum considerations. A number of theories of atomic structure were developed, each designed to explain the experimentally established data relating to atom behaviour. Both static and dynamic atom models have been proposed, but the static atoms of Kossel and of Lewis and Langmuir have been finally rejected as incompatible with experimentally established facts. The dynamic atom model first proposed by Nagaoka, formulated by Rutherford, and developed to a high degree of perfection by Bohr, had to undergo a number of changes. It has imparted a great impetus to the development of chemistry through Sidgwick's theory of valency and the work of Wrede, Debye and Bowen. Its influence was perhaps most important in astro-physics. Eddington's theory of stellar interiors, Kothari's estimate of atom-crushing pressure, Chandrasekhar's and Kothari's relationship between mass and size of stellar bodies, Fowler's work on white dwarf stars, the work of Gamow, Gurney, Condon, Atkinson, Houtermans, Weizsäcker, Bethe and Teller on thermonuclear reactions as sources of solar and stellar energy, Landau's nuclear state of matter, Zwicky's work on supernovae, were bound up with the development of theories of atomic structure.

We now come to the greatest scientific achievements of the twentieth century—Planck's quantum theory and the work of Einstein. Both Planck and Einstein used the modern equivalent of Maxwell's method of scientific research—the use of mathematics as an instrument of investigation. It is because the quantum theory and relativity have been responsible for the birth of two new rival philosophies concerning the nature of the Universe, that it is desirable to give the views of these two of the greatest scientists of all time in their own words.

Planck, in his *Universe in the Light of Modern Physics*[1] says:

"Physics is an exact Science and hence depends upon measurement, while all measurement itself requires sense-perception. Consequently all the ideas employed in Physics are derived from the world of sense-perception. It follows from this that the laws of Physics ultimately refer to events in the world of the senses; and in view of this fact many scientists and philosophers tend to the belief that at bottom Physics is concerned exclusively with this particular world. What they have in mind, of course, is the world of man's senses. On this view, for example, what is called an 'Object' in ordinary parlance is, when regarded from the standpoint of Physics, simply a combination of different sense-data localised in one place. It is worth pointing out that this view cannot be refuted by logic, since logic itself is unable to lead us beyond the confines of our own senses; it cannot even compel one to admit the independent existence of others outside oneself.

In Physics, however, as in every other science, common sense alone is not supreme; there must also be a place for Reason. Further, the mere absence of logical contradiction does not necessarily imply that everything is reasonable. Now reason tells us that if we turn our back upon a so-called object and cease to attend to it, the object still continues to exist. Reason tells us further that both the individual man and mankind as a whole, together with the entire world which we apprehend through our senses, is no more than a tiny fragment in the vastness of Nature, whose laws are in no way affected by any human brain. On the contrary, they existed long before there was any life on earth, and will continue to exist long after the last physicist has perished.

It is considerations of this kind, and not any logical argument, that compel us to assume the existence of another world of reality behind the world of the senses; a world which has existence independent of man, and which can only be perceived indirectly through the medium of the world of the senses, and by means of certain symbols which our senses allow us to apprehend. It is as though we were compelled to contemplate a certain object in which we are interested

[1] Published in Leipzig by Messrs. Johann Ambrosius Barth, and here included by kind permission of Messrs. Allen and Unwin Ltd., the publishers of the English edition.

through spectacles of whose optical properties we were entirely ignorant.

If the reader experiences difficulty in following this argument, and finds himself unable to accept the idea of a real world which at the same time is expressly asserted to lie beyond our senses, we might point out that there is a vast difference between a physical theory complete in every detail, and the construction of such a theory. In the former case the content of the theory can be analysed exactly, so that it is possible to prove at every point that the notions which we apply to the world of sense are adequate to the formulation of this theory; in the latter case we must develop a theory from a number of individual measurements. The second problem is very much more difficult, while the history of Physics shows that whenever it has been solved, this has been done on the assumption of a real world independent of our senses; and it seems reasonably certain that this will continue to be the case in the future."

Einstein says in his *Evolution of Physics*:

"The formulation of a problem is often more essential than its solution, which may be merely a matter of mathematical or experimental skill.

"Physical concepts are free creations of the human mind, and are not, however it may seem, uniquely determined by the external world."

In reply to an imaginary advocate of the classical theory of physics he says, "My mathematical tool is more complicated than yours, but my physical assumptions are simpler and more natural."

"Physical theories try to form a picture of reality and to establish its connection with the wide world of sense impressions. Thus the only justification for our mental structures is whether and in what way our theories form such a link."

"With the help of physical theories we try to find our way through the maze of observed facts, to order and understand the world of our sense perceptions. We want the observed facts to follow logically from our concept of reality. Without the belief that it is possible to grasp the reality with our theoretical constructions, without the belief in the inner harmony of our world, there could be no science."

These quotations show unmistakably that both Planck and Einstein were guided in the formulation of their theories by a belief in the existence of a real, knowable and understandable world, not a world of mathematical abstractions which can only be comprehended as corresponding to a number of equations.

The quantum theory of Planck was followed by Einstein's photon theory, by de Broglie's theory of matter waves, and by the wave mechanics of Heisenberg, of Schrödinger and of Born. From these emerge the two conflicting philosophies concerning the nature of the Universe.

According to Schrödinger's theory, corpuscles, and in particular electrons, are wave packets. The contradiction between the particle and the wave theories is resolved by the denial of material existence of the particle. But what of the medium through which the waves are propagated? The medium can only be the aether whose non-existence Einstein established. Yet Schrödinger's equations are in excellent agreement with observations. If this view is adopted, the material waves of de Broglie are waves of nothing in nothing. Add the Heisenberg principle of indeterminancy as a proof that the position and momentum of an electron are indefinite, and strict causality has disappeared. Add also Lecomte du Noüy's theory of time quantum. Now the whole Universe has disappeared into a modern version of the world of unreality of medieval Muslim theology. And it has done so apparently as a consequence of unimpeachable mathematics supported by experimental evidence. One need not ask where to go from this point, for there is clearly nowhere to go.

There is, however, an alternative philosophy of the Universe. The conflict of the particle and wave theory can be settled on the basis of higher synthesis. It is not a question of acceptance of *either* the particle *or* the wave, but of *both* the particle *and* the wave. J. J. Thomson has shown that electrons are particles, while Davisson and Germer have shown that they behave like waves. While the wave theory of light has been established, the existence of Einstein's photon has been demonstrated by the Meyer and Gerlach experiment on photoelectric effect with small metal particles. Interference phenomena have been obtained with X-rays by Bragg and by Compton, but the corpusclar nature of X-rays has been demonstrated by the

Compton Effect. Stern obtained a diffraction effect with molecular rays of H_2 and He, in conformity with de Broglie's equation. All this seemingly contradictory evidence was synthesised into one harmonious whole by the interpretation that while individual molecules, electrons and photons are particles, crowds of such particles behave like waves because their space-time distribution is not chaotic, but is in accordance with the law of probability. Heisenberg's principle of indeterminacy means that we cannot simultaneously determine both the exact position and the exact velocity of an observed particle; but this does not mean that an unobserved particle cannot possess at any particular instant some definite position and some definite velocity. The indeterminacy is a mathematical expression of the observer's unavoidable errors, and an error is the property of the mode of observation and not of that which is observed. Lecomte du Noüy's biological time is subjective, not objective. For understanding of the nature of the Universe, and in particular for understanding its past history and probable future, subjective time is entirely unsuitable. It becomes meaningless when we consider past or future periods during which no living organisms could be present. Objective time is the only kind of time that can be used in the time-space continuum of modern physics, and objective time does not exhibit a structure of finite indivisible intervals.

This second philosophy gives us a real world, a world of amazing richness, of inner harmony without belief in which, as Einstein says, there can be no science.

There can be no doubt of the victory of the second philosophy over the first. But the conflict has not been valueless. From it emerges a new principle of scientific research which is contained in the words of Einstein quoted above and which may be summarised as follows:

Physical concepts are free creations of the human mind, and are not, however it may seem, uniquely determined by the external world. The formulation of a problem is often more important than its solution. Such a formulation must include the belief in the possibility of grasping reality and in the inner harmony of the world.

This principle emphasises still further the importance of the hypothesis as a guide to action, already stressed by Pasteur. No

brilliancy in mathematics, or skill in experiment, will give us the right answers, unless we ask the right questions. The hypothesis is thus once more invested with an importance almost equal to that given it by Pythagoras. The natural question is whether the wheel has travelled a full revolution. If it has, then we are on the verge of a breakdown of the scientific method, since if the choice of an hypothesis is the only true key to knowledge and understanding, and one's personal beliefs and desires are the determinants of such choice, scientific research must ultimately be enmeshed in the subjective element.

Fortunately the "inner harmony of the world" as understood by Einstein, is a very different thing from the inner harmony of the world of the post-Pythagorean philosophy of Ancient Greece. It is expressible in a scientific form. Einstein's relativity reveals this inner world harmony in two ways. One way in which he reveals this is by demonstrating the essential unity of apparently opposed entities, such as mass and energy, space and time. The other way is that apparently qualitative differences are due to differences in quantity, as in the case of matter and field.

Before proceeding further, a small digression is necessary. The author has been advised by his friends to omit the next few pages because they refer to scientific principles enunciated by two men whose names are now associated with bitter political controversies. Such an omission appears to him to be impermissible by standards of scientific honesty. If science, emancipating itself from theology, is to be enmeshed in political prejudices, then it is the duty of all honest scientists to combat such a disaster. The author believes that most of his readers will be with him in his attitude, but if there are any among them who are the spiritual descendants of the French patriots who sent Lavoisier to the guillotine with the cry "the Republic has no need of chemists", they are recommended to skip the next few pages and so avoid hurt to their tender political conscience at the expense of scientific knowledge.

The fundamental principles to which the author now proposes to refer without further preamble are those first enunciated in the nineteenth century by Marx and Engels and embodied in their Dialectic Materialism. Because Marx and Engels were the founders of modern Communism, numerous

supporters of Communism now regard these principles as not only necessary, but also sufficient guides to scientific research, while numerous opponents of Communism feel in honour bound to denounce or ignore the scientific discoveries of two such men. Both attitudes are absurd, and fortunately there is an increasing number of scientists of widely differing political views, or none at all, who take a more balanced view. One may hope that a time will come when Engels's *Dialectics of Nature* will be removed from political bookshops and put on the shelves of those devoted to scientific publications. Needless to say, the fundamental principles enunciated by Marx and Engels do not supersede all previously discovered principles of scientific research, or invalidate other principles discovered at a later date. They can in no way serve as exclusive guides to such research, any more than any great scientific discovery can give a complete knowledge and understanding of the whole Universe. But they are of the utmost importance.

Of the four fundamental principles embodied in Dialectic Materialism, the first is that everything in Nature has a history, nothing is eternal and immutable, and everything is in a continued state of change. This principle, although first enunciated in this all-embracing form by Marx and Engels, has been formulated in more limited forms long before them. It was first propounded by Heraclitus, and found expression in the works of Bacon, Descartes and Leibniz, in Niels Stensen's theory of the possibility of tracing the history of the Earth through fossils, in Kant's cosmogony, and in the evolution theory of Lamarck. In the twentieth century this principle has been extended by Lemaitre and Dirac, and particularly by Milne. Milne has developed a theory that the colour-shift in the light of distant stars is not due to their recession, but to a change of laws of the Universe with time. In a universe in which change is everywhere observed, the laws of nature themselves need not be immutable.

The other principles embodied in Dialectic Materialism, which, according to Marx and Engels, must be accepted by any scientist, consciously or unconsciously, in framing of a correct hypothesis or final induction, are Unity of Opposites, Change of Quantity into Quality, and Negation, and Negation of Negation. Marx and Engels did not suggest that the mere

acceptance of these principles would make an hypothesis or the final induction right, but they maintained that a contravention of these principles would certainly make an hypothesis or induction wrong.

These three principles may be best explained by modern illustrations. Up to a comparatively recent date there were two conflicting theories regarding the nature of light. According to one theory, light had a corpuscular nature; according to the other, which for a long time held the field, it was of a wave nature. According to the first theory, light was material and composed of small finite particles possessing mass and subject to gravity. According to the second theory, light was not composed of any finite particles, but was a continuous motion possessing energy but no weight. Evidence of a mathematical and experimental nature was brought in support of the two conflicting theories. According to the principle of unity of opposites, however, the only correct theory would be one which could unify the two conflicting theories. The present theory of light, which confirms the correctness of both the corpuscular and the wave concepts, and which explains why light has energy and is also influenced by gravity, is therefore a striking confirmation of the correctness of the "unity of opposites" principle.

The principle of transition of quantity into quality is perhaps most strikingly illustrated in the case of radioactivity, when the mere quantitative increase of U235, or of plutonium, above a certain critical point, produces suddenly a nuclear fission of the entire aggregate with liberation of enormous quantity of atomic energy.

The principle of negation and negation of negation is perhaps most impressively illustrated in astro-physics, by the supernova phenomena. According to the modern theory of stellar bodies, increase of mass beyond a certain point produces a decrease of volume. Increase of gravitational force for bodies heavier than Jupiter is sufficient to crush the atoms, and decrease of volume with increase of weight continues until for a star 1·4 times heavier than the sun the volume becomes zero. Increase of mass thus negates the volume that mass may occupy, and beyond a certain point uncontrollable contraction to a geometrical point sets in. According to the principle of

negation of negation, however, such a state of affairs cannot exist without creating an opposite tendency, a factor which tends to, and at a certain point actually must, negate the negation. A new force must come into play to stop the contraction of the stellar body to a mathematical point. This is what has actually been observed, when Zwicky discovered a number of supernovae—stars which explode with a brilliancy several billion times greater than that of the sun.

The primary value of the principles of unity of opposites, of change of quantity into quality, and of negation and negation of negation, from the point of view of scientific research, must reside in their value as guides to formulation of correct hypothesis. But it is by no means easy to apply them for that purpose. Engels himself was by no means infallible in his judgment of hypothesis, and his faith in correctness of Kant's cosmogony has not been justified by modern astro-physics. Modern scientists who have accepted the philosophy of dialectic materialism have frequently ignored other scientific principles with disastrous results. Some scientists, professing acceptance of dialectic materialism, have done their work entirely without regard to it, and have then stuck it on to the thesis like a false beard. Some of the greatest scientists have quite unconsciously conformed to these principles in their research, as has Einstein, because they were in accordance with their own mental processes. Other scientists, like Bragg, have made discoveries of the highest importance without any application of these principles. It may be said that, on the whole, the possible value of dialectic materialism as a guide to scientific research is almost unexplored.

This failure to explore the possibilities of dialectic materialism as an instrument of scientific research is only partly due to political prejudices. The author, having explored the possibilities of this technique over a number of years with a reasonable combination of hope and caution, believes that this failure is due, to a considerable extent, to the inherent nature of scientific reasoning. Successful scientific reasoning appears always to require as its starting point a consideration of available data relating to the specific problem, and not an attempt to apply to the particular problem general laws of the entire universe, however true and important such laws might be. A

scientist who attempts to start his research from the point that his initial hypothesis must include some or all of the four principles of dialectic materialist philosophy will either frame an unsatisfactory hypothesis, or will find that he cannot make any satisfactory progress, do his research in another way, and either decide that dialectic materialism is useless as an instrument of research or attach it irrelevantly to his final conclusions. The only way in which it seems possible for a scientific investigator to make use of principles of dialectic materialism as instruments of research, is for him to proceed with research without using them until he encounters certain effects which naturally suggest to him that he is witnessing a union of opposites, or a negation of negation, or a change of quantity into quality: he is then able to make use of this discovery in framing a better hypothesis and planning more decisive experiments for its verification.

Biological science of the twentieth century did not undergo the same startling revolution as physics, chemistry and astronomy (or to give it its modern name, astro-physics), but its progress has been so startling as to verge on a revolution.

It may be said that the most important developments in the field of twentieth-century biology were those relating to cell structure and metabolism, hormones, vitamins, viruses, immunity, embryology, genetics and psychology. Of these, all except vitamins may be regarded as having their origins in discoveries made about the end of the nineteenth century, the period which, as we have seen, was also the period of discoveries which led to a revolution in physics, chemistry and astrophysics.

The twentieth-century researches in cell structure and metabolism may be regarded as originating in the determination of coagulating power of uni-valent, di-valent and tri-valent ions by Linder and Picton in their study of colloids in 1895, and the invention of the ultra-microscope by Siedentopf and Zsigmondy in 1903. Subsequent developments include the work of Mines on effects of valency and of protective colloids in biology, Donnan's theory of membrane equilibria, Loeb's explanation of colloidal behaviour of proteins, Willstätter's work on chlorophyl, Wieland's work on oxidation of tissues, work on enzymes participating in the oxidation process carried out by Warburg,

Szent-Gyorgyi and Hopkins, Krebs's study of urea synthesis, the work of Meldrum and Roughton on the enzyme producing a rapid release of CO_2 from blood in the lungs, and energy transfer to cells by fermentation investigated by Hopkins, Fletcher, Meyerhof, Embden and Parsons.

The study of hormones originates with Takamine's isolation of adrenalin in 1901. This discovery was followed in 1902 by the work of Bayliss and Starling, and the whole subject was rapidly developed by Banting and Best, Kendall, Harington, Steinach, Allen and Doisy, Marrian, Kendall and Collip. Somewhat different, but closely related to work on hormones, was the work on neurocrines by Loewi and Navratil, by Dale and Dudley, and by Cannon. A branch of scientific investigation which was supposed to be entirely unrelated to hormones—the study of cancer—was brought into striking relationship with hormone investigations by Kennaway's work on cancer-producing substances.

The study of vitamins was a direct consequence of demonstration by Hopkins in 1912 that chemically pure food was inadequate for growth. The study of the subject has developed rapidly. The work of Karrer, Williams, Haworth, Euler, Wald and others has not only led to the discovery, isolation and synthesis of vitamins, but also to a conclusion that the vitamins so far discovered cannot be assumed to be more than a fraction of those actually existing. The study of vitamins occupies a unique position in the development of twentieth century biological sciences, in that, while other developments have encouraged a belief in man's continuous progress through increasing knowledge and mastery of his environment, discoveries of vitamins and of their effects upon the human system disturbed this complacency by revealing that many illnesses, like rickets, scurvy and night blindness, are due to deficiency of naturally plentiful substances which have been withheld in these particular cases merely by the behaviour of human society.

The work on viruses may be regarded as originating in the proof of their existence by Löffler and Frosch in 1893. Actually the tobacco-mosaic virus was discovered by Ivanovski one year earlier, but his discovery did not receive the attention it merited. The subject was investigated by Stanley, Gortner,

Laidlaw and Kenneth Smith. It is of particular interest because viruses appear to exhibit both the properties of crystalline organic compounds and of living organisms, which naturally raised the question as to whether they are a borderline case between living organisms and inanimate matter.

The study of immunity, which dates back to Jenner's experiments on vaccination and Pasteur's treatment of rabies, was given a great impetus at the close of the nineteenth century by two fundamental discoveries—the discovery of antitoxins by Behring and Kitasato in 1890, and the proof by Ehrlich in 1891 that injection of toxic proteins into an animal resulted in production of specific antitoxins. Thus the production of antibodies by the action of antigens was established. In the twentieth century the subject has been greatly developed, in particular by Landsteiner, Heidelberger, Avery, McCarty and Kendall.

All the above biological investigations and discoveries, which have been grouped under five classifications, have certain features in common. They all rely for their development upon a research technique in which methods of organic chemistry and the microscope play a leading rôle, and they all tend to express the phenomena of life in terms of bio-chemistry and of behaviour of living cells. This mode of interpretation has been shown to be inadequate by other biological sciences of greatest promise—genetics, embryology, and psychology.

While the honour of originating the science of genetics belongs to Mendel, it received its first great impetus from the proof by de Vries and Bateson of the existence of large discontinuous mutations, which must have stimulated the discoverers of Mendel's work in 1900. This was followed by Bateson and Punnett's discovery of sweet pea linkages and its application to chromosome theories by Lock, by Morgan's introduction of the use of *Drosophila* in the study of heredity, by the statistical investigations of Karl Pearson, by Winkler's production of artificial polyploids by heat treatment, by Beadle's work on neurospora, by Scott Moncrieff's work on flower genes, by Crew's work on sexuality, by Müller's use of X-rays to produce mutations, by Fisher's, Haldane's and Wright's statistical studies, and by chemical production of mutations by Auerbach and Robson. The embryology of the twentieth century may be

regarded as dating from the work of Roux and Driesch's experiments on eggs in 1895. It has been greatly developed by Spemann's microsurgery of newts, Vogt's study of dyed embryos, Needham's chemical embryology, and the work of Waddington and of Holtfreter.

It was work in this field which made interpretations of life in terms of bio-chemistry and of behaviour of the living cells, which could in their turn be reduced to terms of chemistry and of physics, inadequate. The first epoch-making discovery which proved this inadequacy was Driesch's result of his experiments on eggs in 1895, when he found that removal of large portions of the egg, and displacement, even removal, of some of the blastomeres, did not prevent the formation of a normal, though undersized, embryo. This and much subsequent work established the principle of Determination, according to which chemical substances in a very young organism do not act in a constant manner, and indeed not even in a manner which could be explained by any variant of the law of probability, but in accordance with a pattern characteristic of the species to which the organism belongs. For this no explanation has been advanced which could be described as a scientific hypothesis capable of experimental verification. The best interpretation which has been offered is that contained in Whitehead's philosophy of organic mechanism, and this has the nature not of a scientific hypothesis but of a mode of presentation of observed facts.

Before dealing with Whitehead's philosophy it is perhaps best to deal briefly with the developments of twentieth-century psychology, since suggestions have been made that the impact of Whitehead's philosophy is likely to affect its future development at least as much as the future development of genetics and embryology.

The first thing that strikes one on approaching the subject of twentieth-century psychology is that it is divided into three separate branches, whose only common contact is that they all proceed from the desire to investigate mental processes. The second is that these studies of mental processes are quite separate from the branch of physiology concerned with the brain and the nervous system. Such a separation of organs from functions associated with them is observed in no other branch of biological sciences, and no unsupported speculation as to the

nature of the association between brain and mind, or the relative importance of the two, can make the separation a satisfactory feature of the mode of investigation.

Study of the physiology of the brain and the nervous system received a great impetus in the nineteenth century from the work of Fritsch and Hitzig on the cortex, and the work of Gaskell and Langley on the involuntary nervous system; during the twentieth century much further work both on the brain and on the nervous system was carried out by Sherrington, Graham Brown and Head.

One twentieth-century branch of psychology originated in the nineteenth-century experiments on sensation and stimuli by Weber and by Fechner. This branch of psychology proceeded through development and use of special devices to determine quantitatively responsiveness to stimuli for vision, hearing, touch, taste and smell, and to devise tests for quantitative estimation of memory, association, co-ordination and so on. The experimental method was adopted by Pavlov in his study of conditional reflexes, which led to Watson's behaviourism psychology. The characteristic feature of this branch of psychology has been the method of objective observations and the use of instruments as aids to observation.

The second branch of psychology originated in Galton's methods of experimental investigations based upon use of the subjective element. Here the use of instruments appeared at first to be impossible, though later the stop-watch was found to be a most useful instrument. The essence of this method of investigation was the subjective observation and study of one's own mental processes, which a psychologist might carry out on his own person, or witness in another person who would go through the investigation under the psychologist's guidance. The greatest investigator in this field was Freud. He was followed by Jung, Adler, and many others. The achievements of this branch of psychology were so impressive that, though psychologists engaged in it had their differences of opinion as regards methods and interpretations, no one dared to challenge it basically on scientific grounds till 1938, when Caudwell published his essay on Freud.

The third branch of psychology originated in the nineteenth-century investigations of so-called psychic phenomena, such as

telepathy, and of hypnotism. There has been considerable conflict between those who felt satisfied about the reality of such phenomena as telepathy and those who tended to explain such phenomena in terms of deception and of hypnotic effects. Hypnotism has been proved to be an important phenomenon worthy of scientific study by the work of Forel, Vogt, Moll, and others, and telepathy and similar phenomena have been investigated with admirable objectivity and penetration by Boirac.

Co-ordination between these three branches of psychology and their co-ordination with physiological studies of the brain and the nervous system have not yet been achieved, though the need for such co-ordination has become evident, and some of the recent work, such as Young's investigations of the possible physiological changes in the brain consequent on learning, have that objective. Twentieth-century psychology has been the battle-ground of rival theories which, no doubt, will ultimately be reconciled in a wider synthesis. Its pioneers have advanced some sweeping interpretations which have a certain resemblance to the speculations of some of the early pioneers of physics and chemistry, and must prove similarly impermanent. Society which has now developed an intelligent regard for other sciences, both pure and applied, reacts to psychology with a mixture of exaggerated respect and equally exaggerated incredulity, reminiscent of a savage's attitude towards medicine. Perhaps the justest thing to say is that all this proves psychology to be a young science, whose greatest achievements are in the future, and which is still waiting for its Faraday and Maxwell.

We may now pass to Whitehead's philosophy of Organic Mechanism. According to this philosophy the universe is only knowable and understandable when regarded as a succession of organisational levels. "An army," says Whitehead, "is a society of regiments, and regiments are societies of men, and men are societies of cells, of blood, and of bone, together with dominant society of personal human experience; and cells are societies of smaller physical entities such as protons, and so on." Assuming that the electrons, neutrons and other elementary particles which constitute an atom are the basic, indivisible constituents of matter, atoms are the organisations of these basic constituents, molecules are the

organisations of atoms, living cells are the organisations of molecules, and so on, in ascending levels of organisation. Each level of organisation has structural features and laws of behaviour characteristic of itself, and not reducible to mere statistical expressions of behaviour of the simpler organisms of which it is composed. The monads, says Whitehead, "do not blindly run, but run in accordance with the whole of which they form a part".

The truth of these statements cannot be doubted, but it is also evident that they can be regarded only as a mode of presentation of observed facts and not as an hypothesis seeking to explain these facts. To what extent can these statements help a scientist in his research? If one is to interpret them as meaning that no measure of understanding of an organism can be achieved through study of the properties of simpler organisms which constitute it, then Whitehead's philosophy becomes not an aid but a hindrance to progress of research, since it is precisely through study of such constituents that scientific knowledge and understanding of organisms has, in the main, been achieved. But such an interpretation is certainly wrong. The correct interpretation is that while the constituents of an organism do determine its structure and mode of behaviour, the organism in its turn imposes modifications upon the characteristics of its constituents.

Whitehead's own view on this point has been expressed in his attack on "simple location". "To say that a bit of matter has simple location means that, in expressing its spatio-temporal relations, it is adequate to state where it is, in a definite finite region of space, and throughout a definite finite duration of time, *apart from any essential reference of the relations of that bit of matter to other regions of space and other durations of time*. . . . I shall argue that among the primary elements of nature as apprehended in our immediate experience, there is no element whatever which possesses this character of simple location."

The radiation of atoms of long extinct stars does influence a man's future at his birth, though it does so to a negligible extent in comparison with the influence of his heredity, or of the society in which he is born and develops.

The significance of Whitehead's organic mechanism in scientific research may be illustrated by a simple example.

According to classical conceptions, water under normal atmospheric pressure freezes at 0°C. and boils at 100°C. This may be taken to mean that an H_2O molecule under normal atmospheric pressure cannot remain in a solid phase above 0°C, or in a liquid phase above 100°C. But a solution of ethylene glycol in water, containing 52·5% glycol by volume, freezes at −40° C.; a water-alcohol mixture, containing 91% alcohol by weight, boils at 78·2° C.; α-nitroso-β-naphthol cobalt complex does not lose its $2H_2O$ under 130° C.; and it is meaningless to speak of a single isolated molecule of H_2O as being in a solid, a liquid, or a gaseous phase. The behaviour of an H_2O molecule is modified by the society of molecules of which it is a member, and, in the case of compounds such as the α-nitroso-β-naphthol cobalt complex, even to a greater degree by the hydrated molecule of the compound of which it is a constituent. The freezing and boiling points of water at atmospheric pressure are nothing but expressions of modification of behaviour of an H_2O molecule by the society of other molecules, which happen to be similar molecules.

It is evident therefore that Whitehead's organic mechanism offers a new approach in the study of problems not only in biology, but in chemistry and even in physics. To the extent to which this has already been realised in genetics and embryology, it has proved of the highest value to the advancement of these branches of science. To the extent to which psychologists have disregarded it by continuing to treat the psychology of an individual on the basis of "simple location", without regard to past and present modifying influences of the social unit of which the individual is a member, and of the society of which the social unit forms a part, to that extent such psychologists have failed to formulate the exact laws of their science.

CHAPTER IV

THE MENTAL APPROACH

IT IS the author's hope that the reader has reached this chapter by reading the three preceding ones, and not by skipping them as a long-winded historic introduction. The reader who has travelled the whole way through the preceding chapters will have found in them definitions of science and of scientific research, the general principles of scientific research, a warning against adoption in scientific research of religious and mythological interpretations, particularly when disguised as scientific theories, a recommendation to make use in research of certain philosophic concepts, and a presentation of these various matters not in the form of isolated facts but, as nearly as the author was able, in the form of a historically connected organic development. The reader will not have found there, however, anything about scientific mentality, of the correct mental approach to scientific research, of the attributes by which a true research scientist may be known, of whether such things are solely the fruits of natural genius or whether they are, at least partly, a matter of development, and if so, what paths one should follow. It is the author's purpose to try to give an answer to some of these questions in the present chapter.

Perhaps, before saying what makes a man most fitted to excel at scientific research, it may be just as well to say what does not. It must not be assumed that because in the previous chapters it has been stressed that the introduction of religion or mythology into scientific research is harmful, while the adoption of certain philosophic concepts is beneficial, that therefore a man's professed religion, or lack of religion, or professed acceptance or rejection of particular political views or social philosophies, make him a good or a bad scientist. Pasteur was a Roman Catholic, Faraday a Sandemanian, while M. and Mme. Curie rejected religion entirely. Haldane is a communist, while Pavlov was an anti-communist. It would

be absurd to say that any one of these persons could not be regarded as a scientist of the highest order. On the other hand, of course, there is no lack of men who, according to the reader's standards, whatever these may be, profess very sound views on all sorts of things, including religion, philosophy and science, who are for all that quite indifferent scientists. We have also seen in the preceding chapters that learning does not confer the power to achieve new scientific knowledge and understanding, or even to appreciate it when it is offered by others. Ability to achieve new scientific knowledge and understanding is not even conferred by an infinite capacity to take pains, for if it were, then every conscientious, hard-working science teacher, industrial research worker, or amateur enthusiast, would achieve wonders.

What is then the quality which enables some men to achieve great things in scientific research? For greatest achievements men must have genius—that elusive quality that so often passes unrecognised, while high ability receives reward and praise. But for achievement genius is not enough, and, for all but the greatest achievements, not necessary. What does appear essential for real achievement in scientific research is a combination of qualities, by no means frequent, but commoner than is genius. It seems that these qualities are clarity of mind, a combination of imagination and caution, of receptivity and scepticism, of patience and thoroughness and of ability to finalise, of intellectual honesty, of a love of discovery of new knowledge and understanding, and of singleness of purpose. Of these the most important is the love of discovery of new knowledge and understanding. If any young readers, contemplating scientific research as a profession, do not feel this love, they need read no further—scientific research is not for them. Let them choose another profession, less arduous and more remunerative. But if they feel this love, then the author begs them to read on, for it is chiefly for them that this book was written.

Clarity of mind, in so far as it is inborn, calls for no comment here. But it is a quality which is not independent of environment and behaviour. Like any other faculty it can be developed or deteriorated. In scientific work clarity of mind is dependent, to a high degree, on ability to free one's observations and interpretations from emotional bias. At all costs the research worker

must resist the subconscious urge to imagine that certain effects which he desires to see are actually there, when in fact they are absent, to elevate to a high degree of significance observations which he wants to be significant but which are nothing of the kind, to explain away awkward facts which are incompatible with his wishes, to arrange data in an impermissible manner in order to prove a cherished hypothesis, to give undue weight or insufficient weight to evidence on emotional grounds, to decline to give proper credit to new evidence or new theories because of a sentimental attachment to an hypothesis or unwillingness to admit his experimental errors.

Many young scientists will think all these warnings are nothing but a superfluous stressing of the obvious, and imagine that they at any rate are free from such dangers. Unfortunately, few scientists are completely free from such dangers, and even some of the greatest scientists have not been entirely immune. The survival of the caloric theory fifty years after Rumford's cannon-boring experiment, and the omission of important details from the first edition of Crookes's record of his experiments with Hume, are obvious examples. A less obvious, and therefore a more dangerous, case is that of Galton's statistical interpretation of Darwin's experiments on the effects of cross and self-fertilisation of plants. To achieve his interpretation Galton rearranged Darwin's data for purposes of comparison. The result appeared to show a great superiority of cross-fertilisation over self-fertilisation. But, as Fisher has pointed out, this result was only achieved in violation of the principle that "the estimate of error must be based on the effects of the very same causes of variation as have produced the real errors in the experiments". *There was no reason for Galton's incorrect rearrangement other than that it appeared to demonstrate a certain fact, whereas Darwin's correct arrangement did not appear to do so.*

Errors due to emotional bias are not restricted to interpretations of observations and choice between rival hypotheses. They enter into recording of observations, and even into the observations themselves. This is one of the reasons why observations should be recorded as soon as possible after they have been made; in this way they may frequently get on record before their implications have become evident and the subconscious desire to make the evidence more favourable gets

little chance to play tricks with one's memory. In observations, errors due to emotional bias can occur only when the observation involves an element of judgment, or sensations in the neighbourhood of minimum perceptible stimulus values, or the discriminations of minimum perceptible differences of stimulus. No normal trained scientific observer could mistake red for green, or a large deflection of a galvanometer for a small deflection, or a high pitched note for a low pitched note, or a luminous intensity of 100 candles for a luminous intensity of 10 candles of the same colour. But any normal trained scientific observer might make a mistake in an estimate of relative regularity of crystals in two similar aggregates, or in a determination of the highest frequency audible to his ear, or in a visual comparison of two light sources of similar colour whose luminous intensities do not differ by more than 1%. Young scientists may imagine that possibilities of errors of observation due to emotional bias cannot be important, since they must be confined to the regions of observation where errors are possible even without such bias. Unfortunately this is not the case. A great deal of scientific progress has been achieved through work originating in observations near the limits of observational powers. The continual urge to improve scientific instruments has been largely an expression of the desire to extend the region of reliable observations. Many discoveries have been made through observation of barely distinguishable microscope images, of the faintest spectrum lines, of transient effects which disappeared almost before they have been observed. The Mars canal controversy was started by Lowell's announcement that he was just able to see something which other observers found beyond their visual power.

"What is all this?" the reader may ask. "Is this a sermon, or is it leading up to advice to pay daily visits to a psychoanalyst?" It is neither of these things; it is merely an argument in favour of certain practical precautions. Observations made near the limits of observational power, and which appear to have a decisive significance, should be checked, if possible, by one or more observers having the sense involved in the particular observation highly developed, and preferably accustomed to scientific observations, but unaware of the object of the particular observation. The observation check is, of

course, not always possible, because the effect may be transient and not repeatable at will. But if the check is possible, it should not be in the form of "please do such and such a test and see what you get" but in the form of, first, "look at such and such a region and tell me what you see", and, if the check does not agree with the investigator's observation, a second check in the form of "I see so-and-so. Do you?" The first check must always precede the second, because the second one already involves a conditioning element. The second check is, however, by no means a waste of time, *because the check observer is still unaware of the purpose of the observation*, and may be necessary because the check observer may miss the particular point through not looking to see whether it is there. All observations should be recorded as soon as possible after they have been made. If any observation strikes the investigator at the time of recording as particularly significant, and the investigation is being conducted jointly with a colleague, the investigator should, if possible, get the colleague's agreement with what is recorded. Unfortunately such a check on recording is seldom practicable, because the particular observation is seldom made in duplicate even if the investigator and the colleague are working in closest collaboration, and because in the event of close collaboration the colleague is liable to be conditioned in the same way as the investigator. When interpretation of results or formulation of an hypothesis is involved, the investigator, having reached his conclusions, should discuss them, if possible, with a colleague who is *sympathetic and anxious to see the work successful, but has different views on the subject.* A discussion with another scientist who is not sympathetic, or not anxious to see the work successful, is merely a source of irritation, and a discussion with a scientist who has an identical point of view is merely a pleasant waste of time. A discussion with a colleague who is sympathetic and anxious to see the work successful, but has a different point of view, strengthens the interpretation or the hypothesis by exposing the weak points, clarifying the doubtful ones, and leading to constructive modifications which largely free the interpretation or the hypothesis from the harmful influence of emotional bias.

The above suggestions for precautions are not likely to prove excessively popular. Some critics will say that their acceptance

would change a research laboratory into a debating society; others will say that no research worker of ability and confidence would act on them, and that most research has been carried out successfully without their aid.

The answer to the first kind of critics is that significant observations in regions where errors through emotional bias are possible are not a daily occurrence; they do not occur in every piece of research, and a piece of research which does include them is not likely to have more than two or three such observations and extremely unlikely to have more than a dozen. Furthermore, a check observation of the kind described is usually a matter of minutes, and very rarely exceeds 30 minutes. The loss of time in observational checks would therefore be negligible compared to the total duration of research. Discussions of interpretations and hypotheses would take more time on each occasion, but the occasions would be much less frequent; several months, even a year, might pass without any need for such a discussion. As to agreement between collaborators on the contents of the records, if they cannot generally agree without much difficulty, they should work separately or find themselves more congenial partners, while the rare occasions of strong disagreement between well-suited collaborators will only occur when there is a real genuine difficulty which it would be the greatest mistake to disregard.

The answer to the second kind of critics is that in practice the above recommendations are much more likely to be followed too seldom than too often. Junior scientists generally get little opportunity of discussing interpretations and hypotheses with their equals in status, little chance of having their observations checked in the manner described, and seldom feel comfortable in discussing these matters with their seniors. Seniors, on the other hand, do not like to waste too much time in discussing their juniors' problems, and seldom like to discuss their own problems with others, particularly their juniors; sometimes from a feeling of infallibility, more frequently from fear of losing prestige. But desistance from a course of action solely on the grounds of lack of opportunity or through shyness, diffidence, pride or fear, is not in itself a good thing. As to the sense of personal infallibility, this is no doubt very useful to any man for getting on in the world, for acquiring money and status, but

whether it is an asset in discovering scientific truths is exceedingly doubtful. Certainly the greatest scientists, like Cavendish, who again and again repeated his experiments on the composition of air, like Darwin who consulted Galton on interpretation of his results and finally recorded both his own and Galton's interpretations, like Kapitza who, speaking most warmly of his teacher Rutherford, said of his own work that a scientist must not only teach his juniors but also learn from them—certainly such scientists were not afflicted with a sense of personal infallibility. History records many great scientific discoveries where the discoverer did not discuss either his observations or his interpretations with anyone till the work was done, but it seems that in most cases this was due to the fact that there was no one with whom such observations and interpretations could be usefully discussed.

An entirely different aspect of clarity of thought is that which is associated with the practice of assumptions and inferences. In all human activities, be they scientific or non-scientific, quite a large number of things are inferred. A good scientist differs from a non-scientist or from a bad scientist in that he is not so ready to take things for granted, and is much more careful in his inferences. One of the best teachers of scientific method, Silvanus Thompson, frequently stressed the necessity for a scientist to be on guard against unjustified assumptions and baseless inferences. It is worth recalling how, in one of his lectures, after explaining this to the students, he showed them two objects—one apparently a stone, and the other, apparently a large, nicely painted, horseshoe permanent magnet—and asked which of these would attract an iron nail. All the students immediately said that the horseshoe magnet would, and the stone would not. Silvanus Thompson then demonstrated that the reverse was the case; the stone was a lodestone, and the horseshoe "magnet", a painted wooden model. The students' inference had been based on characteristics of the two objects which were not, from a scientific point of view, in any way related to their magnetic properties, though the students' frequent observations had led them, through association of ideas, to infer such a relationship.

The care taken to avoid unwarranted assumptions and baseless inferences is one of the greatest differences between scientific

and non-scientific mentality. Ability to exercise such care is a natural gift, but, like all gifts, grows with training and diminishes with disuse. No one can be a true scientist without possessing this quality. The possessors of this quality, however, may be divided into two groups. The first group is large, and includes all "moderately good" to "good" scientists who take as their starting point the acceptance of validity of existing scientific knowledge, both observational and theoretical, and use it as their standard of reference to determine whether various things with which they have to concern themselves in their investigations may or may not be taken for granted, may or may not be inferred. The second, much smaller group, includes all the best scientists, including all the greatest ones: scientists of this group go further in their scrutiny, and do not accept the existing scientific knowledge, both observational and theoretical, as either completely valid or endowed with permanent, invariant quality.

The separation of scientists into two groups, differing in their acceptance or non-acceptance of existing scientific knowledge as an infallible standard of reference, is not an arbitrary division. There is a qualitative difference between the two groups not only in their technique of investigation but also in the results they achieve. The concepts, interpretations and laws which are constituents of science are in a state of continued change and development, and even observations made with the greatest care and recorded as incontrovertible facts must be revalued, and may be invalidated by new knowledge and new technique of observation. The greatest advances in science cannot therefore be made by those who accept the scientific knowledge offered them by their predecessors as a complete and invariant truth. But it does not follow that those who do not question the validity of the results and interpretations of preceding scientists are thereby rendered incapable of successful research and of adding to scientific knowledge on their own account. The most slavish acceptance of preceding scientific work cannot prevent a physicist from accurately determining the melting point, or specific resistance, or magnetic properties of another five, or another fifty, substances for which these characteristics had not been determined previously. On the other hand, lack of knowledge may make successful research impossible, though a great pioneer of research need not be

a deeply learned man. A scientist cannot reject previous data, concepts and interpretations without knowing them. It follows that whether a scientist is prepared to accept the work of his predecessors unquestioningly or whether he is prepared to challenge it, he must in either case possess himself of adequate knowledge of what has already been achieved in the particular sphere in which his investigations must lie. The problem is how to acquire the necessary knowledge without having one's mind "set" in the process.

The same problem arises in connection with combining the mental qualities of receptivity and scepticism, except that in the latter case the knowledge to be accepted or rejected is wider and extends beyond consideration of the work of preceding scientists. Readers who have begun to imagine that they are merely offered a conundrum without an answer may be reassured: it is not the purpose of this book to state problems without suggesting solutions.

A young man or woman about to embark for the first time on serious scientific research may be fortunate enough to have had the benefit of a university Honours degree course, and possibly may have taken an M.Sc. or Ph.D. as a result of post-graduate work; or, being less fortunate, may have had far less advantage of recognised forms of scientific education and may have had to supplement these to a great extent by self-education. In either case he or she will be starting the first serious piece of scientific research with a store of general scientific knowledge, and almost certainly with little specialised knowledge relating specifically to the problem in hand. Those who have had the benefit of a first-class university education in science will have an advantage over those who have had to rely to a considerable extent on self-education. But the latter need not be unduly despondent, for their journey is only just beginning, and more than one great scientist had to start with a similar handicap.

The initial fund of general scientific knowledge is an invaluable asset, but the young research worker should have no illusion about how little it is compared with what he or she should acquire during succeeding years. As to the precise value of this initial fund of knowledge, this depends to a great degree on how it has been acquired and on who has been imparting it. Young scientists cannot realise too soon that existing scientific

knowledge is not nearly so complete, certain and unalterable as many textbooks seem to imply. The original papers of great scientists describing their discoveries and expounding their theories are never as rigid and self-confident as the résumés of these discoveries and theories in textbooks by other men often suggest. Young scientists consulting these original works will find in them "it appears that", "it probably means", "it seems likely that", more than once, not as expressions of good manners or false modesty, but as expressions of elements of doubt which great men felt and honestly put on record. Many statements which have appeared in textbooks as universal and incontrovertible truths have, in their original form, been put forward as only approximately true, or true only in certain circumstances.

Immediately upon starting on the first serious piece of research a young scientist must therefore do two things. The first of these should be a careful reading of original papers or books relating to the problem, written by investigators whose technique and judgment he can trust. While reading these publications in a most attentive and receptive manner, the young scientist must not fall into the error of placing in them a greater confidence than their author would wish him to do. No great scientist ever wants his pupils to be mere gramophone records, faithfully reproducing his remarks, never questioning anything, never wanting to add or subtract from what he has given them.

The second thing a young scientist must do, almost but not quite simultaneously with the first, is to proceed with observations and experiments. The initial observations and experiments will be failures, but they will help the development of the appropriate experimental technique, and *they will give a greater understanding of the literature the young scientist is studying.*

From that point onwards reading will be a guide to experiment, and experiment will enable a proper appreciation of that which is read to be made, and enable the young scientist to judge more accurately what must be accepted as an enlightening truth, and what must be viewed with scepticism. Eventually the young scientist will be able to grasp the problem thoroughly with that combination of understanding, optimism, and caution which is essential. In the later stages of the particular

piece of work further reading will become unnecessary. The young investigator will be no longer in need of finding what is known, but will be adding to that knowledge.

Naturally, reading does not permanently cease at that point, and will have to be resumed for the next problem, or even for the present one if either an unexpected difficulty or a new, highly important, publication turns up. Each time the balance between receptivity and scepticism will be easier to achieve

Reading is not the only way to acquire knowledge of preceding work. There is another large reservoir which may be called experience, and the young scientist will find that every craftsman—the glassblower, the instrument maker, the practical gardener, or the man expert in furnacing—have all of them something they can teach and will generally teach gladly to any young scientist who does not look down upon them with ill-concealed disdain. The information from these quarters differs from information in scientific books and papers chiefly in that its theoretical part—the explanations of *why* things happen—is frequently quite fantastic. But the demonstration and report of *what* happens, and *how* it happens, are sound even if the reports are in completely unscientific terms. Presently the young scientist will learn, in this case also, what to accept and what to reject. One important thing for a young Honours graduate to remember is that if Aristotle could talk to the fishermen, so can he.

With time the young scientist, by then not so young, will find that the craftsmen have now less to teach, while scientific publications have still much to teach and his own researches still more. But by that time a lasting understanding will have been established between the not-so-young scientist and the craftsmen, and the experiments dependent on the craftsman's co-operation will not go wrong and have to be repeated and refashioned in the way they might have to be if such a bond did not exist.

To older scientists, experienced in research, another source of knowledge is open. It is the vast store of traditional practices handed down from father to son, or mother to daughter, of old country customs, of folk lore, of mixtures of native superstition and wisdom. All this is too difficult for a young scientist to explore. Too much knowledge and personal experience is needed here to separate good plants from wild weeds. Older

scientists, on the other hand, are too conscious of the contempt the votaries of established science have expressed about this source of knowledge. But good scientists should remember how much of real value science has found in this wide, confused wilderness and how often scientific discoveries turned out to be rediscoveries of what had existed in this wilderness long ago. Electroplating was practised in Babylon; the magnetic needle of Gilbert's experiments was used by Muslim sailors several hundred years before him, inoculation against small-pox was practised in Constantinople more than a century before Jenner's vaccination; wise old men and wise old women used fungi to cure wounds many centuries before penicillin; men working at steel furnaces or blowing bulbs from molten glass knew that the radiation from the furnace could heal burns, long before the healing properties of infra-red radiation were discovered. Science was not created out of pure thought and starlight—much of it is nothing but systematised experience of humanity.

All that has been said so far about receptivity and scepticism does not dispose of the subject in a satisfactory manner. There is one more aspect, and perhaps the most important one. Many scientists, not only junior and inexperienced ones, but also those senior and with long experience at the back of them, lack responsiveness to new facts and ideas unless these happen to coincide with their previous finite experience and personal points of view. Such scientists simply close their minds tight against everything which does not fit in with their preconceived notions, one of which is that they must never acknowledge their mistakes or admit that their knowledge and understanding had been imperfect. Needless to say, such scientists pay the price by failing to be good scientists. Other scientists, far less numerous, show a positive absence of scepticism in matters where belief gives happiness, or possibly merely pleasure. The uncritical investigators of psychic phenomena come within this category. There is no remedy for excessive scepticism or excessive credulity except self-criticism and an honest examination of one's motives for accepting or rejecting evidence and theories. If the remedy seems unpalatable to any scientist, it may perhaps be made less so by the thought that the penalty of excessive receptivity or excessive scepticism is the loss of power to conduct scientific research of any high merit.

To say that in research one should combine imagination and caution, and to say no more, is to state a platitude. Everyone will agree the soundness of the combination in principle. But only a fraction of those engaged in research have made effective use of the combination in practice. In the extreme case, when caution is negligible and imagination highly developed, successful research becomes impossible. On the other hand, going to the other extreme, and putting caution at a high value and imagination at zero, a condition results when certain kinds of rather pedestrian research can be conducted quite effectively. In any research organisation junior workers are not expected to show outstanding imagination; they are expected, however, to do their work with care and accuracy. Indeed, in a research organisation a junior with little imagination but much caution can produce good results, while a junior with imagination and little caution may produce disastrous ones. In the absence of adequate knowledge, imagination may cause harm, while caution cannot do so. For all these reasons science students are in general trained to exercise caution and discouraged from flights of imagination. Yet for research work of a really high quality imagination and caution are alike indispensable, and achievements of the highest order require great imagination combined with proper caution.

It may be said that, the greater the imagination, the greater must be the caution to achieve a satisfactory balance. An investigator keeping unimaginatively to a well-worn path is safe so long as he preserves a reasonable minimum of caution. But an investigator who has allowed imagination to lead him into untrodden paths needs extra vigilance to avoid fatal mistakes the existence of which cannot be indicated by prior experience.

A form of scientific training which stresses the necessity for caution and, at the same time, discourages flights of imagination, is unsatisfactory from the point of view of scientific research because it is excellent only for mediocrities. There is a need for a form of training which, while teaching caution, would also develop creative imagination. However, young scientists who have just embarked, or are about to embark on research, cannot very well return to their student days as a preliminary to such research, even assuming that the entire

system of scientific training were modified on the lines here suggested, more or less overnight. In any event, the existing system of scientific training is not likely to be changed as readily as all that, even if the majority of professors and teachers engaged in the teaching of science feel exactly the same way as does the author on this question. Young scientists engaged, or about to be engaged, on research are therefore faced with the problem as to how they may best help themselves without waiting for outside help. It is one of the tasks of this book to suggest to them how they may do so with reasonable prospect of success.

There is unanimity of opinion that caution, whether inborn or not, may in any event be developed by appropriate training, and indeed scientific training to-day in general excels in promoting caution. Imagination is, however, a very different matter. Undoubtedly no amount of training will develop any fruitful imagination in those devoid of that gift. But in those who have a natural gift of imagination, training can either stifle it or help it to develop into a controlled, scientific imagination which is very different from the unrestricted imagination of an unscientific mind.

To get the best out of their imaginative powers, young scientists should in the first place steel themselves not to call upon these powers unnecessarily, and in the second place use them fearlessly when circumstances justify this. There are only three circumstances when the use of imaginative powers is fully justified in regard to methods of research, and only one circumstance when such use is fully justified in formulation of theories. The circumstances in which there is full justification for using one's imagination to devise new methods, which might range from a complete change in approach to an improvement of a recognised technique, are:

1. When the orthodox methods fail or are inapplicable to the particular piece of research.
2. When the orthodox methods are difficult or tedious to apply.
3. When the results obtainable by the orthodox methods do not possess the desired degree of accuracy.

The only circumstance in which there is full justification for using one's imagination to construct a new theory, is when the

orthodox theory is shown to be unsatisfactory either because it does not account for certain undeniable facts, or because it contains some completely untenable assumptions.

If the first step, the justification of the use of imagination, has been made, the second step is not a completely unrestricted indulgence in imagination in the manner of a writer of "scientific" fiction whose only criterion is a vivid imagery satisfactory to the reader. Imagination, applied to problems of scientific research, may have a complete freedom only within a certain framework, the boundaries of which must not be transgressed. These boundaries are:

1. In no circumstances is it permissible to evolve a method or a theory which the originator knows to involve any element inconsistent with existing observational data or scientific theories, unless the originator of the new method or theory is prepared to question the validity of such existing data or theories.

2. In no circumstances is it permissible to evolve a method or a theory if this unavoidably involves a theoretical postulation which is, by its nature, incapable of scientific verification.

3. In no circumstances is it permissible to evolve a new method which the originator knows to contain any element which, though different from unsatisfactory elements inherent in the old method, appears to the originator to be a serious disadvantage for which the originator can suggest no remedy.

4. In no circumstances is it permissible to evolve a new theory involving one or more artificial assumptions which the originator does not feel to be reasonable in themselves, but has introduced for the sole purpose of perfecting the new theory, which would otherwise be untenable.

5. A new method, otherwise satisfactory, which makes repetition of observations by the same observer, or confirmation of observations of one observer by those of another, or reproduction of exact conditions of observation, materially more difficult than in the case of the old method, cannot be regarded as satisfactory.

6. A new theory which does not explain at least some of the observed phenomena better than the old theory, nor has the advantage of simpler and more natural assumptions, cannot be regarded as satisfactory.

After the step of actual application of imagination to the devising of a new method or formulation of a new theory, come the difficult steps of examination, verification and, finally, application to the particular problem in hand. As these steps do not come within the scope of mental approach, they will be considered later in the appropriate chapters. It may be remarked here, however, that even senior scientists, including scientists of the highest order, do not always get an opportunity to carry out these steps. Junior scientists are in general faced with considerable handicaps. They usually cannot proceed without approval of a senior scientist, who is not invariably sympathetic to flights of fancy on the junior's part, and may, in any event, be unable to sanction the work owing to lack of facilities, lack of time, or the nature of the programme in hand. Furthermore, examination, verification and final application of a new method or theory to the problem in hand generally involves knowledge and experience which a junior is still in the process of acquiring and which a senior, even if sympathetic and anxious to help the junior's progress, frequently has no time to supply.

Junior scientists will therefore find very frequently, and senior scientists quite often, that examination, verification and application of a new method or a new theory are impracticable. The correct course in such circumstances is not to throw the whole thing into the waste-paper basket in a "what's the use?" spirit, but to put the whole thing on record in the hope of a future opportunity. Even if the opportunity never comes, the process of evolution of a new method or a new theory will not have been a waste of time—they will have served to develop the scientific imagination of the junior scientist, or keep flexible the scientific imagination of a senior one, and to give zest to the work in hand and, if not immediately, then sooner or later, this will bear fruit.

Patience and thoroughness are indisputably necessary to success in research, as is the ability to finalise. It is obvious that essentially these qualities, like receptivity and scepticism, or like imagination and caution, are opposites, and that it is not either one or the other of the opposites, but their synthesis, which makes for success in research. Patience and thoroughness without ability to finalise can produce only an endless accumulation of data which remain dormant until another investigator

with a power to finalise takes it up and extracts from it the fruitful conclusions. Ability to finalise, without the capacity for patient and thorough work, can give at best a succession of brilliant ideas that perish unless they succeed in inspiring a more solid investigator; they are like flowers without roots. For greatest scientific achievements those engaged in research must possess both qualities in a high degree and in the right proportions. Patience and thoroughness are largely matters of self-discipline, and no explanation concerning these is needed. Capacity to finalise and the appropriate balance between it and the capacity for patient, thorough work, are less obvious. At what point should one decide that the time has come to finalise? And if one has made the decision, for or against, how does one know whether the decision is really right? If Darwin and Wallace had such different ideas concerning the amount of evidence necessary to substantiate the validity of the same theory, how is a lesser scientist to come to a decision?

The questions having been asked, the reader has a right to an answer, even though it may be difficult to give it. It may be said that capacity to finalise is a natural gift, though less obviously so than the gift of imagination, or of clarity of thought. Some people are definitely unable to finalise their work, but are merely able to discontinue it, which is not at all the same thing. Most people, however, have some capacity to finalise, though a few only can finalise in a brilliant fashion, and of these few a number fail to do so because they continually see fresh vistas beyond each experiment and conclusion. To those possessing some capacity to finalise, whether in a high or only in a moderate degree, the matter may, at least partly, be reduced to one of initial judgment.

There are several questions which every investigator should ask at the commencement of research. What is the objective? Does the objective justify an indefinitely prolonged effort, or does it justify only a limited effort? If an indefinitely prolonged effort is justified, the only limitations that need be seriously considered are those of resources likely to be available and the useful span of one's life. More often, however, only a limited effort is considered justifiable, and in such an event the next question is—just how much time and resources may be devoted to the objective? The answer gives the *maximum* effort that may

be devoted to the task. The next question is—how much time and resources would be necessary to achieve the objective in the most favourable circumstances? The answer gives the *minimum* effort that may be devoted to the task. Assuming the estimates have not been wildly out, the actual value of the necessary effort must lie between the maximum and the minimum limits, though not necessarily half way between them. An experienced scientist will be able to make a reasonably accurate estimate. A young, inexperienced scientist may greatly overestimate, or underestimate, the necessary effort, but will quickly learn from mistakes to make more reasonable estimates. Thus, for all except the beginners, who in any event would almost certainly have their estimate checked by a senior investigator, half the sum of the minimum and the maximum time and half the sum of the minimum and maximum resources necessary for the achievement of a particular objective should be reasonable indications of what the work would actually involve. *An estimate can now be made at what point the finalising process should begin.*

An inexperienced investigator is prone to underestimate the time and resources necessary for finalising a piece of research. To the inexperienced it generally appears that 10–15% of time and resources involved in a particular piece of research should be adequate for finalisation. This takes no account of the fact that the process of finalisation frequently reveals the necessity for additional information which can be obtained only through numerous observations, careful experiments, or mathematical analysis of a fairly extensive nature. It is far safer to assume, in the first place, that something like 30% of the estimated time and resources would have to be devoted to finalisation. In other words, it is advisable to assume that the time that may be devoted to the research prior to any attempt at finalisation and the resources that may be devoted to it during that period, would each be $\frac{1}{3}$ (estimated minimum + estimated maximum). The time and resources thus made available for finalisation of work would be $\frac{2}{3}$ estimated maximum — $\frac{1}{3}$ estimated minimum, and should be adequate.

It must not be assumed that by following the above suggestions anyone engaged in research would automatically acquire the capacity to combine patient and thorough work

with ability to finalise the results. No method of procedure can confer such powers on a person devoid of the natural gifts for such an achievement. But the suggested method of estimation and of arrangement of work can assist those who possess such a natural gift by ensuring that it is not frustrated by lack of foresight in the planning of the work, and at one and the same time by promoting the thoroughness of the work and aiding the effort to finalise it.

Intellectual honesty needs little comment here. Unlike the subconscious emotional bias to which the noblest natures may be prone, intellectual honesty is a conscious thing just as much as professional integrity or marital fidelity. Subconscious urges may be the causes of intellectual dishonesty, but in scientific work they cannot be invoked as an excuse, because even if charitable friends might forgive such a lapse, science never would.

One last thing remains to be considered before this chapter is closed—singleness of purpose. To those for whom scientific research is just a pursuit provisionally selected out of a number that appear equally attractive, singleness of purpose is not essential. Such scientists might do some meritorious research before they abandon it for administration or teaching, or production or sales posts in industry. They might attain a good reputation, money, and a good social position. The one thing they can never attain is greatness. Greatness is possible only for those engaged in research who have chosen it because of their great love of discovery of new knowledge and understanding, and so could chose no other. It is for these scientists, capable of greatness, that singleness of purpose is necessary. For such scientists frequently combine with their love of discovery other wishes, which are reasonable enough no doubt, and which they are unwilling to discard. These other wishes, like a wish to provide for the comfort and happiness of those one cares for, or a desire to help one's fellow-men and -women, or an urge for creative achievement in music or painting, cannot be fulfilled without expenditure of resources. No doubt, happy personal relations, an active interest in the life of fellow-men and -women, an interest in music and painting, are not deterrents to scientific research—on the contrary, they make a scientist's capacity for research greater by making his life fuller

and richer in content. But for a young scientist an attempt to build up a comfortable home, to become a political leader, or a composer or painter, would require more resources than can be spared.

Young scientists, frequently the most gifted ones, are prone to imagine that they can "manage" somehow to do everything they want. Are not there many scientists with comfortable homes? Are not there scientists with interests outside science? Was not Leonardo da Vinci great in half a dozen things? Alas, comfortable homes take many years to build, older scientists can afford more time and money for outside interests than young ones, and Leonardo da Vincis are rare. A young scientist embarking on research may have apparently inexhaustible resources of energy and enthusiasm, but, in this country at any rate, except in a minority of cases has only slender and precarious economic resources. It is precisely the slender and precarious economic resources that will be strained in the process of fulfilment of these most natural and finest desires—either directly, or indirectly by reducing the effort devoted to research and so extending the time necessary for the completion of the work. The only solutions to the dilemma appear to be either the sacrifice of fulfilment of these desires for the sake of research, or the sacrifice of success in research for the sake of fulfilment of these desires, or an effort up to and beyond the limits of physical endurance. It is not an accident that so many scientists engaged on research appear to develop blinkers which shut out so much that is worth seeing; not an accident that the wives and families of so many pioneers lived through years of poverty; not an accident that so many promising young scientists abandon research they love, "temporarily", to earn more in other branches of activity where science is better paid, only to find that, when at last their resources have become adequate, nothing can give back the vanished years. It would be an impertinence to suggest to young scientists devoted to research what sacrifices they should make. But it is the act of a friend to tell them that, in this country at any rate, they may have to face the need of sacrifices, and may have to ask themselves whether these are worth while.

CHAPTER V

THE PLANNING OF RESEARCH

PART I

THE PLANNING of any research should logically be determined by considerations of the nature of the problem, the method which the investigator proposes to apply to its investigation, the thoroughness with which this method is to be applied, the available resources, and the available time. It is impossible to plan the research in an absolutely satisfactory manner unless all these factors are known, and unless the magnitude of the last two factors is commensurate with the other factors involved. Strictly speaking, the only person competent to plan the research is that scientist who is actually, and not merely nominally, in charge of it, and even he is not able to do so adequately if he cannot control, or at least know precisely, all the above factors. Unfortunately these essential prerequisites of successful planning are frequently absent. In the best circumstances, if the research is conducted at a university, the professor in charge of the faculty, or if the research is carried out in an industrial organisation, a scientist acting as director of research, has all the factors before him, and can make a reasonable decision, though he may often find that the resources, or the time available, or both, are not commensurate with the most satisfactory prosecution of the particular research. If the professor or director of research concerned is personally conducting the research, the planning will be as satisfactory as might be reasonably expected, and may be highly satisfactory if the resources and time available are adequate. But such an ideal state of affairs is not common. To begin with, as has already been remarked, the resources and the time available, or both, are frequently less than are desired. Again, the professor of the faculty or the director of research does not invariably personally conduct all the research under his jurisdiction, but,

on the contrary, only a fraction of it, the rest being carried out by investigators for whom he is responsible. These investigators, however happy their relations may be with the particular professor or director of research, are separate entities with somewhat different conceptions of the method and of thoroughness involved in the research in question, and planning accordingly cannot be ideal, though it may still be reasonably good. These however are the best cases. It is more frequent to find a state of affairs where the investigator actually conducting the research is not fully informed of the resources and time available, and so has to plan partly in the dark. In worse cases, particularly prevalent in industrial research, the investigator gets no real opportunity of making a rational estimate of the resources and time available, because the powers that be who determine these two highly important factors, and are themselves quite unscientific, regard the scientist conducting the research as a person wholly incapable of judging such matters. In the worst cases, likewise found in industry, the powers that be, not content with determining the resources and the time without regard to the investigator's views on the subject, feel that the investigator, being a mere scientist, is also less able than they are to determine the method of research and the thoroughness of its application, and impose modifications of these upon the investigator with most unfortunate results. We shall return to this subject in another chapter. In the meantime, we shall assume that somehow the investigator has formed a general picture of the position and is proceeding to plan the work to the best of his ability.

The general scheme of work must, in the first place, be framed very broadly and regarded as a provisional guide. The objectives are of course clear at the start, or at least should be so if the research is to have any real prospect of success. But the methods by which these objectives may be achieved are not necessarily clear at the commencement of the research. Indeed, it is often not certain at the beginning whether all the objectives are attainable in the course of the single piece of research, or even whether all of them are attainable at all. It can merely be hoped that they will be attained. The nature of planning must depend to a great extent on the nature of the proposed research. For purposes of planning research may be conveniently classified into four groups:

1. Research in which the apparatus and the technique of observations and experiments can be predicted with a high degree of precision. This type of research includes such widely differing investigations as astronomical observations, investigations of electrical and magnetic properties of substances at very low temperatures, study of effects of various chemicals upon the growth and development of various plants and animals, study of forms of life present in polar or tropical regions, determination of optical rotation of various liquids, and determination of crystal structure of various substances. In all these, and similar cases, while the results of the investigation cannot always be predicted and may in some cases be unpredictable, the methods of investigation can be confidently based, sometimes without any modification whatever, upon the work of previous investigators.

2. Research in which the results may be forecast to a considerable extent, provided a suitable method is applied, but where details of technique and of the apparatus used are themselves matters of experimental determination. This type of research includes such types of investigation as work in powder metallurgy and imparting the desired structure to the ductile metals produced, exploration of conditions at altitudes exceeding those previously accessible to similar examination, mass separation of isotopes, synthesis of organic substances not previously synthesised, and production of large artificial precious stones free from faults. In all such cases it is known fairly well what results would be got, if it were known how to get them, and the work of previous investigators is an invaluable guide, but modifications of known apparatus and of technique are indispensable to success.

3. Research in which the result may be forecast in the form of possible alternatives, but where new apparatus and new technique have to be devised to make the investigation possible. This type of research includes such investigations as J. J. Thomson's determination of the mass and charge of the electron, Davisson and Germer's electron diffraction, and Millikan's oil-drop experiments.

4. Research in which no accurate initial forecast is possible, either with regard to final results or to apparatus and techniques which would ultimately be found suitable for successful

prosecution of the investigation. An excellent example of such research was the production and separation of artificial radioactive elements with atomic numbers greater than that of uranium.

Detailed planning is easiest in the case of Research Type 1 and most difficult in the case of Type 4. Paradoxically, it is easier in the case of Type 3 than in the case of Type 2. The reason for this paradox is that Type 3, by allowing alternative results, may be limited in scope by the initially formulated plan of action, and the initially devised apparatus, unless the investigator is not content with the particular result obtained, detects certain objectionable features in the method or apparatus employed which were not initially apparent, and, instead of finalising, repeats his investigations using a different method and different apparatus. In other words, a particular piece of research belonging to Type 3 may be of value even if the result is negative and the conclusion embodies a recommendation to tackle the problem afresh in another way. Type 2 must be regarded as a failure *unless certain predetermined results are achieved*; here initial planning is made more difficult by the fact that method and apparatus which may at first have been postulated as suitable are almost certainly bound to prove inadequate, and possibly may have to be not merely modified, but entirely superseded by something quite different.

In every type of research, including Type 1, the investigator must expect to find that all apparatus required is not ready to hand and not obtainable by simple purchase, and that certain items must be specially constructed. With Types 3 and 4 construction of special devices becomes a matter of highest importance. In many cases at least some of the materials necessary may prove difficult or impossible to purchase, and the preparation of these becomes an integral part of the research. In some cases the entire piece of research may be carried through successfully by a single investigator, given the requisite facilities; in other cases, for example those involving a very large number of experiments in a limited span of time, or a number of exacting simultaneous observations, a single investigator, being merely human and indivisible, cannot possibly cope with the work single-handed. In some cases an initial plan of investigation can be formulated with considerable confidence;

in other cases the initial plan can be formulated only with the mental reservation that the methods proposed may need considerable modification; in yet other cases the initial plan can only be framed by selecting without any great confidence one out of several possible methods, with the knowledge that subsequent proceedings may show that the wrong method had been chosen; in the most difficult cases an initial forecast of the correct method is practically impossible, the correct method emerging gradually as a result of groping through a maze of possibilities and difficulties.

From the foregoing remarks it is apparent that a comprehensive plan requiring little subsequent modification, and embracing apparatus (perhaps a more general term "equipment" might be better), materials, research personnel, and methods, is in general possible in the initial stages of Type 1; frequently but by no means always possible in the case of Type 3; almost impossible in the case of Type 2; and quite impossible in the case of Type 4. Thus initial planning must be followed by re-planning in not a few cases of Type 3, very frequently indeed in the case of Type 2, and invariably in the case of Type 4. Nevertheless, initial planning is both possible and worth while in all circumstances—the only question, settled by the nature of each particular piece of research, is how far such initial planning can be complete and to what extent it must be elastic.

In the cases of Types 2 and 4, and sometimes in the case of Type 3, preliminary investigations before the initial plan is formulated are a great advantage. Such investigations are unfortunately not always possible. They are obviously impossible if the investigator has not got at the time the facilities of an existing research organisation, or if such facilities are hopelessly inadequate for the preliminary investigations in question. But even if the facilities necessary are available, it does not follow that the investigator will be able to make use of them. In a government research establishment, or in a university laboratory, such preliminary investigations will meet with approval. In an industrial research establishment, particularly in a small one with very limited resources, opposition to any but the most elementary preliminary investigations may be considerable. As a typical illustration of

such difficulties the author would refer to an experience he had as quite a young man. He was engaged at the time in a laboratory of an industrial concern, and the problem he had to deal with was the production of a certain entirely novel device which was to have certain desired characteristics. The problem therefore belonged to Type 2. Quite naturally, he proceeded in the first place to determine certain physical data as a basis for attack on the main problem. A director of the company, who had no knowledge of science but was a keen business man, called at the laboratory to see what progress had been made. He was shown a sheaf of graphs and a number of equations and, more in sorrow than in anger, told the author to leave all such things alone until he had produced at least one commercial model of the device required. The author now recalls the incident with considerable amusement, but at the time his feelings were a mixture of exasperation and despair. To-day industrial organisations exhibit a higher intelligence, but the improvement is largely limited to bigger organisations. There are to-day research organisations attached to various great industrial undertakings where research of the highest order is proceeding under favourable conditions. But it would be a mistake for a young scientist to imagine that small industrial laboratories are miniature models of the same thing.

Assuming the investigator has been able to take a general stock of the situation, has taken into consideration the resources and time available, or, if precise knowledge of these two factors has been denied him, made the best guess he could as to their magnitude and possible variation; assuming that he has acquired sufficient knowledge of the problem, by way of reading, experience, information supplied by fellow-scientists, or a combination of these, to be able to picture a suitable plan of attack on the problem either as a whole or in its early stages; assuming furthermore that he has either been fortunate enough to be able to make any necessary preliminary experiments or, failing that, has been able to substitute for them by some reasonable theoretical speculation—assuming all that, the investigator is in a position to proceed with the formulation of his initial plan. In no circumstances should an investigator agree, under pressure from any quarter, to formulate the initial plan before he has been able to prepare himself for the

task in the above manner. If he yields on this point, against his better judgment, and acts in haste, he will certainly repent greatly at leisure. He will find the achievement of success of research based on an impromptu, wildly speculative plan, extremely difficult if not impossible to realise, and those who have urged him to such folly will be the last to sympathise with him in his misfortune. They will forget all his protests and only remember that the plan was of his own making.

In formulating his initial plan the investigator must not lose sight of the possible need of subsequent revision and, particularly in the cases of Type 2 or Type 4, must take care that the resources and time at his disposal are not too heavily committed to pursuit of the initial plan and that, when a revision of the plan indicates that certain methods and apparatus adopted in the first place are unsuitable and other methods and apparatus are essential, ample resources and time are available for the change. Here again the investigator must be prepared to resist pressure from anyone who urges him to "make up his mind" in the sense of making unfounded irrevocable decisions in the early stages of his work.

With these points firmly in mind the investigator should proceed with the formulation of the initial plan and, in the first place, turn his attention to method. The design of the method involves a choice of the sense perception basis to be used, consideration of the relationship between the phenomena investigated and the sense perceptions engendered by them, examination of possibilities of amplification of the sense stimuli, the problem of observation at any desired instant and over any desired time interval, the problems of elimination of undesirable factors, the problems of production and control of desired factors, the problem of combination of accuracy with minimum effort, and the problem of minimum number of essential observations.

All investigations, except mathematical ones, must include observations and experiments and even mathematical investigations, if related to the real world and not to a world of pure mathematics only, need at some stage or another observation and experiment to verify their conclusions. All observations and experiments, even those in the domain of psychology, necessitate the use of sense perceptions. The sense of sight has

been used far more than any other sense in scientific observations, both for quantitative and qualitative information. The sense of hearing has been used to a much smaller extent. Other senses—the senses of smell, of taste and of touch—have been relegated to positions of very minor importance. This discrimination between the senses for purposes of scientific observation has been increased by continued development of new devices enhancing the powers of vision. Yet, at the same time, the increasing perfection of processes and of scientific apparatus has come about through the use of the human hand and the increase of its powers by various means.

The predominant position held by the sense of sight in scientific observation does not mean that other senses are incapable of furnishing quantitative information; still less does it mean that other senses are entirely useless for research purposes. In the case of taste, for example, it is definitely possible to talk of the degree of sweetness. Commonly sensations of taste are divided into four qualitatively different groups—sweet, bitter, acid and saline. Actually, this division is an oversimplification. It does not include certain characteristic taste sensations excluded from domain of taste by theory, but recognised by the tongue, such as those produced by alcohol, or by oily substances, and scores of tastes which cannot be defined in scientific terms, and appear to be compounded of true taste and of smell, like the taste of a freshly baked roll, of a hard-boiled egg, or of celery. Certainly there are possibilities of acquiring qualitative information through the mechanism of taste sensations, not only in regard to edible substances. Certainly, also, in a number of cases quantitative information could be obtained; presumably, for example, there could be a scale for expressing the degree of an oily taste, though no such scale exists at present. But it is all too obvious that the amount of scientific knowledge which may be acquired by investigators through the medium of taste is extremely limited.

The sense of smell is undoubtedly of value in chemistry, particularly in organic chemistry, and if any investigator could be found who combined the intellect of a scientist with the sense of smell of a dog—a most unlikely combination—he would find the sense of smell useful over a much wider field of

investigations. But the sense of smell is even less capable than that of taste of giving quantitative information.

The sense of touch is actually far more useful in scientific research than either the sense of taste or the sense of smell. Its undeserved discredit has been due in no small measure to the history of society, in which a scientific gentleman might see and hear the world around him, but the use of the skilled hand was primarily the province of inferior men—of slaves, of serfs, and later of free but essentially uncultured and ungentlemanly craftsmen. The story of how water at a given temperature may appear either hot or cold to the hand depending on whether the hand has been previously dipped in colder or hotter water, has been reiterated in numerous textbooks, regardless of the fact that the human eye is equally capable of being conditioned —dark-adapted, or light-adapted, or fatigued by red, green or blue light. Actually the sense of touch comprises two quite different types of sensations—the tactile sense which comprises the power of distinguishing differences of pressure, the power of localising the place of contact, and the power of distinguishing contacts in time, and the thermal sense which is the power of perceiving addition or withdrawal of heat. Even if the thermal sense were entirely valueless in scientific research, the tactile sense, which Head established as being conveyed by a different group of nerves, and which, unlike the thermal sense, does not appear to be subject to previous "conditioning", would suffice to make the sense of touch valuable in scientific investigations. There are numerous cases where the unaided sense of touch gives more reliable information than the unaided sense of sight; silica and glass, natural silk and artificial silk, differences in grain size and hardness of powders, all can be distinguished more readily by touch than by sight.

Unlike the sense of taste, or the sense of smell, which may be the guiding sense in a plan of research only when the final results have actually to be expressed in terms of that sense, the sense of touch may be the guiding sense in a plan of research even if it does not appear either in the formulation of the final result or in the last stages of the investigation. The foregoing may be presented more clearly by means of an illustration. In a group of phosphors known as manganese-activated zinc-beryllium-silicates, the colour and brightness of the fluorescence

under 2537ÅU excitation is dependent upon the proportions of the ingredients and the thermal treatment to which the phosphor has been subjected during preparation. Unfortunately the colour of fluorescence of these phosphors cannot be expressed simply in terms of proportions of the ingredients, since for any composition a variation of colour of fluorescence from green, to yellow, to orange, and finally to pink, can be produced by varying the thermal treatment of the mixture. At first sight it appears that the entire investigation must be based on the use of sense of sight, since it involves readings on the balance to determine weights of ingredients, pyrometer readings of temperature, and readings of time of treatment on the clock—and finally examination of the fluorescence produced by suitable short wave excitation. For reasons given above, however, all these visual observations only appear capable of furnishing a large amount of interesting and perfectly valid, but also perfectly unmanageable data. A solution of the problem on the basis of correlation of fluorescence with exothermic and endothermic reactions and the X-ray crystal diffraction patterns was indicated by Nagy and Chung Kwai Lui in 1947. This solution is based on the use of the sense of sight, and is undoubtedly elegant. The data published by Nagy and Chung Kwai Lui was, however, limited to one composition of manganese-activated zinc-beryllium-silicate and to furnacing conditions corresponding to increase of temperature of 13°C. per minute, and much further work in this field would have to be done before it could be regarded as covering the subject of zinc-beryllium-silicates in a completely adequate manner. Long before 1947, however, a technique had been evolved for determination of the optimum thermal treatment corresponding to various proportions of ingredients in manganese-activated zinc-beryllium-silicates, which automatically determined the fluorescence colour optimum for each composition. The reason why this technique, which had been used successfully in practice by more than one investigator, did not receive any prominence in scientific publications, is because it was based on the use, as a guiding sense, of the sense of touch, together with the muscular sense, and was, through convention of habit, regarded as "unscientific". This technique consisted in determining the optimum thermal treatment by the mechanical qualities

of the freshly prepared phosphor, as measured by the manual pressure necessary to crush it in a mortar, the ease with which it could be ground by hand, and the "feel" of the powder thus produced. The results were completely reproducible, not only by the same but by different investigators.

The author has been challenged as to the scientific value of the above method, on the grounds that although it might have been used with perfect success in a number of separate organisations, experience of touch, or touch combined with muscular sense, could not be recorded in units which would have the same meaning to everybody and could not therefore be accurately reproduced when and where desired. The above objection is not well-founded. There is no reason why certain arbitrary, reproducible specimens of materials should not be selected as reference standards for definition of touch. After all, a foot, a pound, and a candle, suitable as they may seem, were in the first place arbitrarily chosen.

The sense of hearing differs from the three senses just considered principally in that it furnishes information characteristic not of the substance immediately in contact with the organ of perception, but of a distant object. The music of a violin, the song of a bird, the clang of a church bell, are characteristic of the violin, the bird and the bell respectively, and not of the air in contact with the eardrum, though it is vibrations of the air which convey the sound. The echo from a mountain is characteristic of the mountain and its spacial relationship to the observer, and not of the intervening air. The ear is capable of determining qualitatively the intensity of an audible note, that is, the energy of vibration at the particular frequencies, of perceiving changes of that intensity, and of determining with a high degree of accuracy when the energies of two acoustic vibrations of the same audible frequency are equal. It is not however capable of comparing energies of notes of different frequencies. It is capable of determining with considerable accuracy the precise frequency of an audible note, as well as determining changes of frequency. Two ears in combination give an indication of the direction of sound. The value of unaided hearing to research is relatively small outside acoustics and the power it gives to communicate with others by means of speech. Even so, however, it can furnish valuable

information on matters as diverse as animal behaviour and the sound of a petrol engine. Its most interesting possibilities in scientific research have been revealed by progress in three directions—one, the conversion of alternating electric current into audible acoustic vibration, as in the case of the telephone and resistance bridge method of measuring the resistivity of electrolytes; another, the conversion of audible vibrations into electrical vibrations and visual recording, as in the case of investigation of sea bed contours by the echo method; and the third, the use of ultrasonics. The last is probably the most promising development arising out of the study of sound waves, representing as it does a new field of experimental technique—but it cannot be regarded as any longer connected with the sense of hearing, since ultrasonic waves are inaudible.

The sense of sight has been firmly established as by far the most important sense in scientific observations. Partly this has been due to the fact that it is the only sense capable of giving an investigator information concerning objects which he cannot actually contact with any part of his body, irrespective of whether such objects are near or extremely far and whether they are separated from the investigator by a material medium capable of transmitting electromagnetic radiation, or by a vast expanse of empty space. Partly this has been due to great diversity of capabilities of the unaided human eye. A normal human eye has the power to determine the position and change of position of objects in space, though not necessarily their distance from the observer, their shape, and, within the limits of its resolving powers, which are very high, the details of their external structure. It has powers of determining qualitatively the intensity of visible radiation, of determining precisely the condition of equality of illumination for the same colour, of estimating, though less precisely, equality of illumination of different colours, of detecting variations of brightness in space and in time. It has the power of distinguishing colours and of matching them with a high degree of precision. It has the power of adapting itself to near or distant vision. It has the power of adapting itself to very high and very low levels of illumination. Yet another reason why the sense of vision has such a dominant rôle in scientific observation is that in nature a vast majority of bodies, from stars to atoms, have the property of either

emitting or reflecting some electromagnetic radiation within the visible spectrum. Bodies which do not emit or reflect visible radiation have been found to emit or reflect electromagnetic radiation in the infra-red or ultra-violet regions of the spectrum, which radiations have been successfully translated by various ingenious devices, like the fluorescent screen or the photographic plate, into images visible to the eye. Phenomena not belonging to the sphere of electromagnetic radiation, and not directly observable by the senses, have been ingeniously translated into visual images—the electric current pulsations into a luminous curve on an oscilloscope screen, the temperature changes on a recording pyrometer, the earth tremors on a seismograph. Such phenomena could not have been translated into sound, and still less into touch, without making the observations far less informative.

It may be said that a blind man might be a great musician, poet, or philosopher, but could never succeed in scientific research, except that entirely limited to mathematics, where his sense of touch might suffice him. It is justifiable to say, therefore, that every investigator in planning his research must aim at making all phenomena visible, if this be at all possible, and visible not only in a general way but in the greatest possible detail, not only from time to time but as much as possible at every instant. But such a sweeping recommendation must carry with it a note of warning. The dominant rôle of vision in scientific observations does not make other senses unworthy of consideration. A man with perfect vision but deprived of the senses of hearing, taste, smell and touch would find scientific research very difficult indeed. Neither taste nor smell can be translated into visual images, and though sound vibrations can be photographed and the texture of a surface can be made visible, the translations of sound and touch into vision are still translations and not the originals, and in translation something is lost, though much may also be gained.

This brings us to the problem of the relationship between the phenomena investigated and the sense perceptions engendered by them. In scientific research this relationship must always be such that the sense perceptions give reliable information concerning the phenomena in question. The mere

fact that, by means of a suitable apparatus, one is able to obtain a series of sense impressions which are clear, reproducible and attractively simple, is not sufficient. It is also necessary that they should provide an undistorted and informative picture of the phenomena under investigation.

There is a twofold danger of error—the mechanism of translating the phenomena into sense perceptions may introduce effects characteristic of the mechanism and not of the phenomena, and the mechanism may be so indirect that the sense perceptions are characteristic not of the actual phenomena under investigation, but of other phenomena associated with the phenomena under investigation. A simple example of danger of errors of the first kind is provided by a mechanism for measuring light flux by means of a combination of a photocell, an amplifying unit and a milli- or micro-ammeter. The sensitivity of any uncompensated cell is very different from that of the human eye, and unless suitable filters or more complex corrections are used, errors may be very high. Again, the response of the cell is dependent on its temperature, and this in turn is dependent not merely upon the temperature of the surroundings, but also upon light intensity. Therefore accuracy demands a thermostatic control of cell temperature. It is also essential to take care that the amplification of the amplifier unit is constant over the entire range of observations. Unless all these precautions are observed, the meter reading may be hopelessly misleading. A simple example of danger of errors of the second kind is provided by the recording pyrometer of a furnace as a means of determining the temperature—time variations of a substance under thermal treatment. The chief error here is that the pyrometer, even if absolutely accurate, merely indicates a thermoelectric current dependent on the temperature in a particular region of the furnace. The substance under investigation may be undergoing important exothermic and endothermic reactions which make its temperature at given instants of time materially different from that of the surroundings which the thermocouple explores.

The only way in which the investigator can hope to avoid such errors is by considering the possibility of every kind of error which he can imagine may be present, and then making sure either by study of scientific publications, or, if there be

no relevant or reliable publications available, by experiment, that such errors are naturally absent in the particular case, or properly corrected, or are not of sufficient magnitude to affect his results to any appreciable extent.

There is another source of danger. The sense perceptions which the phenomena under investigation may produce with the aid of certain mechanisms may be quite correct in so far as they are free from errors due to the mechanism adopted, and are actually engendered by the phenomena under investigation and not by another phenomena, *but they may be inadequate. They may only convey a part of the story.* An example of this is provided by the recording of the electrical impulses in nerves associated with the sensation of sight. However accurately they might be recorded, they can take no cognisance of the function of visual purple in the phenomena of vision.

The temptation to succumb to the last mentioned danger is unfortunately very great. There is always a desire to extract from observations a complete solution of the problem, and when the observations do definitely reveal a large part of the solution, the impulses to interpret them in a manner which appears to make them quite adequate is both natural and strong. The investigator must guard against this danger, and may do so more effectively if he decides *beforehand,* before his experiments have been performed, whether the observations are *capable* of giving a complete answer, assuming the experiments prove successful.

It is, in general, desirable to choose a method of investigation which makes the relationship between the sense perceptions and the phenomena under investigation which engender them as *direct* as possible. If, for example, it is equally practicable to measure the temperature of a body by a thermocouple or by an optical pyrometer, it is better to use a thermocouple, because the use of an optical pyrometer involves assumptions regarding the radiation emitted by the body at the particular temperature, which may be quite incorrect if the body's behaviour is, at that temperature, materially different from that of a black body.

At the same time it is most important to guard against the possibility of *the mode of observation exercising a modifying influence upon the phenomena under investigation.* For example, exploring

electrodes have been recognised for some time as a means of investigating the potential distribution in an electric discharge. The author found, however, that when he applied this method to an arc discharge between a tungsten anode and a tungsten-thoria cathode in an atmosphere of nitrogen at 5–12 in. mercury pressure, exploring electrodes attained a temperature at which they became sources of electron emission, and the conditions in the arc region therefore became different from those which appertained in the absence of the exploring electrodes.

Yet another matter in which an investigator must exercise great caution is that of inference that because a certain mechanism makes it possible for a particular phenomenon to be accurately translated into an effect perceived by one of the senses, therefore any variation in this perceived effect *must* represent the same change in the phenomenon investigated as if the effect were a continuous function of the phenomenon over the entire region of the investigation. Such an inference is frequently correct. But it is not invariably correct. An accurate photographic record of sound waves can be produced, and the sound can be reproduced with complete accuracy from such a photographic record. But it does not follow that because the photographic records of two sounds having the same fundamental frequency are different, therefore the two sounds must also appear different to the ear. The difference in the two photographic records might correspond to inaudible harmonics, or to harmonics within the audible region but with insufficient energy to produce an audible difference.

Passing to the problem of amplification of the sense stimulus, we find that this may be of three kinds: the amplification of the sense stimulus within the regions in which perception by the unaided sense is possible, the extension of the region of the sense stimulus beyond the limits within which perception by the unaided sense is possible, and the amplification of the difference between stimuli. An example of the first kind of amplification is provided by a microscope using radiation within the visible region of the spectrum; an example of the second kind is provided by a photographic apparatus making use of ultra-violet radiation; an example of combination of the first and second kind of amplification is ultra-violet

microscopy; an example of the third kind of amplification is the method of beats of comparing the vibrations of unknown frequency with those of known frequency.

The only senses for which stimulus amplification has become an established technique are hearing and vision. In the case of hearing, amplification of the first kind, that is, amplification of the stimulus within the region in which perception by the unaided sense is possible, has been widely practised. Amplification of the second kind, that is making ultrasonics audible, although quite attainable, has not been seriously pursued, because it has been held that the translation of ultrasonics into visual images was a preferable solution. Amplification of the third kind, that is amplification of differences between acoustic vibrations, has been used with conspicuous success, particularly for determination of frequencies. In the case of vision, all these kinds of amplifications have been practised with a very high degree of success.

Amplification of audible vibrations has been of two types—direct, depending on resonance, and indirect, in which the audible vibrations are first translated into electrical waves which are amplified, and after such amplification translated back into audible vibrations. Indirect amplification has been developed to a high degree of perfection, and may now be regarded as superior to the direct amplification in almost all circumstances. The problem of making ultrasonics audible involves, for its satisfactory solution, the acoustic equivalents of the photographic plate and the fluorescent screen. The acoustic equivalent of the fluorescent screen has not been found, but a perfect acoustic equivalent of the photographic plate exists in the gramophone record. It is only necessary to record ultrasonics on a gramophone record, and then run the record on a gramophone at a fraction of the recording speed, to reduce the frequencies of the recorded vibrations proportionately and thus bring them within the audible range. Unfortunately, the analogy between the gramophone record and the photographic plate ceases at this point. It is possible to examine the whole photographic plate, and even a large number of such plates, *simultaneously*. A gramophone record, on the other hand, can only be heard while in motion, so that the investigator can obtain only a *succession* of audible effects.

This makes the conversion of ultrasonics into audible vibrations far less satisfying than their conversion into visual images, which can be recorded and examined as and when desired. Amplification of differences between acoustic vibrations has enabled a great increase of accuracy in quantitative study of such vibrations. Thus, an experienced observer can determine when an audible note corresponds to 1,000 cycles per second, by direct observation, to within 0·5%. If, however, the audible vibrations whose frequency has been thus estimated are heard simultaneously with audible vibrations of a tuning fork of similar and accurately determined frequency, the beats produced enable a determination of the unknown frequency to within 0·01% without difficulty.

In the case of vision, direct amplification of the stimulus within the visible region of the spectrum has been achieved with great success, for distant objects by the telescope, and for near objects by the microscope. In both cases the function of the apparatus is that of presenting to the eye an enlarged image of adequate brightness. In the case of the telescope the degree of magnification has been limited by the practical difficulties of making lenses and mirrors of increasing size. With the microscope, a much more serious limitation was encountered. This limitation was of a theoretically insurmountable kind—the fact that resolution is proportional to wavelength of radiation employed. This has made a magnification of 1,000 about the highest useful magnification of a microscope designed for visible radiation. Much smaller objects can be observed by the Siedentopf and Zsigmondy ultra-miscroscope, but such objects are only made visible by the light they scatter; that is to say, they are not seen, but merely detected. Indirect amplification of the stimulus within the visible region of the spectrum has been achieved most successfully in the case of measurement of light energy by combinations of photocells and valve amplifiers and by the use of electron multipliers. In this case there is, of course, no limitation imposed upon the degree of useful magnification by the wavelength of light used.

Amplification of visual stimulus by extension of the region of the sense stimulus beyond the limits of wavelengths within which perception by the unaided eye is possible has been achieved over a remarkably wide range, extending from infinite

wavelengths (measurement of direct current) down to ·005 ÅU (hard X-rays). There is no reason to suppose that any limiting frequency exists beyond which such amplification would be impossible. The principal means of this kind of amplification are the infra-red and ultra-violet responsive photocells, the thermocouple, the fluorescent screen, the photographic plate, the cathode ray tube, various types of meters in which a deflection of a moving part is produced by an electrostatic or an electromagnetic force, the Wilson cloud chamber type of apparatus, and the electroscope. Within the same type of amplification may be properly included all cases where the effect translated into a visual stimulus is not an electromagnetic vibration. This inclusion brings into the sphere of the particular type of amplification measurements of temperature, where thermal energy is caused to operate a thermometer or, through the medium of a thermocouple, a pointer indicator instrument; measurements of gas pressure translated into visual effect on a McLeod gauge; visual effects corresponding to various properties of materials, such as elasticity, viscosity and surface tension; measurements of weight, when gravitational force causes a movement of the balance pointer or deflection of a reflected beam of light.

Combinations of the first and second kind of amplification have been developed with great success. Examples of this are the ultra-violet microscopes operating with illumination of 2748·58 ÅU from a cadmium spark, and the electron microscope.[1] Other examples are oscillographic recording of small electrical impulses and valve amplification of photo-electric currents due to infra-red or ultra-violet radiation.

The third kind of amplification, the amplification of differences between stimuli, covers a very wide field in a highly effective manner. Photometry, based on comparison of brightness of similar surfaces, which in the case of an experienced observer can determine equality of brightness to within 0·5%, is a simple example. Colorimetry and spectroscopy enabling differentiation between wavelengths of light are examples of more complex, and also more effective amplification of differences. The degree of precision reached here is illustrated

[1] Philips have produced an electron microscope with a variable useful magnification of from 1,000 to 50,000.

by the fact that an experienced observer can detect, without use of a spectroscope and by comparison with suitable colour standards only, a wavelength difference in the yellow part of the spectrum not exceeding 0·5 to 1·0 ÅU. Optical rotation, which escapes detection by the unaided eye, has been accurately determined by the polarimeter and small differences of optical rotation can be accurately determined. Diffraction has enabled differentiation between X-ray radiation of different wavelengths and their accurate measurement down to the wavelength of 0·005 ÅU.

With such vast possibilities open, the investigator, in choosing the method of investigation, must aim not only at making the phenomena under investigation visible and quantitatively determinable by the visual method, but *at amplifying the visual stimulus in various ways so as to extract the most detailed information possible within the limits imposed by the resources and time available. It is a great mistake to suppose that any details so obtained might be valueless.* It is true that for certain purposes some of the detailed information may be omitted without the research being thereby rendered unsuccessful, and considerations of economy of resources and time may dictate such omissions, but whenever such detailed information is included, it not only adds to knowledge and understanding of the phenomena, but frequently reveals unexpected facts of highest importance to the investigator. On the other hand, no investigator has unlimited resources and time for any problem. It is far more likely that both the resources and the time available are less, often much less, than a thorough investigation of the problem demands. The decision which the investigator must therefore make, in his initial planning, is *what detailed information he dare omit, and what information must be obtained to make success possible.* This decision frequently requires a great deal of thought, and taxes the ability, foresight and ingenuity of an experienced senior scientist. It is beyond the ability of a junior scientist who lacks experience necessary for such decisions. The safest policy is to plan the work on simplest, minimum essential information lines, but to make provision for more detailed investigations, should these become necessary, or should resources and time permit these later, by acquiring certain additional apparatus and materials for the purpose. For example, a scientist engaged

in metallurgical research *must* have a metallurgical microscope, pyrometers for measuring the temperature of his furnaces, and apparatus for testing the mechanical (possibly also the electrical and thermal) properties of his samples. He may be able to succeed in his research without the use of X-rays to determine the crystal structure of his samples—*and he may not.* If he *can* afford an X-ray apparatus, he would be acting prudently if he obtained it or made all necessary provisions for obtaining it at short notice, even if he decided that he would *probably not use it*: it is quite likely that at some stage or another of the work the X-ray photographs might make all the difference to the progress of the work, though a continuous use of X-rays might be unnecessary. Unfortunately a scientist will frequently find that those providing the resources, themselves not scientists but business men, permanent officials, and so on, almost invariably fail to understand why anyone should be interested in an apparatus or a method unless he is going to make liberal use of it. A scientist who tells such people that he should like a particular piece of apparatus which he believes he may not have to use, appears to them either as an extravagant maniac or a muddle-headed fool. However, we shall return to this aspect of the situation later.

The importance of precautions in choice of method and their application from point of view of certain possible errors and the importance of bearing in mind that, valuable as the sense of sight is, there are other senses which must not be disregarded, have already been mentioned. One more important factor, however, must be considered. This is the problem of so devising the method of investigation that observations may be made at any instant and over any desired time-interval. The point needs some explanation. It is unusual for a method of observation to be perfect, so that any desired information can be obtained at any and every instant throughout the whole range of investigations. In cases where this is possible, the investigation is usually of a very simple type. Far more frequently observations can only be made during certain stages of investigation, the intervening periods being covered by inferences as to what takes place. So long as everything proceeds as expected, this is not a serious handicap. If, however, something unexpected occurs, if things do not go according to plan, the

question of what actually took place during certain periods when observations were not made, or did not cover the particular effects which need interpretation, becomes extremely important. Frequently investigators attempt to fill the gap in their knowledge by speculation. This is seldom satisfactory. Speculations must be substantiated by experimental proof, which involves time and effort, and if the evidence of experiment proves the speculation to have been unsound, one has to start speculating and experimenting all over again. This process may have to be repeated a number of times and may end in complete failure. If only one can find means of observing what actually does take place during the unexplored interval of time, certain knowledge replaces blind groping.

Perhaps the best way of explaining the point is by an actual example. A good many years ago the author had to produce a very robust electron emissive filament for a discharge device. The filament had to be capable of withstanding intense ion bombardment and excessive temperature of operation without permanent injury. It was decided that a tungsten filament coated with a relatively thick layer of thoria should answer the purpose. A quantity of 0·00225 in. diameter tungsten filament was coated electrolytically with ThO_2, using a saturated alcoholic solution of $Th(NO_3)_4$ as electrolyte, the filament as cathode, a carbon anode and a porous pot as separator of the anode and cathode regions. A uniform hard coating of ThO_2 was obtained. A number of diode valves were made up with lengths of coated filament as cathodes and nickel plates as anodes. The exhaust process included the baking out of the glass envelope, outgasing the filament at a high temperature for some time, and outgasing the anode by electron bombardment. The filaments were then tested for electron emissivity. The results were erratic. On breaking open the valves and examining the filaments under a microscope it was found that the emissive coating had an irregular, cracked appearance and large parts of it were entirely missing. Why did the initially uniform coating break up during exhaust? Was this due to heating the filaments, to ionic bombardment, or to some fault in coating structure? Coated filaments were examined microscopically before being made up into valves. The coatings were found to have a crystalline appearance. A coating

0·006 in. thick was found to consist of crystals of max. size 0·0013 in. and average size 0·0004 in. By varying the current density during electrolysis, conditions were obtained when a coating 0·0013 in. thick, consisting of crystals with a max. size of 0·0009 in., and an average size of 0·0002 in., was obtained. This was better, but not good enough. By altering the electrolyte to a mixture of 80% saturated alcoholic solution of $Th(NO_3)_4$ and 20% saturated alcoholic solution NaCl, coatings were obtained with a maximum crystal size of only 0·0002 in. It was assumed that such coatings would not break up on heating or under electron bombardment. But when the coated filaments were mounted in diodes and the original test repeated, so was the original failure.

At this stage there were three courses of action open: (*a*) to accept defeat and abandon the work; (*b*) to engage in speculations as to the causes of failure and proceed to verify each hypothesis in turn in the hope that one of them might prove right and that the discovery of the cause might also point a way to remedy; (*c*) *to devise a method of observing exactly how the breaking-up of the coating took place.* The third course was adopted. A length of coated filament was mounted in an evacuated bulb and observed through a telemicroscope while an increasing current, passed through it, gradually raised it to the maximum temperature to which it would have been raised during diode exhaust. It was then observed that, as the filament temperature rose, the coating was rent by the expanding metallic core. The breaking up of the coating was due to the fact that it was a rigid body having a lower coefficient of expansion than the core surrounded by it. The remedy was now obvious. Tungsten filament 0·00220 in. diameter was electrolytically coated with copper to a diameter of 0·00293 in. The copper-coated filament was electrolytically coated with ThO_2 to a diameter of 0·00470 in., using the electrolyte and current density corresponding to maximum crystal size of 0·0002 in. Finally the copper coating was removed by making the filament an anode in a bath of aqueous solution of potassium sulphate; this left the ThO_2 coating intact in the form of a loose sleeve, 0·00470 in. external diameter and 0·00293 in internal diameter, upon a tungsten core 0·00220 in. diameter. To prevent the delicate thorium oxide sleeve being damaged in

handling, it was impregnated with a solution of nitrocellulose in amyl acetate. The coated filaments were again tested for electron emissivity in the previously described manner, and this time the result was highly satisfactory. Not only were the results consistent and reproducible, and the electron emissivity equal to that of tungsten completely covered with a unimolecular film of thorium, but the filaments had all the robustness which it had been hoped to attain. Electron emissivity was unimpaired either by ionic bombardment or overheating of the filament and was unaffected even if the filament was operated for long intervals at three times the normal energising voltage.

CHAPTER VI

THE PLANNING OF RESEARCH

PART II

THE problem of devising the method of investigation so as to eliminate all undesirable factors likely to introduce serious errors, to mask the effects which the investigator seeks to study, and in the case of Research Type 2 or Research Type 4 make the research an obvious complete failure, is frequently a very difficult one. An inexperienced young scientist frequently imagines that the elimination of these undesirable factors is synonymous with using materials of the highest possible purity, weighing and measuring everything with greatest precision, making a very large number of observations, and repeating the experiments a number of times. If this recipe for elimination of undesirable factors were adequate, the problem would be a delightfully simple one: all that would be necessary would be for the scientist to exercise care, be diligent, and use accurate instruments and very pure materials. Unfortunately the recipe is hopelessly inadequate. It gives no guarantee whatever that the disturbing factors will be eliminated, or even reduced, and frequently does no more than to increase expenses and effort and give the investigator an impression of reliability of the results, which in the case of Research Types 2 or 4 is rudely refuted by the final failure. Perhaps the simplest proof of the foregoing is to be found in chemical analysis. The author recalls an instance where a conscientious young research chemist had to analyse a mixture of compounds X and Y, of which X included elements A, B, C, D and E, and Y elements E, F, G and possibly H. The work was carried out with great care, and the proportions of constituents were confirmed repeatedly, and always added up nicely to about 99·5%. But owing to an error in method, a portion of A appeared as F. The results were therefore hopelessly wrong, and it was only

because certain other data showed that the quantity of F was incompatible with the quantity of G, that the error was discovered. Had the young chemist used a correct method, with much less labour and precision, he would have obtained proportions perhaps adding only to 99%, but he would have been far nearer the correct result.

The undesirable factors which have to be eliminated, or at least minimised or compensated, may be broadly divided into eight principal types, and undesirable factors of several types may be encountered in the same piece of research. The types into which these factors may be classified are:

1. Predictable undesirable factors present in the method of investigation but not in the phenomena investigated, as for example in the case of a method of analysis which is not suitable for the substances analysed.

2. Unpredictable undesirable factors present in the method of investigation but not in the phenomena investigated, as for example in the case of a synthesis in which the chemical purity of the constituents has been checked, but no notice has been taken of their physical condition, which may have an important influence on the results of the particular synthesis.

3. Interfering external factors, such as temperature and humidity of the room, presence of dust or of micro-organisms.

4. Natural variability of specimens under investigation, owing to causes other than those within the scope of the particular investigation, such as is encountered in biological research.

5. Presence of minute quantities of interfering substances, whose presence can be ascertained only when the research has been in progress for some time, and which are capable of producing a profoundly disturbing effect; for example, the presence of traces of "poisoning" elements in thermionic emission investigations, or of unsuspected virus in a biological experiment.

6. Observable random variability of experimental factors which are meant to be controlled factors, such as variations of high frequency induction heating due to variations of supply frequency.

7. Known large masking effects superimposed on the

phenomena under investigation, such as the masking effect of visible radiation of the mercury arc on feeble fluorescence of a substance excited by ultra-violet radiation of the arc.

8. Unknown large masking effects superimposed on the phenomena under investigation, such as would be encountered, for example, in the case of study of effects of a relatively mild drug upon blood pressure, when the conditions of administration might provoke anger or fear in the subject, thereby resulting in a release of large quantities of adrenaline.

The solutions of the problems presented by difficulties of the above types are dependent on the types to which the difficulties belong.

In the case of predictable undesirable factors of Type 1, the remedy lies in foreseeing the undesirable factors and eliminating them by suitable modifications of method based upon already existing knowledge.

In the case of unpredictable undesirable factors of Type 2, the remedy is much more difficult to apply, because the undesirable factors frequently cannot be foreseen at the commencement of the work. Undesirable factors of this type occur mostly in Research Types 2 or 4 and the wisest course is to guard against them by preliminary speculation as to their possibilities, followed by preliminary experiments, already mentioned, before the method is fully planned. The preliminary experiments should serve to indicate whether the possible undesirable factors do in fact exist in the particular case, and if they do, to indicate the necessary precautions. This solution is by no means as easy as it may sound. In the first place, it depends for its success on the soundness of preliminary speculations, and it is possible to think of a number of undesirable factors that are subsequently proved by preliminary experiments to be non-existent without actually thinking of the one undesirable factor that has to be guarded against. Real scientific insight and imagination are necessary here, and discussions are likely to prove most helpful. Secondly, it is, as already mentioned, by no means invariably possible to make preliminary experiments. Nevertheless, the recommendation given above is well worth following, because the chances are that the undesirable factors

will be revealed thereby before the real research is commenced, and it is far better to attempt to achieve this even when there is not a 100% chance of succeeding, than to go on without taking this precaution until the undesirable factors reveal themselves half-way through the research programme.

Interfering external factors can sometimes be foreseen, and sometimes do not become apparent until the research has been in progress for some time. In any event, their existence or non-existence cannot be determined by practicable preliminary experiments. On the other hand, once their existence has been established, the remedy is usually not difficult to apply, and the amount of harm done before the remedy is applied is seldom serious. Probably the best course in this case is to do nothing about the matter initially, but to be vigilant as regards possible emergence of these interfering factors.

Natural variability of specimens can always be foreseen. The remedy here is not the removal of the variability, or an increase of the number of observations to some colossal quantity, but the devising of a method of taking representative samples and the use of statistical method of correction. These points will be considered more fully in another chapter.

Presence of minute quantities of interfering substances can be observed only when the research has been in progress for some time, but the possibility of emergence of this type of interference can frequently be foreseen before the research is commenced. If preliminary considerations show that such interference might emerge later, steps must be taken to eliminate it, even if the steps are in the nature of tedious and excessive precautions. It is better to be safe than sorry.

Observable random variability of experimental factors which are meant to be controlled factors can sometimes be foreseen, but certainly not always. The best remedy is to devise the method of research so that a random variation can always be observed at once, and the experiment can be interrupted without ill-effect until the random variation subsides. If this remedy is impracticable, for example, if the particular experiments cannot be interrupted, or if the random variation can cause harm before the interruption can be effected, the only course is to eliminate the random variations; unfortunately this solution is generally difficult and expensive.

Known large masking effects superimposed on the phenomena under investigation can be foreseen when the research is planned, and must be eliminated. Generally such elimination is not difficult. For instance, in the case of the specific example of the particular type of interference mentioned above, all that is necessary is to employ a suitable filter for the arc radiation, transmitting ultra-violet but cutting off the visible part of the spectrum.

Unknown large masking effects superimposed on the phenomena under investigation are probably the worst types of interference. They may escape notice, in which case the results of the research will be hopelessly wrong. If they are noticed, it does not follow that the cause will be discovered. Even if the cause is discovered, it does not follow that it could be eliminated without radically altering the entire scheme of research. Probably the only useful suggestion that can be given concerning this type of interference is that if the results obtained in the course of earlier part of research appear to be totally different from what might be expected, the presence of an unknown masking effect should be suspected, and an attempt made to confirm the suspicion, and also to find a remedy, by carrying out some experiments involving a radical departure from the original plan.

The problem of production and control of desired factors is a very wide one, and it is impossible to deal with it adequately within the scope of this book. Nevertheless, its brief presentation under a number of sub-headings may be useful, though such a simplification of the subject cannot lay any pretence to completeness. The following sub-headings are perhaps the most suitable ones for covering the main aspects of the problem:

1. *Location of research.* This may be extremely important. For example, if the research is in astro-physics, it is essential that the site of observation should be so chosen that clouds and dust do not prevent continuous work; if the research is botanical, it may be advantageous to carry it out either in a place where climatic conditions are suitable or where it is practicable to obtain ample cheap power for glasshouse heating, and, perhaps, illumination; if the research is of a chemical nature, it may be desirable to have it situated in or

near a large industrial town so that various chemicals and apparatus required at different stages of research might be readily obtainable.

2. *Provision of general facilities and equipment other than actual apparatus.* This must be adequate, and include a variety of items, from well-lighted and sufficiently roomy premises for chemical or physical research to heated suits and oxygen equipment for investigators working at very high altitudes, and to provision of food and water for investigators working in regions where such things cannot be readily obtained.

3. *Means of producing and maintaining desired artificial conditions.* Under this sub-heading come such things as means of obtaining high or low temperatures, high pressures, vacuum, gaseous atmospheres of various compositions, and special thermal, electric or acoustic insulation.

4. *Means of selecting or producing desired specimens.* These include selection of the *types* of specimens, as in the case of Morgan's use of *Drosophila*; selection of *natural individual* specimens, as in the case of quartz crystals; production of *natural group* specimens, as in the case of breeding of pure lines in wheat; selection of *artificial individual* specimens, as in the case of triode valves selected from a group of triodes of supposedly identical construction on the basis of electron emissivity of the filaments and the grid backlash current; production of *artificial group* specimens, as in the case of phosphors of a particular type with one or more variants.

5. *Means of varying artificial conditions in a desired manner.* These are exemplified by such devices as rheostats, variable inductances and capacities, needle valves for control of gas flow and variable gears.

6. *Means of maintaining the constancy of artificial conditions.* These are represented by such devices as thermostats, constant voltage devices, and photocell control of illumination.

7. *Means of locating objects in space and of manipulating them.* Under this sub-heading come such devices as variable clamps, magnetic chucks, centrifuges, microscope stages with centring, rotary, lateral and transverse movements, and micro-manipulation.

8. *Means of constructing special devices* These include such diverse apparatus as precision lathes, glass working equipment, vacuum pumps and lens-grinding machinery.

It may be seen from the foregoing that the problem of production and control of desired factors is both a question of method and of apparatus. Before saying anything more about the eight sub-headings of this problem, it may be advantageous, therefore, to say something about apparatus in general.

The choice of apparatus for any piece of research is not wholly dependent on the outlook of the scientist, but (always subject to the all-important consideration of available resources) is largely determined by the nature of the problem and the existing state of the particular science. If the particular science is one still in the earlier stages of development, and the particular problem is so novel that qualitative rather than quantitative information is the objective, very little apparatus is required, and that generally of the simplest kind. Psychology is a case in point: many experiments require nothing more than a room, pencil and paper, and a stop watch. On the other hand, when the particular science is relatively old and has reached the stage of being an exact science, and the particular problem is the heir of many previously solved ones, in consequence of which exact quantitative information is alone of interest, a great deal of highly specialised apparatus may be essential.

Simplicity of apparatus is a relative conception. What may appear simple, and even elementary, by modern standards, would have been complex and indeed unattainable in the days of Faraday, and Faraday's simplicity was far beyond anything the most elaborately equipped scientist could hope to possess in the days of Archimedes. Nevertheless, this does not rob the expression "simplicity of apparatus" of all meaning. It is justifiable to define as "simple" apparatus which to-day might be within the scope of meagre financial resources, or might be readily made by hand with ingenuity and a little skill. To a modern scientist endowed with a measure of Faraday's genius, simplicity of apparatus and great discoveries can be altogether compatible. Such an achievement is actually easier when new ground, perhaps a new branch of science, is being opened up, than when one more addition is being made to a structure of detailed, co-ordinated knowledge. On the other hand, great scientific discoveries may be achieved by modern scientists who

do not exhibit any marked talent for reducing their apparatus to very simple forms. The virtue of simplification of apparatus is not that of facilitating great scientific discoveries, but of permitting the successful tackling of scientific problems too novel and too differentiated from existing scientific knowledge to attract substantial financial support for the investigator, and the compulsion exerted thereby upon the investigator to think clearly. But though the virtue of simplification of apparatus is real, it can come, like any other virtue, only from within; that is to say, it can be achieved by the scientist concerned only as a result of personal choice, ability and consideration —or not at all. It cannot be imposed upon the scientist from without by businessmen, officials, and similar gentlemen, no matter how important their status—all they have the power to do is to deprive him of resources.

The selection of apparatus cannot be entirely dependent on the scientist making the choice, because, as already mentioned, it is largely determined by the degree of development of the particular branch of science involved. For example, if investigations of thermionic or photoelectric phenomena are contemplated, and if such investigations are not to be limited to mere tests of already existing thermionic or photoelectric devices, the investigator must be in a position to construct his own thermionic devices—possibly variants of the high vacuum radio valve, possibly more specialised devices for investigation of secondary electron emission, possibly metallic vapour devices—or his own photoelectric cells. He must therefore have at his disposal not only sources of electric current, both direct and alternating, of various voltages, not only voltage and current measuring meters, resistances, inductances and condensers, but also a glassblower's bench with its gas and compressed air supply, and, if need be, oxygen, a complete pumping equipment with its oven, its rotary oil pump and its diffusion pump, its gauges, its high frequency furnace, its high voltage bombarder equipment and its filament outgassing equipment, a spot welder, a furnace for gas or vacuum treatment of metallic parts, a selection of hand tools and small presses, a grid winder and welder, and an oven suitable for glass annealing. He is certain to have to do some chemical work, and must therefore have at his disposal a chemical

bench, a fume cupboard, a water bath, diverse bunsen burners, retort stands, chemical ware, refractories, at least one gas or electric muffle furnace, a balance, a pump for filtering, a drying oven, a motor-driven stirrer and a ball mill. He must have an optical pyrometer, a thermocouple pyrometer, thermometers and thermostats. He is sure to need a microscope with accessories for measurement and photography of specimens, and a dark room for developing the photographs and making certain tests. All these necessities are not of his choice, but are imposed upon him by the requirements of the existing technique of the particular branch of science. Forty years ago investigations in the same field would have involved much lower minimum apparatus requirements.

Apart from the requisite minimum of apparatus characteristic of the particular branch of science, the investigator may find himself in need of certain special apparatus characteristic of the particular problem with which he is concerned. Such special apparatus might be purchasable, or might have to be made specially for his purpose. If it is purchasable, all the investigator need do is to make sure that it is really required. Young scientists, lacking experience which time alone can bring, are apt to be charmed by ingenious and elegant devices which are really useless to them. The author, in the course of his own experience, has encountered many such examples of improvidence, and, in his youth, furnished not a few of them personally. It is often difficult for a senior scientist to persuade a junior one that the delightful piece of apparatus the latter requests is really not worth getting. On the other hand, the junior scientist often has the trying experience of unyielding refusal of a special piece of apparatus which he has good reason to expect to be useful, solely because some earlier mistakes of his own, or possibly of other junior scientists, have set up a resistance to such requests in the mind of the person responsible for the decision. The only really satisfactory way is to deal with each such special apparatus purchase entirely on its own merits, with the scientists concerned submitting the matter to closest critical scrutiny.

The construction of special apparatus is an entirely different matter. Here it is entirely a question of provision of facilities for making things. A small workshop with a lathe or two, a

drilling machine, a cutting wheel, some metal- and wood-working tools, and a small forge, is not a luxury but an essential for any properly equipped laboratory. Indeed, if the laboratory is planned on an ambitious scale, the workshop facilities can well be expanded much further. If the subject of research does not involve glass-working as part of the investigations, as for example in biology, it is nevertheless desirable to be able to make up or repair glass apparatus, and for this reason a glass-blowing equipment should be added to the workshop. A well-equipped workshop can extend its activities to making up special electrical apparatus, such as time switches, and even optical apparatus. The more the workshop can cope with the problem of special devices, the more rapid is the progress of research likely to be.

An engineer, a physicist, or a chemist is not likely to question the utility of a good workshop. A biologist is more likely to imagine that a well-equipped laboratory, provided with incubators, drying ovens, refrigerators, microtome, centrifuges, microscope, thermometers, thermostats, and other devices for biological research, needs no workshop. Any repairs of apparatus, or any new devices, he may say, can be handled by outside organisations. And so they can—but the price in time lost will outweigh the cost of the workshop many times, for the time lost will frequently prove to be coincident with decisive experiments that cannot be interrupted for long without grave harm to the research in hand.

Let us now return to the problem of production of desired factors and examine more fully the questions of the means of production and maintenance of desired artificial conditions, of varying the artificial conditions in a desired manner, of maintaining the constancy of artificial conditions, of selecting or producing desired specimens, and of locating objects in space and of manipulating them. The first three of these are closely related, and it is possible to give a few warnings and make a few suggestions which are broadly applicable to all three.

One warning is against errors due to baseless inferences and against attempts to compensate such errors. It is obvious that a furnace devised to operate at a maximum temperature of 800° C. is unsuitable for obtaining a maximum temperature of

1,300° C. Many young scientists will imagine that a furnace capable of giving a maximum temperature of 1,300° C. is suitable for continuous operation at 700° C.—but, in the case of many furnaces, particularly of gas muffle furnaces, this is quite untrue. The young scientist, finding that his 1,300° C.-type furnace cannot be readily kept at a constant temperature of 700° C., or readily varied on a desired time basis between 650° and 750° C., is apt to seek for a remedy in the means of controlling the temperature so as to keep it constant at a desired value, or to vary it in a desired manner. The attempted solution, however, will prove costly and unsatisfactory. *An error in the choice of means of producing and maintaining desired artificial conditions cannot be compensated satisfactorily by changes in the means of varying these conditions in a desired manner, or in means of maintaining their constancy.* Similarly, a young scientist who has chosen an insufficiently responsive temperature control for his furnace must not attempt to compensate for the mistake by making the furnace more sluggish. This will hide the mistake, but will also make it more difficult to vary the temperature of the furnace with time in a desired manner. *An error in means of maintaining the constancy of artificial conditions cannot be compensated satisfactorily by changing the means of producing and maintaining these conditions. Similarly, an error in means of varying the artificial conditions in a desired manner cannot be compensated satisfactorily by altering something else.*

Another warning is against any assumption that means to achieve an effect can be completely defined without reference to the conditions of their application. This is a type of error which even an experienced scientist may make on occasions. The author recalls a case when such an error was made by an experienced chemist for whose knowledge and ability he has a very great regard. The chemist wished to maintain a specimen at a certain temperature in a thermostatically controlled bath. The thermostat was capable of controlling temperature to within very narrow limits, but the thermometer near the specimen showed much wider, and quite impermissible, temperature variations. On examining the apparatus, the author found that the thermostat was situated some way away from the electric heater element, and the size of the bath, and the characteristics of the mechanical stirrer, were such that

there was a temperature difference between the liquid of the bath near the thermostat and that near the electric heater. Consequently, when the thermostat switched off the current, the temperature of the liquid near the heater was already much too high, and the temperature of the liquid in the vicinity of the thermometer continued to rise until a uniform bath temperature was attained. The recognition of the source of the trouble made the remedy obvious.

An important point to have in mind is that means for producing an effect must always be selected with due regard to the properties of the specimen to be treated. Consider, for example, the question of heating a specimen. There are various heating means available, together with their appropriate means of temperature control. There is the gas muffle furnace, the gas furnace in which the flames or products of combustion are in contact with the specimen, the resistance type electric furnace, the high frequency furnace for heating metals, the radiant-heat furnace, heating of insulators by high frequency dielectric losses, and various other means. The choice cannot be determined by considerations of attainable temperature alone. If painted surfaces are to be dried, radiant heat is most suitable. If a body of plastic is to be heated, high frequency dielectric loss heating is best. If crucible loads of substances contaminable by flame or products of combustion must be heated, a gas muffle furnace may be best. For each type of specimen there is some particular optimum type of heating.

The selection and use of means of production and maintenance of desired artificial conditions, of maintaining their constancy and of varying them in a desired manner, cannot in general be determined satisfactorily solely on the basis of published results of previous investigators. The conditions of investigation are seldom identical; details of practical importance are frequently omitted, and except for cases where the previous investigator is an acknowledged master of the particular experimental technique, there is always the possibility that he did not use the best method, or use the method to best advantage. A sounder practice in making decisions regarding choice and use of means of producing and maintaining desired artificial conditions, of maintaining their constancy and of varying them in a desired manner, is to be

guided only to a limited extent by the published work of previous investigators, and to rely rather more upon personal experience, trustworthy information, published or verbal, concerning characteristics of various apparatus, and *the careful application of scientific first principles to the problems of choice and use.* The last mentioned point is perhaps the one that is really decisive, since first principles are usually the only ones that are applicable in dealing with a novel set of conditions, and since without proper use of these even a correctly chosen apparatus may prove quite inadequate, while if these principles are properly applied, both the choice of means and their application should be the most suitable ones.

Means of locating objects in space and of manipulating them all represent amplifications, or imitations and amplifications, of powers of the human hand, or of the human hand supplemented by a simple hand tool. The press amplifies the pressure a human hand can exert; the vice imitates and amplifies the firmness of the grasp; the lathe imitates and amplifies the speed and accuracy of rotary motion of which the hand is capable, and a screw-cutting lathe is an excellent example of increase of accuracy of the hand-manipulated cutting tool. The above examples represent amplification, or imitation and amplification, of the powers of the human hand corresponding to making the hand stronger, swifter, steadier. The advantages of such amplification of the powers of the hand have long been appreciated, and achievements in this field have been numerous and important. More recently, however, it has been realised that there are advantages in an entirely different type of amplification, or imitation and amplification—that equivalent to providing a miniature hand, with its movements reduced to a small fraction of those of the normal hand. This kind of amplification first appeared in the microscope, and has recently produced remarkable results in the technique of micro-manipulation, notably of micro-surgery.

Further developments of means of locating objects in space and of manipulating them are certainly possible. Certainly, micro-manipulation must be regarded as a highly promising new scientific development. It is interesting to note that so far there has been no attempt to develop an artificial equivalent of a miniature human hand not from the point of view of miniature

movement (which of course has been achieved) but from that of miniature force. There must be a wide experimental field in which amplification of delicacy of touch in manipulation is of great importance, and cannot be achieved satisfactorily by miniature movement alone, but only by a combination of miniature movement and miniature force. Investigators concerned with the problems of location of objects in space and of their manipulation should remember that they are all the time seeking to extend the powers of the human hand either directly, by amplification, or indirectly by providing a better substitute, and that this principle must never be abandoned. This warning and the preceding explanation are not superfluous, because in a number of cases, such as magnetic manipulation of objects in vacuum, manipulation of incandescent solids or liquids, and of radioactive materials, it is only too easy to forget that the human hand has anything to do with the case. As to the importance of the principle, it may perhaps be made more evident by a specific example. In the electric lamp industry this principle was partly recognised twenty years ago by one of the largest and most progressive companies, and this enabled the company to produce an automatic machine for a particular process which made use of the mechanical equivalents of half a dozen hands operating simultaneously. Recently the mechanism has been greatly improved. It now makes use of a mechanical equivalent not of half a dozen hands, which only mythical gods and goddesses can claim, but of *one pair* of hands.

Selection of desired specimens involves three main considerations: choice of specimens on the basis of their desired characteristics, choice of limits of permissible variation, and choice of grounds for rejection of specimens which satisfy the first two conditions. In the case of choice of *types* of specimens, only the first and third considerations are of importance; Morgan's choice of *Drosophila* was based upon the rapidity of reproduction of the particular species, its high prolificacy and its small number of chromosomes, and it was only recently that any important disadvantageous characteristic was encountered, namely the difficulty of embryological work. Selection of *individual* specimens, whether *natural or artificial*, involves all three considerations, the second consideration being often the most

difficult to formulate. This may be illustrated by the previously mentioned example of selection of triode valves. The desired characteristics in this particular case would be the ideal electrical characteristics—maximum electron emission theoretically obtainable and zero grid backlash current. The undesirable characteristics which would justify rejection would be constructional, such as displacement of electrodes and mechanical defects likely to produce premature failure. But the permissible limits of variation would not be nearly so obvious. Just how far short of the optimum would filament emissivity be tolerable? How much might the permissible grid backlash current exceed the ideal zero value?

There are two factors which fix the limits of permissible specimen variation—one is the percentage of specimens which may be rejected; the other, the requirements imposed by treatment to which selected specimens would, or might be, subsequently subjected. The two factors must give compatible specifications of permissible limits of specimen variation. If they do not, and the specimens are artificial, the method of production of the specimens must be reviewed to bring the two factors into conformity. If the specimens are natural, the problem is more difficult to solve: at best it can be solved by finding means of suitably modifying the treatment to which selected specimens would, or might be, subsequently subjected or, alternatively, of modifying means of obtaining the natural specimens which would make a higher percentage rejection permissible. If neither of the two above solutions of difficulties in the case of the particular types of natural specimens appear possible, the only solution left is to find natural specimens of a *more suitable type.* If this last solution is impracticable, the entire piece of research may have to be abandoned, or radically altered in scope.

Preparation of specimens is a wide subject, only a part of which comes within the scope of planning. In the case of artificial specimens, two important points must be legislated for when the plan of research is prepared. One point is that samples of prepared specimens *at various stages of preparation* must be preserved from the beginning. This is most important, not only from the point of view of ensuring reproducibility of satisfactory specimens, but also from the point of view of

ascertaining the precise conditions for obtaining the specimens desired. If this precaution is not observed, extremely important causes of variation, which may be due to presence or absence of minute traces of chemical impurities, differences of physical state, differences in processes, differences in mode of selection of unfinished specimens, may entirely escape notice. Classical examples where minute differences, at first unobserved, proved of decisive importance are the Welsbach gas mantle, and electron emissive tungsten filaments containing thoria. In cases where artificial specimens are not merely samples of material but devices, such as gaseous discharge tubes, changes in shape, size and disposition of components and the manner of their assembly are additional factors which may have unsuspected importance. In the case of artificial specimens consisting of assembly of devices, such as radio circuits, the original devices, their manner of mounting, their spatial relationship, and the precise details of their interlinking, may contain important factors, not immediately obvious, which may be absent in repeat specimens.

Another important point which must be considered in the planning of the preparation of artificial specimens is the suitability of alternative processes to be employed in specimen preparation. For example, in the case of separation of two mixed powders, chemical, magnetic, electrostatic, high frequency eddy current, specific gravity, air elutriation and liquid elutriation methods of separation are available, and it is a question of selecting the method most suited to the particular case. The author recalls when separation of two mixed powders of non-magnetic, non-conducting material was virtually impossible by any of these methods because the only important difference between the two powders was that one consisted of fairly uniform, roughly spherical particles, and the other of much larger, oblong, flattish particles. Elutriation, which at first appeared to be the likely method, proved unsatisfactory because the larger surface per unit volume of flattish particles compensated for their mass being greater than the mass of roughly spherical particles. In this particular case the author was able to obtain a satisfactory separation by suspending the powders in a liquid medium and shaking the suspension in a long glass cylinder the bottom of which consisted of a sintered

glass disc with a porosity adequate to permit the passage of the spherical particles, but not of the larger, flattish particles. *The success of the particular separation method was due to the fact that it was dependent for its efficiency on the differences which actually existed between the powders in this particular instance.*

Preparation of natural specimens belongs mainly to the domain of biology. In so far as it is concerned with breeding of plants or animals, it borders closely on selection of natural specimens and in this field planning of specimen preparation and specimen selection are interlocked. Preparation of natural specimens coming under such headings as tissue culture, bacteria culture and study of viruses, is primarily a provision of optimum conditions for certain forms of life and their prolification. Production of natural specimens, such as preparation of micro-sections of plants or animal tissues, micro-surgery of embryos, separation of individual nerve fibres and isolation of hormones, involves manipulation and micro-manipulation and the use of various physical and chemical processes. Thus the production of natural specimens of this type borders on production of artificial specimens. But while planning of preparation of such natural specimens includes a choice of alternative processes, there is no equivalent of the practice of preservation of samples of specimens at various stages of their production. The nearest approach to such an equivalent is the case when specimens are taken at various stages of a process—for example, tissue samples taken during the process of growth. In general only preservation of finished specimens is possible and the very act of production of such durable specimens involves steps which profoundly modify their characteristics. In some cases specimens cannot be preserved for any length of time by any known means.

The problem of planning of research has been considered in some detail in this and the preceding chapter. The subject, however, has not been adequately covered. The problems of finance organisation and human relationships have hardly been touched. An endeavour will be made to deal with these in subsequent chapters.

CHAPTER VII

ORGANISATION

In the early stage of its history scientific research was invariably individual research. A scientist might have a few pupils who were later to become scientists in their turn, and one or two assistants who performed merely menial tasks, but the entire research was to all intents and purposes carried out by the scientist himself down to the smallest detail. Later larger organisations developed. In the twentieth century large organisations appeared in which research was carried out by teams of scientists working in collaboration. To-day team-work in scientific research has become something of a fetish, and it has become commonplace to praise its superiority over individual research without any examination of the merits of the case.

A mere glance at these suffices, however, to show that the alleged invariable superiority of team-work research to individual research is by no means an incontrovertible fact. Many great scientists prefer to do their research work personally, even down to small details. Kapitza has gone so far as to say that no scientist could remain a true scientist if he did not continue to carry out personally all tasks associated with his research—even the simplest, most elementary routine tasks. On the other hand, many great scientists have declared their faith in team-work—but the meaning of their declarations cannot be assessed without regard to what they understood to be team-work in science. Certainly there is more than one type of team-work, and the difference in the performance of scientific teams bears testimony to this. If all team-work were of the same type, one might say that if one team were three times as large as another it would do three times as much successful research in a given time as would the smaller team. But this is certainly not invariably true. Indeed a smaller team, and even a single scientist working individually, might outstrip the achievements of a large team. Yet there are scientists, who have done most

valuable work as members of a team, who would be neither able nor willing to shoulder the responsibilities of a purely individual piece of important research. Clearly, the whole subject is not as simple as eulogists of team-work are often apt to assert.

Individual research has the elements both of strength and of weakness, which make it superior to team-work in some circumstances and inferior to it in others. Consider the case of a scientist engaged in such individual research, either absolutely on his own or, perhaps, with a pupil and an unskilled assistant to do the simplest routine work such as cleaning of apparatus. The scientist would have to rely entirely upon himself. He would have to be capable not only of making all the observations and calculations, but also of doing all the jobs, from repair of apparatus to construction of new devices, which might require the skill of several kinds of craftsmen. He would have to spend a great part of his time on routine work, manual and mental. He would have to limit the scope of his experiments and the number of his observations to enable results to be obtained in a reasonable span of time. All these disadvantages could be overcome only by a great scientist who included in his scientific abilities a high power of simplification and a gift for economising effort, and who could, if need be, display the skill of a most versatile craftsman. Even so versatile a scientist would find his great gifts inadequate if he had to deal with problems in which very many observations were essential, or where numerous specimens requiring lengthy preparation had to be used. But if the particular piece of research was of sufficiently novel nature to enable valuable data to be deduced by the best methods of experimentation and interpretation from a limited number of observations and a small number of specimens, such a scientist would enjoy peculiar advantages over a research team. Because the research would be of novel nature, unexpected points of interest would crop up everywhere, even in the course of the simplest manipulation or calculation. He would be in a position to seize upon these points, which would have escaped a less penetrating observer, draw conclusions, devise short cuts, and make improvements at a speed which could not be attained in any other way. Because the whole of the research would be in his own hands,

observed with his own eyes, and guided and interpreted by his own brain, he would be able to shape it and achieve results in a manner which no team could emulate. He would have an advantage in his scientific work over a team somewhat akin to that which a single composer would have over several composers producing a joint symphony. It is for this reason that great discoveries in pure science, and even in applied science, have been mainly due to individual investigators, often hampered by limited facilities.

But let us now consider a type of scientific research which necessitates a great deal of craftsman's work, and a scientist who is well-qualified to conduct research of the highest order, but no craftsman. Here no progress can be made unless the scientist can have, besides his pupil and his unskilled assistant, the help of one or more craftsmen. This is still not a team in the modern sense of the term, because the entire work still bears the stamp of an individual effort. It is however beginning to display some of the advantages and disadvantages of a team. The advantages are the added skill of craftsmen. The disadvantages are that the scientist is now beginning to miss some of the points which he would have observed if he were dealing with the whole matter single-handed.

Let us now consider a team of the kind which a scientist himself would desire for a particular piece of research. An example of such a team would be an arctic expedition. No scientist would be prepared to take upon himself the joint duties of a geologist, meteorologist, biologist, engineer, navigator and doctor, as well as arctic explorer. A team is essential here, and its use is not a twentieth-century innovation. But suppose research is of a kind which does not call for a great diversity of knowledge and skill but only for a large number of similar observations. Such teams were organised for bacteriological research before the twentieth century. They were small teams, and the scientist at the head still felt that he was handling the main factors of research personally. But it is debatable whether the increase of performance thus attained proved commensurable with increase of total effort, and it is arguable that the scientist at the head of the team might actually have made more headway if he had only two or three pupils worthy of him.

Let us now pass on to consideration of team work in its characteristic contemporary form. The first thing that strikes one is that there are several types of teams. The second is that all these types of teams have been brought into existence, not by demands of the scientists themselves, but by their patrons or representation of patrons—that is, by government officials, by business executives of private enterprises and by gentlemen who were not themselves scientists but thought that they understood better than scientists how research should be organised.

Scientists who objected to such team-work met with a good deal of organised criticism but, in any event, since the majority of patrons accepted the idea of team-work, and since scientists are seldom men of independent means, opposition to the team idea could not be maintained. As for the patrons responsible for the innovation, their attitude was most natural. They observed that a hundred employees in a shoe factory could produce not a hundred times more than one shoemaker, but several thousand times more, and that this was also true for all production, from newspapers to sausages. And if this was true for shoes, newspapers and sausages, why not for scientific research? True, certain scientists thought otherwise, but then these scientists were clearly incapable of making money out of shoes or newspapers, or even out of sausages.

It must not be supposed, however, because modern team-work was originated not by scientists but by their patrons, that it is therefore entirely inferior to earlier forms of scientific organisation. There are four main types of teams, and of these one gives satisfactory and another highly satisfactory results. All teams have a head of research, who may be a first-class scientist, and is at any rate a scientist of repute. The head of research, if he is a great scientist capable of brilliant research on his own account, finds it galling to be compelled by the nature of the organisation to waste a great deal of his time in administrative work, reports to patrons and so forth, to have only a general contact with the actual research of the team and to have little time left for personal research work. His best chance of doing himself justice is that offered by the type of team which is composed of a number of autonomous team units, each concerned with and capable of carrying out a complete piece

of research. This type of team is the best of the four team types. In it the senior scientist in charge of each team unit is in a position to carry out research with most of the advantages associated with individual research, plus the advantages of equipment, information and additional experiments which other team units can provide. The head of research in such circumstances can not only find time for active participation in research, but also can establish a relationship between the scientists of the team and between them and himself which, according to individual circumstances, is of colleague with colleague or of teacher and pupil, and are in the best tradition of the practice of science.

Such a team, however, is the very best of the four types. A type not nearly so satisfactory is that in which the conditions described above are varied by each team-unit being constrained, for reasons of higher policy, to conduct research on specific lines, irrespective of whether these lines do or do not appear to the senior scientist at the head of the team unit to be the best lines of investigation. This type of team was employed during the Second World War on atomic bomb research. The justification of this type of team is that it produced the desired result in the minimum time—which was essential for war purposes. But it is an inefficient type of team, because a high proportion of its effort is doomed in advance to be wasted effort.

The third type of team is one which cannot appeal to a head of research who is himself capable of brilliant research work, and is best directed by a head who is a competent scientist and has a gift and a preference for organisation work rather than for research itself. This type of team has originated from patrons' desire to economise on equipment expenditure, and their idea that the principle of subdivision into process sections, which had proved advantageous in mass production, could also be applied to scientific research. A team of this type is divided into autonomous units, none of which is capable of carrying out a complete piece of research, but each of which is capable of carrying out processes of a specific type. For example, one unit is equipped to do chemical analysis; another, high-temperature treatment; and so on. From the point of view of mass production technique, the results are excellent.

A piece of analysis, for example, can be carried out much more quickly than in a research team organisation of the first type. But from the point of view of scientific research, the results are deplorable. No scientist in the team now has a complete grasp of any piece of research, but merely a grasp of component parts of several pieces of research. No amount of free interchange of information between the units can remedy this. The head of research himself is the only person who can have a complete picture of research in his mind, but he is submerged by the work of organisation, co-ordination, reports, and so on, to a far greater extent than a head of the first or second type of team; moreover, all his information is second-hand. Able scientists are often found in teams of this type, but it does not attract the great scientists who would be prepared to work in teams of the first or second type. No doubt this type of team produces results which satisfy its patrons—or it would by now have disappeared. But by standards of scientific research of the highest order it is a complete failure.

The fourth type of team is the worst, and should be shunned by all good scientists, young or old, senior or junior. This type of team is similar to team type three, except that its component units are too closely controlled by the head of research to be described as autonomous, and there is no interchange of information between the units, except through the head of research, or to the very limited extent sanctioned by him. Secrecy is enforced, and radiates from the head down to the most junior member of the staff, the object being to ensure that only the patrons and the head of research know what all the work is about and what results are being achieved. Such a team produces results of the lowest value. It is a death-trap for young scientists, and a slough of despond for older ones. Only third-rate scientists are content to remain for long members of such a team. As to the head of such a team, he may be an energetic and able man, well endowed with qualities ensuring him financial and social success. But he is extremely unlikely to be a scientist of high order, or one genuinely devoted to the cause of scientific research.

Omitting from consideration the fourth type of team organisation (to which no further reference will be made), it

can be said that the head of any research organisation must possess, besides his scientific ability, a number of qualities fitting him for leadership of those under his direction. First and foremost, he must be able to win and to retain the confidence both of his patrons and of the research staff. He must appreciate the point of view of his patrons—that is, not their ideas about ways of conducting research, a subject which they are hardly qualified to teach him, but their ideas as to why research is worthy of their support, what they expect it to give them as a reward for their expenditure, and how quickly they want this reward to materialise. He must take care to satisfy them that the research under his direction will give them the results which in their opinion would justify their financial support, and see that his promises are implemented. If he does not do this he will get no financial support, or a support so meagre that no successful research could be conducted with its aid. He must be prepared to argue the case not only of the organisation as a whole but of any items of expenditure, any piece of apparatus, or any member of the organisation, should those who furnish the finance so desire; and he must be prepared to argue on grounds and in terms not of his own choosing. Equally, he must be able to understand the members of his organisation, deservedly to gain and keep their trust and respect, to guide and inspire their efforts, both to teach them and to learn from them, and to promote between them the spirit of comradeship and co-operation in the common task. Very few men possess all these virtues, and the greatest of them generally prefer the more free air of a university, or the wider vistas of nationally-sponsored organisations, to the atmosphere of industrial research establishments, except for the largest and most progressive of these. The fact that so many industrial enterprises have failed to attract such men, or having attracted them, fail to recognise their merits, has meant a great loss to industrial progress. It has been the author's privilege to know two such men, one of them of international reputation, both of the highest character, for whom industrial executives in their wisdom could find no place at the head of their research organisations.

It frequently happens, in the case of smaller research organisations, particularly in industry, that the man in charge of

research has no direct contact with those who determine the financing of research, its objectives, or the size and form of the research organisation itself. For example, a laboratory engaged in specific industrial research might be under a chemist or physicist who is himself under a works manager, or assistant works manager, the latter in his turn being responsible to a director. Under such conditions the so-called research laboratory might do quite a lot of work, but none that is likely to deserve the title of serious organised research. The reason for this is that real decisions are not made by the chemist or physicist in charge, or even by the manager or assistant manager who at least sees the head chemist or physicist, but by someone whose contact with the laboratory is third-hand. Now no one in the research laboratory can have any idea of what resources might be available for any piece of research, how long it is to continue, when it may be radically altered, expedited, curtailed or wiped right out, or whether the discoveries and recommendations of the research staff are going to be accepted, rejected or altered beyond recognition. One or two determined spirits usually make desperate efforts to finish off a piece of research that appears most promising—whatever the official instructions. Men start working "off the record". Eventually the promising piece of research is stopped dead and everyone feels disgruntled, though for different reasons. In the meantime men in various other departments start experiments on their own account and bring their ideas to the manager, or assistant manager, who according to his own judgment either rejects or accepts them. If they are accepted they are presented to the head of the laboratory as new truths, which frequently sets the research staff's teeth on edge. All this is, of course, very unfortunate, and the fact that everyone concerned is acting from excellent motives, and as well as the circumstances permit, does not help matters. There is no remedy for this state of affairs other than the creation of a real research organisation whose head is directly in contact with the patrons.

In all research organisations a scheme of work involves a choice between rigid and elastic plans of work, and between working to a time schedule or ignoring the time element. Sometimes the choice is made outside the organisation and

imposed upon it, as in the case when the results of a particular piece of research are required by some body or person outside the organisation by a certain date. Certainly at no time is it practicable to work to a completely elastic plan, since such a plan would in fact be no plan at all; similarly no serious research can be conducted without any regard to time whatever. But a plan so rigid as to be unalterable in any circumstances is likely to prove satisfactory only if the research problem is a simple one, requiring little more than steady work for its solution and with nothing startling likely to turn up. And a rigid time schedule may mean that the most important points may have to be slurred over, and the work brought to a lame conclusion instead of being finalised in a really satisfactory manner. The satisfactory course is the "golden mean". A plan should have enough rigidity to ensure that a little difficulty, or a slight set-back, does not lead to a plunge in some new direction, and that persistent effort should be made to follow a mapped course. But the plan should also have enough elasticity to enable a change of course once it is clear that progress cannot be made without such a change. Similarly, a time schedule is a valuable guide, and all reasonable efforts should be made to keep to it. But the best results cannot be expected if keeping to the time schedule is made into a fetish. The course of research may show that some extension of time would enable important additional data to be obtained, doubts resolved, and conclusions amplified or confirmed. In such circumstances, unless the time schedule is imposed from outside and cannot be altered, it should be stretched to fit the new circumstances.

The question of working to a rigid or elastic plan, to a rigid or elastic time schedule, leads to the question of discipline. Discipline in a research organisation may be imposed, or may be self-discipline, or a combination of the two. Imposed discipline may operate satisfactorily in a mass-production factory (though not invariably so, as Charlie Chaplin showed in *Modern Times*). But in a research organisation, where men have to exercise such qualities as imagination, scepticism, enthusiasm and devotion, it can work only in small doses. A scientist engaged on a particular piece of research may have to put in 48 hours' continuous work to deal adequately with a particular

problem. It is absurd to expect him to turn up on the third day at the normal hour and put in a normal day's work. A man may feel keen enough to work on a problem long after hours—if so, he cannot be expected to be punctual in the morning. A man engaged on research is at his best when he feels completely free and "at home" in the laboratory. Consequently restrictions which operate normally in mass production establishments should be waived in a research organisation. A research worker will work better and not worse because a cigarette, a cup of tea, a sandwich or a chat, can be indulged in during working hours. A man used to his pipe will do far better research if he need not remove it from his mouth because of a regulation. But relaxation of imposed discipline calls for creation of self-discipline. Men and women freed from artificial restrictions can do good research only if they discipline themselves to do their best. Not all men and women who find that the only discipline they need consider is self-discipline, rise nobly to the occasion. There are quite a few who will accept the privilege of unpunctuality without imposing upon themselves the burden of late hours, convert the odd cups of tea, which should be a refreshing break, into tea parties, stretch friendly little chats into a meandering waste of time. There is only one really effective answer to this sort of abuse. The research organisation cannot be fashioned to suit the personal characteristics of those who will submit to imposed discipline but shun self-discipline as the devil was reputed to shun incense. But the research organisation can and should dispense with such people. It should replace them by others, who do not need a policeman, and who willingly impose upon themselves a self-discipline for the sake of the research they love.

A picture of research organisation cannot be complete unless it presents both what the organisation as a whole and the senior scientists within it may reasonably demand from the young junior scientists, and what these young scientists have a right to expect from the organisation and the seniors in their turn.

Young scientists must be prepared to take their part within the research organisation and to carry out their tasks both seriously and enthusiastically. Seriousness and enthusiasm do not always go hand in hand, but in this case they must do so, for seriousness without zest of enthusiasm, or enthusiasm which

is careless and improvident, are neither of them adequate for success in research. Of necessity a good deal of the work which a young scientist has to do while in a junior position is going to be far short of the marvellous achievements he may have dreamt about before joining a real research organisation. Much of it will be routine, and most of the rest will appear too simple. The young scientist should console himself by two considerations—first, routine work has to be done by someone, and is an absolute essential to success of any research; secondly, he is almost certainly wrong in imagining that the work which seems too simple is as simple as it seems. The author would assure the doubters that he can recall many instances when an intelligent, but over-confident junior, filled with contempt for the "too simple" task, perpetrated mistakes that were absolute "howlers", and caused no end of trouble before they were discovered and corrected by a senior.

Another thing the young junior scientist should bear in mind is that he is there both to help the senior scientist to whom he is responsible, and to learn from him. The young scientist should feel that the senior scientist deserves both his confidence and his conscientious assistance. If he does not feel this, he would do far better to go elsewhere, for the existing arrangement will prove unsatisfactory to all concerned. The senior scientist has a right to expect that the junior will carry out his requests to the best of his ability, will honestly report difficulties and failures, and will accept the information, explanations and advice given as reliable. The junior scientist must not forget that he is in the process of learning how research should be done, and that he has much to learn. It is inevitable that some of the things a senior will request the junior to do, and some of the information, explanations and recommendations he will give the junior, will strike the junior as conflicting with his previous concepts. If the junior feels that the senior deserves his confidence and support, he will accept the senior's guidance, though he may very reasonably argue the points strongly on occasions—which will help to clarify things. If on the other hand the junior decides that the senior is an ass and treats his requests and opinions accordingly, neither the work, nor the junior, will make any progress. On no account should a junior carry out the work "in his own way"; that is, differently

from the way in which it is supposed to be carried out, or make changes, omissions, and additions in experiments without previous report to the senior, or, what is even worse, any report at all. The whole plan of experiments can be wrecked in this way. Above all, in no circumstance should a junior ever stoop to "cooking" results.

When, in due course, the junior gets an opportunity of doing interesting novel work requiring initiative and presenting real difficulties, he should develop neither a swelled head nor cold feet. He must assume that the work is not beyond his power, but, at the same time, a difficult step forward which cannot be made without thought, care and effort.

So much for what the young scientist, starting on a career of research, should do. But besides his obligations, the young scientist has also rights. He has the right to expect that the senior will treat him, not as a blind instrument, but as a pupil. He has the right to expect that his ideas, whether right or wrong, will be given a hearing. He has a right to expect sympathy and encouragement. He has a right to expect that his seniors will regard him as one who will in time qualify to be their equal—or to tell him frankly that he is not fitted for a research career. He has a right to a future.

CHAPTER VIII

EXPERIMENTATION— GENERAL CONDITIONS

IN THE previous chapters several references have been made to the value of discussions. Here we shall consider discussions as an integral part of the conduct of research in all its phases. There is little doubt that discussions are of very great value in initial planning, in experimentation, and in formulation of final conclusions. There is no better way of co-ordinating effort, avoiding errors, solving difficulties, and speeding the progress of work. But these advantages come only of discussion carried out in a manner calculated to reveal the scientific truth. Earlier in this book a reference was made to a great contribution made by Socrates to principles of scientific research. This contribution was in the mode of argument devised by him. The unique power inherent in the Socratic dialogues of revealing the truth has not yet been sufficiently appreciated by scientists, though many great scientists at times showed this appreciation by publishing their views in dialogue form. In law, where honest judges were always concerned with establishing the truth, the dialogue between counsel and witness became an established feature of legal proceedings, the question and answer technique being clearly modelled on the Socratic dialogue. In politics, however, where each politician was concerned with establishing his case and demolishing that of his opponent, an entirely different technique has been established. In a political debate, whether in the Press or on the platform, a politician seeks to put his case to best advantage. Unless he is denied a report in the Press, or gets no fair hearing, he does so put his case and gets a limited number of criticisms, to which he replies with the advantage of having the final word. His political opponent does likewise. Thus, both politicians, if allowed to present their views, have the advantage of a technique calculated to enable each to present his case

in the best possible light, and the truth of the matter cannot possibly emerge in the debate itself, but only from the way in which, subsequently, the public finds the politicians' statements to be in accord with or at variance with evidence from other sources. Scientists, unfortunately, have made too much use of the politician's mode of argument and too little of the Socratic method.

Consider a typical case of a scientist who wishes to present the results of his work. He may publish a paper in a scientific journal, or he may read a paper before a learned Society. His object is to obtain recognition and acceptance of his work. If he is not refused publication, like Waterston, and is not, like Newlands, jeered at by an antagonistic audience, he presents his case to the best advantage, has the last word in replying to his critics, who cannot ply him with a succession of questions, but can only express a view once or twice, and the only way in which his scientific opponent (assuming he has one) can seek to demolish his case, is by presenting his own thesis under similar conditions. The method is certainly not a Socratic one and resembles much more closely the technique of politicians. The only scientist who really suffers through this method is the one who has the temerity to present a really revolutionary view. Unless his reputation is already established, he may find it impossible to get into print or present his views in a lecture hall. He will not be able to present his views adequately in the form of criticisms of another's work. Indeed, his position may become quite hopeless unless he succeeds in getting the support of a recognised authority. No doubt many scientists will disagree with the above remarks, and say that no scientific discoveries or views which have merit are ever accorded such poor treatment to-day. But they cannot deny, in the face of history, that such things did happen in the past though established scientists declared this to be impossible, and it is difficult to see what grounds there can be for an assertion that to-day human nature and understanding are so perfected that it is no longer possible, anywhere, at any time, for an obscure scientist to have his revolutionary discoveries rejected.

The Socratic method of argument, however, irresistibly urges us towards revelation of truth. If it were possible for an opponent of a scientist who had just published or read a paper

to challenge the author to a Socratic debate as to the scientific merits of the case, and an obscure scientist who had been denied the opportunity to publish his revolutionary views could similarly challenge his censors, there is little doubt that a great deal could be added thereby to scientific knowledge and understanding, whatever the rights and wrongs of each particular case. There is no reason to suppose that such a procedure would embarrass the wise and the learned and give undue liberty to all sorts of charlatans, cranks and ignoramuses. The Socratic method can only confirm the wisdom of the wise and the learning of the learned, and charlatans, cranks and ignoramuses could never stand up to it for any length of time without being ruthlessly exposed. If, on the other hand, a supposed charlatan, crank, or ignoramus could, by this mode of argument, embarrass the supposedly wise and learned, this would only prove that some established ideas needed revision and what was generally believed before the debate should not have been believed.

Perhaps one day the Socratic method will get complete general recognition in the scientific world. In the meantime, however, there is no need to await such a recognition before making full use of it within a research organisation, and, indeed, it has been used widely in this way, though often unsuspectingly like prose which Monsieur Jourdain spoke unwittingly all his life.

The following examples of discussions, one actual and the other imaginary, illustrate the difference between the Socratic and what may be conveniently termed the "politician's" method of argument in a research establishment.

The Politician's Argument

JUNIOR: I have just measured the density of some fused tungsten and found it is 20·15.

SENIOR: Impossible! The density cannot exceed 19·35.

J.: I know 19·35 is the recognised figure for pure tungsten. But I made my measurements very carefully and I wonder whether the previously determined figures were too low because they were obtained for unfused tungsten and the specimens may have been slightly porous.

S.: The figure of 19·35 is recognised by the highest authorities. You do not suggest that you know better than all of them?

J.: I realise these authorities know much more than I do. But their determinations were not made on fused tungsten. I cannot see how I could have made a mistake. Would you care to see what I have done?

S.: I don't want to see what you have done. Take it from me, you have made a mistake.

Junior and Senior go their ways shaking their heads, each dissatisfied with the other.

The Socratic Argument

JUNIOR: I have just measured the density of some fused tungsten and found it to be 20·15.

SENIOR: You must have made a mistake. The generally accepted figure for pure tungsten is 19·35, and the latest scientific evidence indicates that it probably does not exceed 19·30.

J.: I know the recognised figure for pure tungsten is 19·35. But I made my measurements very carefully, and I wonder whether the previously determined figures were too low because they were obtained for unfused tungsten, and the specimens may have been slightly porous.

S.: I see you are determined to challenge established authorities. Very well. Your argument is that the authorities may have made a certain kind of error in their determinations, while you are sure that you have not made any error. Is that your point?

J.: Put that way it sounds bad. All I mean is that I cannot see any error in my work, and I have suggested a reason why previous results were too low.

S.: Let us first of all examine the first part of your argument. Do you maintain that while even authorities can make errors, you could not make one?

J.: No, of course not; but I cannot see what error I could have made.

S.: Does any man always see the error he has made? You have just pointed out that authorities could have made an error.

J.: That is true, but I have suggested what their error might be.

S.: One thing at a time. You have admitted that you are capable of error. Now let us deal with the second part of your argument. The density of tungsten has been calculated from its atomic weight and crystal lattice. Porosity, therefore, has nothing to do with previous determinations. You can only argue that previous results are inaccurate if you are able to challenge either the previous atomic weight of tungsten or the previous crystal lattice data. Have you any grounds for challenging either?

J.: I have not looked at it that way. It is difficult to challenge atomic weights or crystal lattice data.

S.: Of course. However, to satisfy you, let us try to do so. Tungsten has four isotopes. If your specimen were composed entirely of the heaviest isotope, the atomic weight of your tungsten would be higher than usual. Do you think this might explain matters?

J.: Since you put it this way, might not the fusion of the specimen in the arc for a long time have expelled the lighter isotopes?

S.: I think this is improbable. But suppose it did, it would give you in the extreme case an atomic weight of 186 as against the recognised average atomic weight of 184. Could this explain your higher density figure?

J.: No. I see it could not.

S.: Very well. Now as to lattice structure. Do you think this could be different in your case?

J.: Might not there be a different crystal form of tungsten?

S.: If there is, you have discovered it. What reason have you for making such a claim?

J.: My density shows . . .

S.: No, that won't do. Would you be prepared to agree that any difference in density between specimens of the same element was a conclusive proof of different crystal structures?

J.: No. But a difference in density might be due to a difference in crystal structure.

S.: It might, but you would have to prove it, would you not?

J.: Yes.

S.: Don't you agree that we have now come to the point when the only ground on which you can challenge the authorities is that you could not have made a mistake?

J.: I suppose so.

S.: And have we not agreed that you are capable of a mistake?

J.: Yes.

S.: Well now, what is the conclusion?

J.: I must have made a mistake.

S.: There is no "must" about it. It is exceedingly probable that you have made a mistake. Unless you can produce much stronger evidence to challenge the authorities, the only reasonable conclusion is that you did make a mistake. You could only challenge the authorities if you could prove by X-ray analysis that you have produced a new crystal structure in tungsten. Personally, I don't think you have produced such a new crystal structure, because the only evidence in support of its existence is that it would confirm that you could not have made a mistake in your density determination.

Junior and Senior part on good terms, the Junior deciding to learn more about the work of authorities and to check his methods of density determination.

Some may criticise the above example of Socratic argument on the grounds that a senior cannot afford to waste so much time in educating the junior. This objection is unsound for two reasons. In the first place, seniors who are content with juniors who are merely diligent and obedient can afford to treat their juniors' attempts at independent constructive effort in a peremptory manner: but in such event they cannot hope to have continuous assistance of any but the worst type of juniors. A junior who can never think for himself, has no desire to become a scientist of high order, and is entirely unimaginative and lacking in powers of scientific criticism, can never get much above the level of a routine worker, and his value to a research organisation must always remain low. In the second place, a junior genuinely interested in scientific research has every right to sympathy and guidance. He has a right to expect that, in return for his devotion to his task, he will get every encouragement and help to develop into a good

scientist capable of doing high quality research on his own account.

In any research organisation there are inevitably discussions concerning information, methods, results and interpretations. In the better kinds of research organisations such discussions are more frequent and more fruitful. This aspect of research has already been considered. There are, however, several additional remarks which should be made. No one should plunge into scientific discussions before going into the subject carefully, thinking about it, and getting as clear an idea as possible of the facts, difficulties and possibilities. Naturally, a man who has already done this need not go over the ground every time prior to a discussion: the senior, in the example of Socratic argument presented above, had no need to review his knowledge of tungsten before discussing it with the junior. But even the best scientist would find it of doubtful advantage to discuss a new problem the mental picture of which is still clarifying itself in his mind. A man entering a scientific discussion for which he is unprepared is under a double disadvantage: he has to rely on others for presentation of the facts to which he is unable to make a full contribution, and he has little opportunity to present any original ideas. Thus a premature discussion may easily result in conclusions which disregard important facts or submerge fruitful ideas before they have taken proper shape. It is far better, in the case of an important discussion, for all parties concerned to have reasonable notice of it so that they can prepare for it, and if some delay appears necessary, it is generally well worth while to have such delay rather than take the risk of unsound conclusions. Interchange of information is quite another matter, and should be possible at all times.

The above remarks apply principally to discussions between scientists. A scientist should remember, however, that discussions with people who are not scientists—with craftsmen, production engineers, and so on, can be very helpful. In such discussions conditions are very different from those which are met at a conference of scientists. The first thing of importance is to find a common language. A scientist discussing a problem with non-scientists should keep clear of scientific jargon, and stick to facts, commonsense and straightforward reasoning.

If he has any scientific objections to the alleged facts presented to him, he must translate such objections into non-scientific language, or the conversation will end in a misunderstanding. In all this, the scientist must not imagine that he is "talking down" to the herd. A craftsman may be not only a very intelligent man, but may also know quite a few things which the scientist does not know, and the scientist's ideas may appear on occasions just as absurd to a craftsman, as a craftsman's to the scientist. The author recalls an incident when a mathematical physicist devised what he imagined would be an excellent device involving the use of an evacuated thin-walled glass bulb in the form of a disc about 2 ft. in diameter and about 2 in. thick, with numerous electrodes sealed into it. He explained his requirements to a glassblower, and asked the latter to blow him such a bulb. The glassblower, who did not understand what the device was or why it was wanted, did on the other hand know perfectly well that such a bulb could not be blown on any bench by any glassblower. The mathematical physicist, who realised that the glassblower understood no science, imagined the latter to be a simpleton, and repeated his request in terms appropriate to a listener of very low intelligence. Whereupon the glassblower lost his patience and told the mathematical physicist to flatten a spherical bulb by sitting on it. With this the discussion closed.

If a scientist should not belittle the information which a non-scientist offers, he should not go to the other extreme of overvaluing it. A craftsman's recollections of his own successful practice are completely reliable. His recollections of details of experiments in which he participated to a limited extent are, however, generally unreliable and can be very misleading. The difference between the reliability of the two types of recollections is due to a craftsman's unfamiliarity with the scientific method, and his over-confidence. The over-confidence is easily explained. A craftsman, particularly a good craftsman, knows the tricks of his trade so well that they become parts of his unconscious actions. He does many things without prior thought, just as a swimmer swims automatically, or a cyclist does not think about balance. Certain things which a craftsman does have to think about in his trade he remembers with such

perfection that not fatigue, illness or alcohol can blot them out. There have been instances of craftsmen doing their job perfectly when they could not stand upright. This, quite naturally, gives a craftsman great confidence in his memory. But his memory is really no better than any other man's, except in matters connected with his own trade. He cannot remember parts of an experiment unconnected with his trade better than a scientist who witnessed them; in fact he will remember them worse, because for the scientist these parts of an experiment will have more meaning. A scientist, however, has learned to distrust his memory, and will doubt some of his recollections, which will increase the confidence of the craftsman accustomed to trust his own memory.

In general, in scientific research, particularly in experiments, trusting to one's memory is a bad practice. Every research organisation should have a good reference library, well stocked with books both directly and indirectly connected with the type of research pursued; with scientific journals, copies of papers; abstracts; and, in the case of industrial research, patent specifications. A card-index type of abstracts of publications with an appropriate system of cross-references is a valuable asset. There is generally neither money nor space available for all the scientific journals, going back some thirty years and kept up to date, including every article of interest. It is, however, always practicable to have a number of such complete journals, anything from two or three to twenty or thirty, dependent upon the size of the research organisation and the nature of the research, and to have printed or photostat copies of interesting papers from other journals. In the case of scientific papers in foreign languages, translations of the most important papers should be available; in theory a qualified scientist is a master of several foreign languages, but in practice it frequently happens that some one engaged on a particular piece of research does not know the language in which an important paper is published, or knows it imperfectly. The expense of such a library is well worth while, for the alternative —that a scientist should consult the publications at an institution library, the Patent Office, or any other organisation unconnected with the research laboratory, or borrow them from a lending library, one or two at the time—means that the

scientist would have to rely on his brief abstracts of the publications, and his memory of the rest, instead of having the full information all the time at his elbow. However excellent a man's memory may be, the practice of burdening it with accumulation of details is not satisfactory, particularly when it is often impossible to say in advance what details are worth memorising.

Even more emphatically is it inadvisable to trust to memory in the matter of records of actual experiments. There are many reasons, some of which have been considered in an earlier chapter, for recording each experimental step and the results obtained immediately, or at any rate as soon as possible after their occurrence. As far as measurements are concerned, especially when these are numerous, the necessity of immediate recording is obvious to every research worker. The necessity of immediate recording of experimental steps and results, expressible not in figures but in words, is much less obvious. Young scientists often imagine that a description of such steps or results, particularly if these appear to have a clear meaning, can be written down just as well days and even weeks after the completion of a particular experiment, as on the day the experiment was performed. Nothing is further from the truth. The conclusions drawn from an experiment remain firmly fixed in memory, if the experiment gives a fully expected or a startlingly unexpected result, but if the result is nearly but *not quite* what was expected, the slight differences tend to fade from one's recollection as time goes on. The details of the actual experiment are retained by memory less firmly than the conclusions. With time, only the details which appeared significant during the experiment remain. A young scientist who has just obtained his university degree and has successfully memorised great quantities of data will find it difficult to believe that such things can happen to him and may imagine that the foregoing remarks are meant only for those with bad memories. But there is a great difference between memorising data in the course of study and memorising data of experiments in the course of research. In the first case, one sets out deliberately to memorise facts; one knows what facts must be memorised and what facts may be held lightly in memory, or not at all, and one's recollections are not blurred by superimposition o

other data, which must also be memorised, and which bears a general resemblance to the initially memorised data, but differs from it in various ways which may, or may not, be marked, and may, or may not, prove later on to have been important. In the second case, one memorises data unconsciously, or not at all; one cannot be sure as the experiment proceeds what emerging facts must be memorised and what facts may be forgotten with impunity, and the experiment is followed by other experiments which frequently have a general resemblance to the first experiment and to each other but differ in various particulars, the relative importance of which may, or may not, be apparent at the time. In the first case, conditions are highly favourable to memorising facts, in the second case, they are unfavourable; moreover, and this is the most important point, in the first case it is always possible to refresh one's memory, because the data have been recorded by someone else, while in the second case an observer who has not recorded the facts cannot take them out of the past and look at them afresh, but may only, and that not always, perform the experiments all over again.

It is therefore the essence of sound research practice that all steps taken in the course of an experiment, and all observations, should be recorded not only immediately, or at least as soon as possible, but that they should be recorded methodically, clearly and in the greatest possible detail, and that such records should be carefully preserved. Such recording may be properly described as *main recording*. For purpose of drawing and presenting conclusions, such records are too unwieldy, and contain a great deal of matter which, in the final presentation of the research, can be greatly condensed. It is therefore highly advisable to prepare, on the basis of main recording, an abridged record containing only what *appear* to be the relevant essentials. This abridgement may be conveniently described as *subsidiary recording*. The term "subsidiary" appears more appropriate than such terms as "summarised", "final", or any other terms which would suggest that such subsidiary recording does in fact contain the entire essence of the main record. Subsidiary recording, invaluable as it is, cannot be regarded otherwise than as an aid towards the final presentation of the work performed and of the conclusions

reached. Any subsidiary record may, at any time, have to be reviewed because of newly emerged facts which may indicate its inadequacy in certain particulars. In such event, the investigator must go back to the main record, and it may well happen that, consequent upon this, the subsidiary record has to be amended. Again, it may happen that the investigator, in the course of his research, finds himself pursuing several objectives, some of which may not have been apparent in the initial stages of the research. In such an eventuality there may be several subsidiary records, each connected with a specific objective. Like the main record, subsidiary records should be preserved with this difference; that while the main record must be regarded as permanent, a subsidiary record need be preserved only so long as it does not require amendment and can be entirely discarded in favour of an amended version.

The question of main recording cannot be disassociated from the question as to who makes the observations or carries out various steps in an experiment. In the ideal case, when the scientist responsible for conduct of a particular piece of research makes every observation personally and himself carries out every experimental step, down to the simplest detail, there is the possibility of the main record being as near perfection as human imperfection might permit. But such an ideal practice, as we have seen in the chapter on organisation, is far from being universally observed in scientific research, and is in fact impossible not only in many organisations, but in many fields of enquiry also. The question concerning conduct of experiments and the making of observations is to a large extent the question of what steps in an experiment, and what observations, a scientist conducting the research may reasonably delegate to others, and if so, under what conditions, and what steps in an experiment, and what observations, he should make every effort to carry out personally.

With regard to steps in conduct of experiments, the scientist conducting the research should delegate, as far as possible, only such steps as may be agreed and *completely described in advance.* The satisfactory carrying out of these steps then becomes largely a matter of the reliability of the persons to whom the work has been delegated. The possibility of contact between the scientist and his assistants should in the circumstances be a

sufficient safeguard against unexpected departure from the mapped course. When a particular step cannot be *completely* described in advance, the scientist must keep in closest contact with his assistants, to the extent of personal participation in the experiment, while the factors necessary for *complete* definition of the particular step in the experiment are being elucidated; if he does not maintain such close contact at this critical stage, he ceases to have a complete grasp of the experiment. If steps in the conduct of the experiment cannot be determined in advance, or can be determined only in a vague, general manner, a scientist delegating such steps to others abandons personal participation in the experiment, except in so far as general advice and criticism, and co-ordination of results, are concerned. When a scientist abandons personal participation in research involving unpredictable steps in experimentation, the research is doomed to failure unless those to whom he delegates the carrying out of such steps are themselves scientists of high ability; in such circumstances, therefore, successful research is possible only if the scientist in charge is the head of a research team.

With regard to observations, possibility of delegation of the work largely depends on whether the observations are of a quantitative or qualitative nature; on whether they are "pure" or interlinked with one or more experimental processes; and on whether the conditions under which they are to be carried out, and the mode in which they must be conducted, can or cannot be specified precisely in advance. If the observations are interlinked with one or more experimental processes, the remarks just made concerning delegation of steps in experimentation apply. If conditions under which observations are to be carried out, and the mode in which they must be conducted, cannot be specified precisely in advance, delegation may be quite satisfactory provided the uncertainty at the commencement of the work is limited to that of choice, for all or for some of the observations, of the most suitable of a limited number of clearly defined possibilities in regard to conditions or mode of observation. If, however, the uncertainty goes beyond a choice of defined possibilities and implies the devising of conditions or mode of observations, or of both, delegation is only possible if the scientist allocating

these tasks is prepared to abandon personal participation in the experiments and to limit himself to general guidance and co-ordination.

Quantitative observations may in general be delegated more readily than qualitative ones. This may strike a young scientist as paradoxical, since quantitative information in science implies a more exact knowledge than does merely qualitative information. The apparent paradox is, however, easy to explain. Quantitative observations, provided the work is carried out in a competent manner, are not a matter of personal judgment, and cannot vary with the observer. The readings on a milliammeter, on a spectroscope, or on an eyepiece micrometer, are not matters of opinion, but of fact. Interpretations of the observations can be deferred until the record is inspected by the scientist conducting the research. Qualitative observations, on the other hand, are essentially a matter of judgment. In the simplest case, such as, for example, decision whether a liquid is acid, alkaline or neutral, judgment is a simple matter, and delegation of observations can be adopted without hesitation. In many cases, however, judgment is not at all simple, and demands not only knowledge, experience and care, but keen perception, power of discrimination, and a high critical faculty. Furthermore, unlike quantitative observations, which can be recorded first and judged afterwards, qualitative observations can be judged satisfactorily after recording only if the *judgment of the observer* has been sound and adequately recorded. Lastly, adequate recording, which is only a matter of proper care in the case of quantitative observations, while straightforward enough in the case of qualitative observations of a simple nature, can be extremely difficult in the case of qualitative observations of the kind which tax the perception, discrimination and critical powers of the observer.

The question of delegation of observations, and of steps in experimentation, brings up another question—that of allocation of credit for work done. No doubt the question of credit is not one which can be regarded as directly connected with methods of scientific research, since it is not concerned with the problems of acquiring new scientific knowledge and understanding, but only with the problem of deserved praise

and just rewards. But it is indirectly connected with methods of scientific research, and this indirect connection is of great importance. Scientific research is not conducted by disembodied spirits, to whom reputation and material rewards are matters of supreme indifference, but by human beings of flesh and blood, to whom such things matter a great deal. A number of scientists well qualified to express an opinion on this subject have not found it necessary to draw attention to serious difficulties or abuses in the matter of allocation of credit: apparently they have not found any cause for great uneasiness on the subject. Others have expressed some strong criticisms of the existing state of affairs. Bernal, in his *Social Function of Science*, says:

"It is always a distressing experience for a young man to find that age and genuine eminence are not guarantees against the temptation to enjoy credit for what one has not done. Perhaps the most convenient chiefs are those amiable scoundrels who establish a kind of symbiosis with their research workers, choose good ones with care, see that they are well supplied with apparatus, attach their own names to all their papers, and when at last they are found out, generally manage through their numerous connections to promote their protégés into a good position."

These remarks, harsh as they may sound, do not touch upon the worst cases which are encountered in industrial research, when the published evidence of meritorious achievement is not in the form of a scientific paper but of a patent specification: in such cases it is possible for all credit to be denied to the person to whom it is due, and though such abuses are illegal they are by no means non-existent.

Research organisations, however, are not staffed exclusively by lamb-like juniors with halos on their brows, a few seniors who behave like wolves to the juniors and like lambs to the head, and the head who is, in the best circumstances, an amiable scoundrel, and in the worst presumably just a plain scoundrel. Some heads of research might be scoundrels, amiable or otherwise, but equally there are among them men of the finest character, honourable and generous in their dealings. Some juniors may be lamb-like, but there are others who are willing enough to climb to success using all those they deal

with as stepping stones in a most unscrupulous manner. As to the majority of juniors, seniors and heads, they are neither pure white nor deep black, but ordinary human beings no better and no worse in their principles than other average human beings. The faults manifested in apportionment of credit are due not so much to individuals as to the system.

When two men collaborate in a common task, not on a basis of equality, but on the basis of one being superior to the other in authority; when the man holding superior authority has the right of allocating the part of the task each has to perform; when the man superior in authority is in a position to direct the course of the work and to make all important decisions; it is seldom possible for the two men to take the same view of the relative importance of the work each of them performs. It is almost inevitable, when collaboration is on unequal terms, that each collaborator should take the view that his own contribution is more important than the other collaborator believes it to be. If the apportioning of credit in connection with such collaboration is dependent upon the decision of an impartial arbitrator, such apportioning might not only be absolutely fair, but might be recognised as being so by both collaborators; in the case under consideration, however, the senior of the two collaborators is also the judge. If the senior collaborator is not an absolutely honest judge, he is tempted deliberately to bias the apportionment in his own favour. If he is an absolutely honest judge, he is still emotionally biased. If he is not only honest but generous, and deliberately gives his collaborator more than his due credit, there is no guarantee that the latter will be satisfied as to the justice of the decision.

The position created by the system is peculiarly unsatisfactory, since it puts a premium on unfairness and is hard upon the most honourable. It is not the unscrupulous, ruthless and scheming young man who suffers through this system: he manages to make his way by filching credit from others. It is the sincere, honest young idealist who is likely to be painfully surprised by being robbed of credit due to him. It is not the really unscrupulous chief who ever finds himself embarrassed by criticism for deliberately unfair apportioning of credit: he treats such criticism with cynical contempt. It is the scrupulously honest chief, anxious to be absolutely fair, and

often generous in his apportioning of credit, who is likely to discover with pain that a life-long record of fair dealing is no guarantee against baseless charges of meanness and dishonesty.

On the basis of his own experience, and that of his friends both in senior and in junior positions, the author believes that the worst features of the system can at any rate be mitigated by a simple procedure. A scientist delegating certain work should, *before doing so*, file a dated record of his plan of work, of particular parts delegated by him, of the special recommendations given with the delegation, and of various ideas he proposes to try out in connection with the work. If the work is of a kind which might lead to a patent, a draft of the possible provisional patent should be prepared and left with an appropriate person or department at the same time. Subsequent interventions by the scientist in the work of his assistants, his amendments of such work, criticisms and recommendations made by him, as well as his personal contributions, should also be recorded if possible, duly dated. Records should be kept not only of assistants' work, but of any independent contributions and suggestions made by them. The assistants should similarly keep their records. All such records not only help the progress of research, but are an aid to fair apportionment of credit and a deterrent of possible misunderstandings. As an illustration of the wisdom of the above course, the author would mention a case when a research worker A was attacked by a research worker B on the grounds of filching B's idea and incorporating it in a patent. All protestations made by A as to the unfounded nature of the accusation were angrily brushed aside by B. Fortunately A was able to produce his original patent draft, which established beyond any doubt that he had gone into the whole matter in detail before B was even aware of the existence of the problem.

Senior scientists who have not already tried the above precautionary measure may well give it some thought. As to young scientists, just commencing their career of research, the author would add a little practical advice: Do not start by regarding the head of research as a kind of god, and then go to the other extreme of regarding him as an incompetent

rascal, merely because he turns out to be a human being. Do not imagine that because your senior gives you only occasional advice and guidance that you are therefore entitled to all the credit for the work you have done—the deciding factor in this matter is whether you would have done the wrong thing without such advice and guidance and not whether you believe that you would have found the mistake yourself "in time". Do not imagine that the addition of your chief's name to the first papers you publish is an injustice to you—if the papers are bad, his reputation will suffer more than yours, and if they are good, they will be more favourably received than they would be if they were under merely the name of a wholly unknown person. Do not join a research organisation before finding out something about the people in it. If it is an organisation in which all good work seems invariably to be done by only one or two men, and those joining it do not seem to develop into first-class scientists at all, or only after they leave it for another organisation; if those formerly working there never want to see their former chief again; if the head of the organisation never feels sorry to lose anyone on his staff—don't go there; it is the wrong sort of organisation. But if you find that good work is often done by many members of the organisation; that those joining it develop into really good scientists while they are there, and either stay on doing good work or pass into excellent posts elsewhere; that the head of the organisation is still glad to remember those who worked under him and past members of the organisation remember it with affection: join it unhesitatingly; it is the right sort of place.

CHAPTER IX

ACCURACY AND ECONOMY OF EFFORT

IT IS SELF-EVIDENT that in experimental work both accuracy and economy of effort are most desirable. It is undoubtedly true that a striving for a superlative and wholly unnecessary degree of accuracy in research must involve a great deal of effort which might otherwise be more profitably applied. But it is equally true that research carried out without sufficient regard to accuracy may result in a great deal of waste of effort, since the results obtained in such cases may be completely incorrect, and must, in any event, be quite unreliable. Thus, while accuracy and economy of effort in experimental work are interrelated, the relationship is not one of simple direct or inverse proportionality. Those who prefer to think in visual images may picture the relationship between effort necessary to obtain the desired information and the degree of accuracy, as a curve shown in Fig. 1. When the degree of accuracy is less than a certain value, depending on the nature of the research and the precise information desired, no amount of repetition of experiments will yield really informative results; in other words, the effort necessary to achieve the desired object becomes infinite. Similarly, when the degree of accuracy exacted becomes greater than a certain value, dependent on experimental technique, the necessary effort again becomes infinite. Between these limits there lies a useful portion of the curve, with a well-defined minimum. No research worker ever works on the portion of the curve approaching infinite effort in order to obtain the highest attainable degree of accuracy; work on that part of the curve is only possible if the finest apparatus and best technique are used, and these are only at the disposal of research workers of the highest calibre who are not prone to make such a mistake. On the other hand, a research worker, sometimes a very

brilliant one, may unwittingly be working on the portion of the curve approaching infinite effort for low degree of accuracy. Only the best experimentalists can work continually in the neighbourhood of the minimum of the curve.

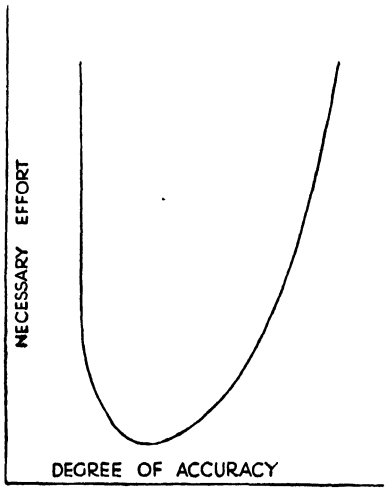


FIG. 1

It is far safer to work on the right than on the left of the minimum effort. A research worker working on the right of the minimum will certainly waste effort, but at any rate he will have a much better chance of getting the desired information than the worker who is operating to the left of the minimum. A young scientific worker, whether engaged in routine tests, routine processes, general application of knowledge to normal daily problems of the profession, or assistance to senior scientists engaged in research, is generally more prone to be not accurate enough, rather than too accurate. A senior scientific worker, whose experimental accuracy is not as high as it should be, may, in spite of this, achieve valuable, sometimes highly valuable, results. A junior scientific worker, on the other hand, if not accurate enough, gets no opportunity of proving what he can do in spite of this, through grasp of theory, imagination and initiative, but is relegated to less responsible duties where he may nevertheless succeed in manifesting himself as a real menace. On the other hand a

junior scientific worker who is excessively devoted to accuracy is always socially useful, even if he goes to the length of not only wasting effort but of wearing thin the patience of better experimentalists. It is not surprising, therefore, that in schools, universities and various scientific establishments, there is an overwhelming general tendency to train young scientific workers to work on the right side of the minimum of the effort-accuracy curve. In due course young men and women so trained frequently become senior research workers with rooted beliefs that the further one works to the right of the minimum of the curve, the better the quality of the work; or, more frequently, engaging in activities other than research, and coming in contact with practical problems, where such high accuracy is intolerable, they proceed to unlearn what they have been taught, rejecting even essential precautions in work, until the penalty of the consequences calls a halt.

There are many branches of activity where to work on the right-hand side of the minimum is an absolutely satisfactory course in practice. This, for example, is the case in industrial work where processes or tests carefully devised and standardised do not permit work too far to the right of the minimum. But in scientific research work, the ability to work in the neighbourhood of the minimum of the effort-accuracy curve is most important. The resources available for scientific research are not unlimited—on the contrary they are far from adequate in many cases. The time at the disposal of a scientist is finite, and often too brief. The difference between working near the minimum of the effort-accuracy curve and working well away from it may be the difference between success and failure.

Thus, in scientific research the choice of the correct degree of accuracy in experimentation is a matter of highest importance. There is no mathematical formula for determining what degree of accuracy is the correct one—if there was such a formula it would, no doubt, have been put to the widest use long ago. The correct degree of accuracy varies with the problem; it is determined by the nature of the objective, by the existing state of knowledge, and by the resources available. The first thing that a research worker has to do, if he wants to work in the neighbourhood of the minimum of the effort-accuracy curve, is to consider most carefully these three factors

determining the position of the minimum of the curve in his particular case.

The first factor which should be considered is the objective. The objective should first of all be defined in the simplest terms. It is then found to fall within one of three possible groups:

1. Objective where only qualitative information is desired.
2. Objective where only quantitative information is desired.
3. Objective where both qualitative and quantitative information are desired.

Although quantitative information concerning a natural phenomenon gives a fuller knowledge of that phenomenon than information which is only qualitative, it does not follow that qualitative information is, on that account, always easier to obtain than is quantitative information. There are various phenomena where qualitative information is difficult to obtain, and other phenomena where quantitative information can be obtained without difficulty. Thus, for example, the confirmation of the presence or absence of micro-organisms in the Antarctic or on the highest mountain peaks is a much more difficult problem than the accurate measurement of specific resistance of a series of nickel alloys. Even when the investigator is concerned with acquiring knowledge of one particular phenomenon, he may find it much more difficult to obtain qualitative knowledge in the first place than to obtain, subsequently, detailed quantitative knowledge. Thus, for example, it was much more difficult to establish that cadmium tungstate could be made capable of fluorescing under ultra-violet excitation if activated with uranium, than to find the optimum concentration of the uranium activator. But it can be said with reasonable confidence that in all cases where both the state of existing knowledge regarding particular phenomena and the resources available for investigation are the same, qualitative information is easier to obtain than is quantitative information, demands a lower degree of accuracy, and involves less effort. Quantitative investigation of particular phenomena which is preceded by a qualitative investigation is much easier than it would be without such preliminary qualitative investigation, since it can start

with the advantage of additional knowledge, and possibly also additional resources, which the success of the qualitative investigation has made available. It frequently happens, when quantitative investigations are preceded by appropriate qualitative investigations, that the degree of accuracy demanded by the quantitative investigations, and the amount of effort they must involve, is not only greatly reduced, but even becomes less than for the preceding qualitative investigation, because the qualitative investigation has revealed that certain precautions are wholly unnecessary.

As already indicated, the curve in Fig. 1 is not a single curve covering all experimental investigations, but a representative of a very large family of curves of similar type. Fig. 2 shows several such curves, all of which have the common features of a minimum, of two limbs extending to infinity, and of the useful portion of the curve to the left of the minimum being shorter than the useful portion to the right of the minimum. The common feature of the minimum being nearer

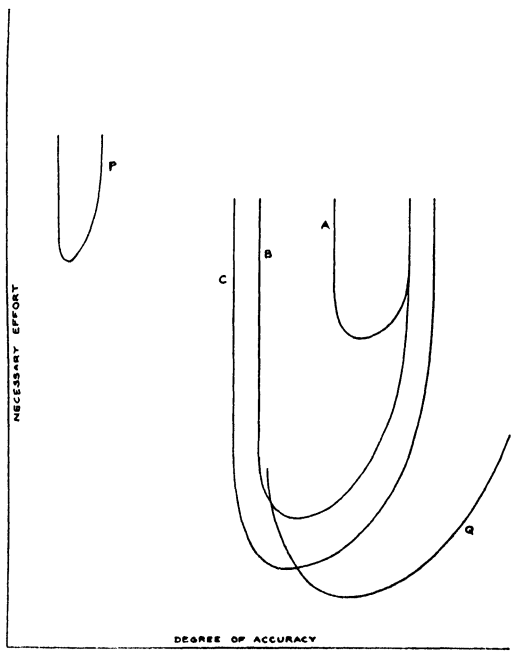


FIG. 2

the left- than the right-hand vertical portion of the curve is a graphical presentation of the fact that consistent maintenance of a degree of accuracy materially greater than the optimum indicated by the minimum of the curve is likely to have a much less harmful effect on progress of the research than maintenance of a degree of accuracy materially less than the optimum.

In Fig. 2 curve P corresponds to qualitative investigations of phenomena when existing knowledge and available resources are limited and the experimental work is difficult. In such a case optimum accuracy is low, the corresponding effort high, accuracy much above the optimum unattainable, and accuracy appreciably below the optimum gravely prejudicial to success. Curve Q corresponds to quantitative investigations when existing knowledge is extensive, available resources ample, and experimental work straightforward. The optimum accuracy is in this case much greater than for P, and the corresponding effort much less. It is possible to maintain a degree of accuracy considerably above the optimum without making the effort impracticably high. The useful portion of the curve Q to the left of the optimum is more extended than in the case of curve P, but the ratio of the practicable accuracy increase on the right of the optimum point to the practicable accuracy decrease to the left of the optimum point is greater in the case of curve Q than curve P.

Curves A, B and C relate to investigation of a problem in which quantitative information is desired but qualitative information is not yet available. Curve A corresponds to an experimental method designed to obtain the quantitative information without a preliminary qualitative investigation, curve B corresponds to a preliminary qualitative investigation, and curve C to a quantitative investigation following a preliminary qualitative investigation. In curves A and B the maximum possible accuracy is the same, and therefore the right-hand vertical limbs of these curves coincide. But the optimum accuracy for curve B is lower, and the optimum effort considerably lower, than for curve A. Also, permissible decrement of accuracy from the optimum value is greater for B than for A. Curve C, because of preliminary information derived from experiments to which B relates, has a lower optimum accuracy

and a lower optimum effort than B, a higher maximum possible accuracy than B, and a lower minimum useful accuracy. The most important point is that while the use of the technique represented by curves B and C has an optimum accuracy lower than that corresponding to the technique represented by curve A, the sum of optimum efforts for B and C is much less than the optimum effort for A. It may also be noted that the adverse effects of departure from the optimum accuracy when the method of research is represented by curves B and C are much less serious than when the method of research is represented by curve A.

The correctness of the foregoing theory of economy of effort may perhaps be more vividly demonstrated by records of two experimental investigations with both of which the author has been intimately associated. In both cases the problem was to produce a phosphor with certain desired properties; that is to say, in both cases quantitative information was apparently required.

In the first case it was desired to determine the correct proportions of three metallic constituents in a compound excited to fluorescence by ultra-violet radiation. It was already known that the compound fluoresced with the three metallic constituents present in unspecified proportions, and the problem was to find the exact proportions to obtain maximum energy output in certain parts of the visible spectrum for a given ultra-violet excitation.

A young but very capable chemist, who had no previous experience of the compound, proceeded to produce a large number of samples with the three metallic constituents in varying proportions, taking every care to purify his materials to the highest degree and carefully controlling his conditions of preparation of the compound. The intention was to submit all the samples to a spectrophotometric examination which would give the quantitative information ultimately desired. However, the spectrophotometric examination did not take place, because the author, examining the general appearance of various samples, developed a suspicion that they were, in fact, not merely variations of the compound it was desired to investigate. He examined the samples microscopically under visible and ultra-violet illumination, and, it was discovered, and

confirmed by physical and chemical tests beyond any shadow of doubt, that in every case the sample consisted of a mixture of two compounds one of which was not fluorescent and did not include all the three metallic constituents. The proportion of the two compounds in each sample was different, and could not, for various reasons, be determined, and the two compounds could not be completely separated by any chemical or physical means without affecting the component it was desired to investigate; the precise composition of which, in every sample, was now to be regarded as unknown. The entire experiment, which the young chemist had been conducting conscientiously and confidently, was a complete failure. Much time had been wasted and a fresh start had to be made.

The problem was now tackled afresh, but this time the principle illustrated by curves A, B and C was utilised. In the first place, experiments were carried out to obtain purely qualitative information; the questions were asked: How a sample of the desired compound might be prepared, regardless of precise proportions of the metallic constituents, so as to be free from the second, non-fluorescent, compound? What relatively small impurities, without regard to their precise concentration in the phosphor, might be readily encountered in its preparation, and which of them might be detrimental? Apart from this, were there any special points concerning preparation which had to be taken care of?

The qualitative investigations were completed in a comparatively short time, and the quantitative investigations which followed were finished even more quickly. The total work involved in the second attempt was brought to a satisfactory conclusion in less time than was lost in the first unsuccessful effort.

The second experimental investigation was undertaken to verify the author's hypothesis concerning production of phosphorescence in various phosphors. It had been established by Froelich and Fonda that exaggerated phosphorescence of silicate phosphors could be produced by small additions of arsenic. The author's hypothesis was that there were two other elements, either of which, in small quantities, could produce a similar effect in any types of phosphor by creation of electron traps, provided the addition element entered into the crystal

lattice of the phosphor. Experiments with two types of otherwise non-phosphorescent phosphors confirmed this hypothesis, but the third type of phosphor presented a difficulty; neither of the two electron-trap elements could, theoretically, be incorporated in the crystal lattice without the presence of a supplementary element which was itself known to produce phosphorescence in this particular type of phosphor.

The problem appeared at first sight to possess both qualitative and quantitative aspects, because it involved not merely determination of the presence of phosphorescence, but determination of its amount also, and because the effect would be dependent not merely on the presence, but on the precise quantities, of both the electron-trap element and the supplementary element entering the crystal lattice. At first sight, therefore, this looked like a problem involving not only photometric measurements of phosphorescence, but X-ray crystal analysis of the phosphor samples. Such work would require expensive apparatus and a great deal of time, neither of which were available. Actually, the apparatus available was of a rather elementary nature, and the time which could be devoted to the experiments was only a few days.

The problem, consequently, would either have to be shelved or solved in a totally different way. The author therefore examined the nature of the problem in its broadest aspect. It was true that quantitative investigations appeared necessary. But these quantitative investigations were merely intended to establish whether either of the electron-trap elements could produce phosphorescence if incorporated in the crystal lattice of the phosphor. *That is to say, the objective of the investigation was essentially qualitative, and the quantitative aspect was merely inherent in the proposed method.* That being the case, the problem should be susceptible of solution by a simpler method, not involving any elaborate measurements. The actual solution was obtained by breaking down the problem into three parts, each requiring a qualitative answer, and obtaining the answers required by three simple experiments.

The first question to be answered was: "Does a sample of the phosphor containing a quantity of supplementary element exhibit the same phosphorescence as a similar sample with an addition of an electron-trap element?" This was tested by

preparing and examining ten samples of the phosphor treated in precisely the same way. The samples were arranged in five pairs, each pair representing a different concentration of the supplementary element, and one member of each pair containing a quantity of the electron-trap element. The concentration of the electron-trap element in all samples containing it was the same. The phosphorescence of the sample, after ultra-violet excitation was examined in a dark room by the unaided eye, comparison being made between two samples of each pair. It was found that in only one case out of five was the phosphorescence of both samples of a pair the same. In the case of one pair, phosphorescence was slightly greater in the sample containing no electron-trap element. In two other pairs phosphorescence was considerably greater in samples containing no electron-trap element. In one pair the sample containing the electron-trap element had a much greater phosphorescence than the sample without that element.

While the precise effect of the electron-trap element on phosphorescence of the phosphor was still to be determined, it could be said that the answer to question 1 was negative—the phosphorescence of a sample containing a quantity of supplementary element was not the same as the phosphorescence of a similar sample containing in addition a quantity of the electron-trap element.

The second question was now formulated: "How does increase of the supplementary element concentration affect the phosphorescence of a sample containing no electron-trap element?" The five samples were compared for phosphorescence after ultra-violet excitation, comparison being for samples taken in pairs and observed by the unaided eye. It was found that phosphorescence increased continuously with increase of concentration of the supplementary element, the increase being at first rapid and then assuming a lower, more or less uniform rate.

This relationship between phosphorescence and concentration of the supplementary element, in the absence of electron-trap element, could be roughly represented by curve D in Fig. 3. As the phosphorescent brightness of specimens was merely estimated by eye, curve D might be considerably different from a curve which could be deduced from accurate

measurements of phosphorescent brightness, *but it would be of the same family.*

The third question could now be formulated: "Was the relationship between phosphorescence and concentration of the supplementary element, for samples containing the electron-trap element, expressed by a curve of the same family as curve D?" The five samples containing the electron-trap element were compared for phosphorescence after ultra-violet excitation with the five samples observations for which had been expressed by curve D. The results obtained were plotted as curve E,

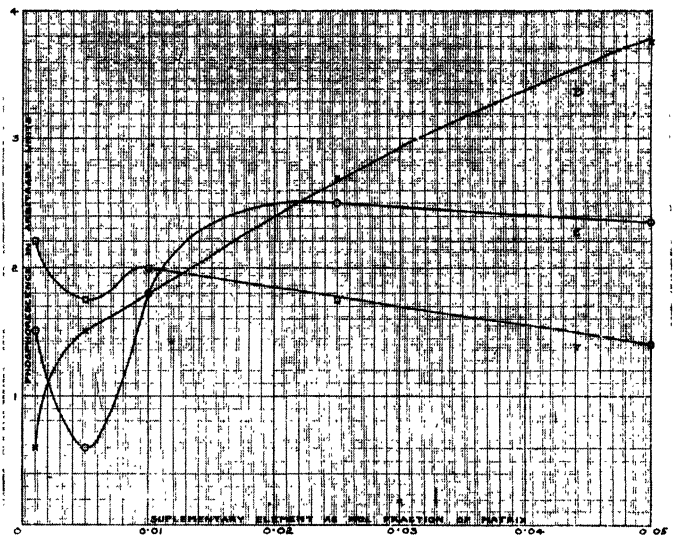


FIG. 3

the ordinates of which were derived by expressing the phosphorescence of the samples containing the electron-trap element in terms of appropriate ordinates of the D curve. *It was found that curve* E *was quite definitely not of the same family as curve* D.

The reliability of curve E was confirmed by taking portions of the five samples containing the electron-trap element, and submitting them to a further temperature treatment to modify their phosphorescent properties. The phosphorescence of the resultant five new samples was then compared with the

phosphorescence of five samples containing no electron-trap element, and a curve F obtained from these observations. *Curve* F *was found to be different from curve* E, *but clearly of the same family.*

Now, it has been established that the introduction of the electron-trap element into the phosphor produces *a change* in its phosphorescence. This change might be due to one of three causes: (*a*) the electron-trap element not entering into the lattice and reducing the amount of supplementary element within the lattice; (*b*) the electron-trap element entering into the lattice without affecting the amount of supplementary element; (*c*) the electron-trap element entering into the lattice and, at the same time, displacing some of the supplementary element from the lattice. If (*a*) was the case, curves E and F would be both of the same family as curve D. Since E and F both belonged to one family of curves, but curve D to another family, interpretation (*a*) was impossible, and the correct interpretation was either (*b*) or (*c*). There was not sufficient evidence to decide definitely between (*b*) and (*c*), but since the addition of electron-trap element increased phosphorescence for low concentrations of the supplementary element, and *decreased* phosphorescence for high concentration of the supplementary element, interpretation (*c*) appeared to be the more probable one. In any event, *the increase of phosphorescence on addition of the electron-trap element at low concentrations of the supplementary element indicated that the electron-trap element in small concentrations produced phosphorescence of this particular type of phosphor on entering its crystal lattice.*

It might be argued that the foregoing conclusion is dependent on too few experimental points on the curves, and that errors might be responsible for the apparent fundamental difference between curve D and curves E and F. Possibility of such errors was not, of course, excluded. But if such errors were present, they were not errors which could have been anticipated. All precautions which made for essential accuracy, such for example as care to reduce the amount of iron, nickel and copper below 1 part per million, or care to control furnacing temperature to within 10° C., were observed. Any errors which may have crept into preparation of the specimens must have been of an unknown and unpredictable nature. No errors of such a

magnitude could have been introduced by methods of observation and comparison of phosphorescence of the samples. If errors of unknown and unpredictable nature were present, and had vitiated the results, such errors would have been equally likely had phosphorescence been accurately photometered and had the X-ray analysis been applied to the samples. In either case, the only way to make sure that no serious unknown and unpredictable errors were present would be to repeat the experiments and see whether the results were reproducible, and far less effort was required for repetition of the experiments described than for repetition of more elaborate experiments involving photometry and X-ray analysis.

Another objection that might be levelled against the author is that even if the author's method was sound, his observations were still too few and should have been more numerous. But observations cannot, in any circumstances, be made to cover the entire possible range at frequent intervals, without waste of a high proportion of effort, if only a small fraction of the range is likely to be of real interest. Since, in any event, experiments should be repeated to make sure that the results are reproducible, the first series of experiments, for the sake of economy of effort, should be made over the entire *promising* part of the possible range with a relatively small number of observations, sufficient to indicate the part of the range of *real interest.* The repeat experiments should then cover the part of the range of real interest with observations at much closer intervals. In the tests described the portion of the range of concentration of the supplementary element which appears to be of interest is indicated as below 0·01 mol. fraction of the matrix, the most interesting part being between 0 and 0·005 mol. fraction of the matrix. Therefore the repeat experiments should cover the range of supplementary element concentration of 0 to 0·01 mol. fraction of the matrix, with smaller intervals for the portion of 0 to 0·005 mol. fraction of the matrix, and *including repeats of* 0·001 *and* 0·005 *mol. fraction of the matrix points.* It would be reasonable to make observations between 0 and 0·005 mol. fraction of the matrix at every 0·001 mol. fraction interval. If such frequency of observations had been applied in the experiments described between the limits of 0 and 0.05 mol. fraction, they would have necessitated 100 samples and 150

visual comparisons instead of 10 samples and 15 visual comparisons, and the repeat experiments would still be necessary and might still show that results were not reproducible.

The foregoing experiments have been described at some length to show how, with a little thought and care, an investigation which might involve much expensive apparatus and take six months, might be modified so as to require very little apparatus and take a couple of days.

CHAPTER X

MINIMUM NUMBER OF ESSENTIAL OBSERVATIONS

ANY SCIENTIFIC research, in its final stages, presents the investigator with two questions: Do the observations made justify the investigator in forming certain conclusions with confidence? Are the observations made adequate to satisfy critics as to the validity of the conclusions? The two questions are by no means merely different forms of substantially the same enquiry. Stronger evidence is necessary to convince the critics than to satisfy the investigator. A very enthusiastic and imaginative investigator may be satisfied by a small volume of evidence to which he attaches a high degree of significance, and a prejudiced, hostile critic will not be satisfied by mountains of evidence. In science, however, dispassionate judgment is not only an ideal which one must strive to attain, but an ideal which can be approached quite frequently in practice with a fair degree of approximation. There are, as we shall see, certain scientific conventions which both the investigator and the critics can reasonably accept and which make it possible for them to reach agreement as to validity of the investigator's conclusions without the investigator being compelled to expend an inordinate amount of effort and time in amassing the decisive evidence.

The advantages inherent in a technique enabling valid conclusions to be drawn from a minimum number of essential observations are of the highest importance. They often represent an enormous saving of time and expense which not only enables the investigator to achieve far more within the limits of time and resources at his disposal, but also makes it possible for him to carry out personally as much of the observational and experimental work as he may wish, instead of being compelled to delegate a great deal of it to assistants who might miss any finer points. Unfortunately, the advantages

of this technique have not been adequately appreciated. Indeed, no other established principle of scientific research has provoked such wide-spread hostility among its insufficiently informed opponents. Fisher, in the introductory chapter of his *Design of Experiments*, says: "In the foregoing paragraphs the subject-matter of this book has been regarded from the point of view of an experimenter who wishes to carry out his work competently, and having done so wishes to safeguard his results, so far as they are validly established, from the ignorant criticism by different sorts of superior persons." The "different sorts of superior persons" to whom Fisher refers are "professed statisticians" and "heavyweight authority". Such critics, whatever Fisher may have to say against them, are at any rate scientists. In industrial research there is a third type of "superior person" to be considered—the person who says with modest pride that he is, thank God, not a scientist but a practical man. This third type of "superior person" simply refuses to consider any arguments in favour of validity of conclusions based upon a small number of observations, and asserts that "in his experience" many hundreds of observations are necessary to establish anything at all—which is no doubt true enough as far as any efforts *he* may have made to establish any technological point are concerned.

A scientist adopting the technique of drawing his conclusions from a minimum number of essential observations should confine himself to meeting the criticism of the two types of "superior persons" mentioned by Fisher and do his best to evade arguments with the third type of "superior person", for though the scientist may find arguments for such a person he will never, to quote Dr. Johnson, find him an understanding.

Scientific conclusions based upon experiments or upon observations involving no experimentation can be regarded as valid only if (*a*) the work has been correctly planned, (*b*) if the plan has been correctly carried out, (*c*) if the conclusions are in conformity with the results, and (*d*) if no adequate alternative explanation of the results appears possible. The last of these four conditions can be satisfied only temporarily; that is to say, only within the existing state of knowledge. Manifestly, at any time new discoveries may upset previous explanations of phenomena. But progress of science is only

possible if explanations which appear at the time to be correct are accepted as correct until challenged by newly emergent knowledge. Therefore a temporary satisfaction of condition (*d*) may be regarded not only as necessary, but as sufficient as a final step in establishment of validity of conclusions reached. Even so, the possibility of establishing the validity of conclusions is not always present. Such a possibility can exist only when the investigator can plan and carry out the work in a manner which must lead to one or more definite conclusions; that is to say, when he can formulate his investigations in advance so as to enable him to decide at the end whether certain effects or certain relationships between phenomena do, or do not, exist. There are many cases where the research conducted cannot be put into such convenient form, because the results are quite unpredictable; nevertheless, in such cases also an effort must be made to form final conclusions. Conclusions formed under such unfavourable conditions may prove to be demonstrably valid, or may be merely those most likely to be correct. In the latter event it is still desirable to examine the validity of the conclusions, not in order to establish such validity, but to establish whether validity may be regarded as highly likely.

In the chapters on the planning of research we have seen how the results may be affected by errors in observation and by various interfering factors, and how such errors and interfering factors may be eliminated; we have also considered the questions of preparation and selection of specimens. Any person wishing to arrive at valid, or probably valid, conclusions as a result of experiments and observations, and unfamiliar with the technique of research involving the use of a minimum number of essential observations, proceeds to do one or both of the following things: (1) to take great pains in preparation and selection of "perfect" specimens, and (2) to make a very large number of observations for the purpose of eliminating by an averaging process the effects of any interfering factors or sources of error. Whether such a person will do the first or the second thing, or both of them, he will in any event expend an unnecessarily large amount of resources and time. The production of "perfect" specimens, absolutely alike save in regard to certain desired and strictly controlled variables, is an ideal

unattainable in practice. Furthermore, the mere increase of the number of specimens to a large value almost inevitably increases their heterogeneity, so that an increase of numbers designed to enable a more effective elimination, by the process of averaging, of variations due to causes other than those under investigation, tends to defeat the investigator's object by increasing the variations which it is desired to eliminate.

The technique of the use of minimum number of essential observations varies with the problem. Much, though not all of it, comes within the sphere of statistics or is closely related to statistics. That coming within or closely related to statistics may be described as the technique concerned with establishing the validity of scientific conclusions. There are, however, other cases when the technique is not related to statistics; in such cases the technique is concerned with demonstrating the fact that the validity of certain scientific conclusions may be regarded as highly likely. To a scientist who is, above all things, a statistician, a technique of the use of the minimum number of essential observations which cannot establish the validity of a scientific conclusion, but only demonstrate that such validity may be reasonably regarded as highly probable, does not appear attractive. There is no doubt, however, that it has practical value, in spite of the fact that it lacks the merits of rigorous proof, since it can be a most useful guide to further investigations. It is impossible to deal with the entire subject of the technique of the use of a minimum number of essential observations in a single chapter. It is, however, well worth while to consider a few typical cases, each of which may be regarded as representative of a large group.

All cases of technique of the use of the minimum number of essential observations, where validity of the conclusions is definitely established, are concerned with the mathematics of probability first developed by Gauss in his theory of errors. The theory of accidental errors is so similar to the theory of variation of properties or behaviour of a supposedly homogeneous group sample of objects, that the same mathematical expressions cover the variations in both cases. It is not possible to deal in this chapter with the theory of probability, but certain consequences of this theory are presented here, since they are

essential to explanation of the technique of the use of the minimum number of essential observations. Fig. 4 shows a probability curve of the form $y = e^{-x^2}$, which is the normal distribution of errors. Probability curves may depart considerably from the normal, as shown in Fig. 5, which presents graphically Maxwell's law of distribution of molecular velocities in a gas at a given temperature. The particular development of probability theory which is of the greatest importance for our

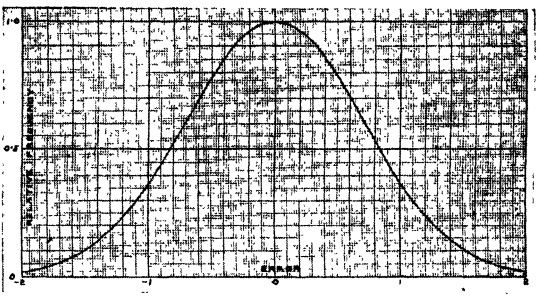


FIG. 4

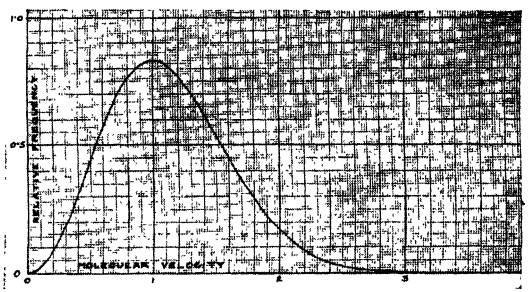


FIG. 5

purpose is that known as Student's *t*-test. The *t*-test was developed in conformity with the normal distribution of errors, and since, as Fig. 5 shows, this cannot be assumed to hold in all cases, it is essential to know whether the *t*-test may be regarded as still applicable when normal distribution of errors is not assumed. It has been pointed out by Fisher, however, that application of the Student's *t*-test to the "null hypothesis" in the cases of correctly "randomised" data may be examined for any distribution of errors, and, dealing with a specific

case involving 15 pairs of observations, Fisher demonstrated that in that particular case the difference between results of the *t*-test for a normal distribution and *any other* type of distribution was of negligible practical importance.

The "null hypothesis" is the modern equivalent of Euclid's *reductio ad absurdum.* It is applicable to any case where it is desired to ascertain whether a certain cause operating in the case of objects of a common group, for example, change of chemical composition or physical treatment of specimens of the same type, does or does not produce a certain effect. The investigator, in such an event, does not proceed to establish directly the validity of the conclusion that the effect *does* exist, but indirectly, by assuming that the effect does not exist and that statistics will show no difference between objects of the same group in which the cause is operative and those of that group in which the cause is not operative, other than that which can be accounted for by pure chance. If statistics show that there is a difference between these two kinds of objects which *cannot* be accounted for by pure chance, the *non-existence* of the effect is disproved, hence the *existence* of the effect is indirectly established.

"Randomisation" is a physical process of selection of objects to be subjected to the two treatments under comparison (such as addition or non-addition of a chemical ingredient, or different temperature treatments), and of any subsequent causes of differentiation to which the already selected objects may be submitted, so designed that the objects under investigation may be regarded as having been selected in a random order. To achieve this objective it is essential that the objects are selected in a random manner, for example, by tossing a coin or using a pack of suitably numbered cards to decide each choice, before they are subjected to the treatment the effect of which is under investigation, and that this treatment is either the *last* of the stages in the history of the objects prior to the test likely to affect their behaviour under test, or is followed only by such stages in their history prior to the test, likely to affect their behaviour under the test, as have been themselves randomised or predetermined for each object prior to the treatment under investigation.

All cases in which the validity of investigators' conclusions is

statistically established have one important feature in common: the establishment of validity does not mean that there is no possibility whatever of the conclusions being invalid. All that the statistical method can do is to show that the probability of the null hypothesis being correct is less than a certain value. This probability can be determined by plotting the curve of probability P against t corresponding to ν, the number of "degrees of freedom" characteristic of the particular experiment, calculating the exact value of t from the observed errors, and reading off the corresponding value of P on the curve. This value of P represents the probability of the value of t being exceeded either in the positive or negative direction by pure chance. If the nature of the experiment is such that the investigator is only concerned with the probability of the value of t being exceeded in the positive (or in the negative) direction the corresponding value of P is halved. The convention is to regard all values of P below 0·05 for cases when t may be exceeded either in the positive or negative direction, and all values of P below 0·025 for cases when the question is whether t has been exceeded in one direction only, as a mathematical proof that the null hypothesis is invalid in the particular case. Obviously these values are no more than conventional, and anyone at any time may insist that they are too high in a particular case, though no one would care to suggest that they are too low. The reason for the conventionally accepted limits is that, in the great majority of cases, people are quite content to be right in 95 cases out of a 100 over a large number of investigations, and take a chance of the particular case being one of the 5% mistakes. Of course, if a mistake might mean some terrible disaster, such as the death of people for whom the investigator cares most, he might consider that even a value of P below 0·001 is too high.

While there are numerous types of problems to which probability methods may be applied, it is unfortunately impossible to deal with them here. Only four types of problems are considered in this chapter, and these have been selected because they are the ones most commonly encountered and therefore most likely to be of interest to readers. The first of these four types of problems is that of ascertaining whether a particular treatment, such as addition of a chemica lor thermal

treatment, a mechanical treatment, and so on, affects the properties of specimens belonging to a supposedly homogeneous group in a manner which can be detected by one of the senses, but cannot be expressed quantitatively. This type of problem relates to cases where the sense employed to indicate the presence of a change in any property of the specimens is one of touch, taste or smell, or the so-called psychic sense the existence of which in the particular experiment may itself be questioned. The second type of problem is that of ascertaining whether a particular treatment affects specimens of a supposedly homogeneous group in a manner which can be expressed quantitatively and relates to circumstances when the experiment can be so designed that treated and untreated samples can be compared in pairs. The third type of problem is similar to the second, but differs from it in that treated and untreated samples cannot be compared in pairs, and are compared as two groups, one consisting of treated and the other of untreated samples. The fourth type of problem is that concerned with the effect upon specimens of a supposedly homogeneous group of several independent variables simultaneously present.

In each of the four types of problem it is, of course, essential substantially to eliminate both interfering factors and errors due to methods of observation which are capable of masking entirely, or almost entirely, the effect under investigation. In practice this is best achieved by taking steps to reduce such interfering factors and errors to magnitudes such that any effects they may produce in the final tests could be only of the second order of smallness. Complete elimination of such interfering factors and errors is in any event an impossibility, and even if it were possible it would have no advantage over their substantial elimination by adequate reduction of their magnitude, while it would have the disadvantages of expenditure of a quite prohibitive amount of time and effort. Interfering factors and errors due to methods of observation which are known to be incapable of completely, or almost completely, masking the effect under investigation need not be eliminated for individual specimens or observations, but must be dealt with by effective randomisation.

The problem of the type in which it is desired to ascertain whether a particular treatment of specimens of a supposedly

homogeneous group effects the properties of such specimens in a manner which can be detected by one of the senses, but cannot be expressed quantitatively, may be in one of two distinct forms. It may have the form when only the reactions of *one particular observer* are of interest, or it may have the form when the reactions of *a number of different observers* are of interest, and the reactions of any *one* of them have only a limited value. An example of the first form is the case of a subject who claims psychic powers which enable correct differentiation between pieces of paper with writing on them and otherwise identical pieces of paper without such writing, although every piece of paper is enclosed in the same kind of sealed opaque envelope. An example of the second form is an investigation of whether two types of fabric weave are equally satisfactory to the touch.

The first example admits of several methods of treatment. One method is to take n envelopes, and, strictly at random, place pieces of paper which have been written upon into some of them and plain pieces of paper into the rest. For maximum sensitivity of the test the number of the envelopes containing pieces of paper with writing upon them must equal the number of envelopes containing plain paper. It can be seen that if the subject identifies all envelopes correctly, the value of probability P of his having done so by pure chance is 1 in $C_{\frac{n}{2}}^{n}$, so that $P = 0{\cdot}05$ when $n = 6$, and when $n = 8$, $P = 0{\cdot}0149$ approx. Furthermore, if the test is repeated with $n = 8$ ten times, P will not exceed 0.05 even if only 2 out of the 10 tests show the subject's choice to be completely correct; in other words, if the subject is to be submitted to 10 tests, each involving 8 envelopes, he will not only have proved the validity of his claim to psychic powers by success in one of these tests, but confirmed it conclusively by a second success *even if the other 8 tests showed him hopelessly wrong each time.* The second method is to determine the contents of each envelope by a random process. In this case the value of P for a completely correct selection by the subject is 1 in 2^n when n is the number of envelopes, or $P = 0{\cdot}031$ approx. when n is only 5, and $P = 0{\cdot}015$ approx. when $n = 6$.

The second example, the investigation of whether two types of fabric weave are equally satisfactory to the touch, differs from the foregoing example in that the observations must be

repeated by a number of observers, and that the observers must be so selected as to constitute a representative sample of the population whose reactions to the two kinds of fabric weave it is desired to investigate.

The problem of the type in which it is desired to ascertain whether a particular treatment affects specimens of a supposedly homogeneous group in a manner which can be expressed quantitatively, and where the experiment can be so designed that treated and untreated samples can be compared in pairs, may be exemplified by the case when it is desired to ascertain whether a modification of exhaust technique has an effect upon the efficiency of certain discharge lamps after a given number of hours of normal running. A batch of unexhausted lamps, supposedly homogeneous in regard to previous treatment, is taken, and n lamps are selected for normal exhaust by a randomising process. For each lamp so selected, a companion lamp, to be exhausted by the modified method, is selected by a randomising process, the two lamps constituting a comparison pair. Exhaust is not the last treatment to which lamps must be subjected before test; there are also the capping and ageing processes. It may be reasonably assumed, however, that capping has no effect upon the final tests. Ageing can be randomised on its own account.

If now the difference between efficiencies of lamps of a pair is x and the mean value of x for n pairs is $\bar{x}$, t is given by the equation

$$t = \bar{x} \sqrt{\frac{n(n-1)}{\Sigma (x-\bar{x})^2}} \qquad \ldots (1)$$

The number of degrees of freedom is given by the equation

$$v = n - 1 \qquad \ldots (2)$$

It is obvious that the less the variations of x, the nearer $(x - \bar{x})$ approaches zero and therefore the greater the value of t.

From Fig. 6 it can be seen that, for the purpose of establishing statistically the validity of conclusions reached, there is no advantage in increasing the number of comparison pairs above 15, and little advantage in increasing them above 11, that for lower values of n the refinements in manufacturing processes

involved which would be essential to establish validity of the conclusions increase rapidly with decrease of n, and that for $n = 2$ no amount of experimental skill can be hoped to yield a valid result.

The problem of the type in which it is desired to ascertain whether a particular treatment affects specimens of a supposedly homogeneous group in a manner which can be

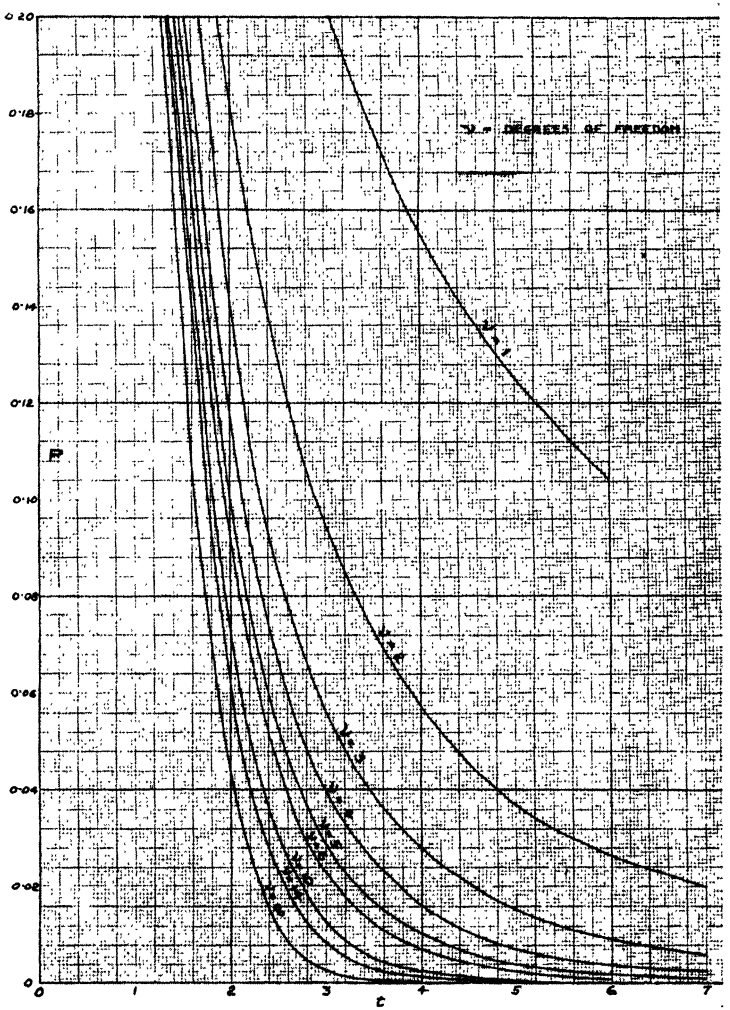


FIG. 6.

expressed quantitatively, where it is not practicable to compare the treated and untreated samples in pairs and the comparison must be made between a group of treated and a group of untreated samples, may be exemplified by a case where it is desired to ascertain whether the life of certain discharge lamps is affected by change of treatment of the wire used in manufacture of the coated electrodes of such lamps. Here the treatment, the effects of which it is proposed to investigate, occurs very early in the history of the samples and is followed by a large number of other treatments which have an effect on the final test results. It is therefore preferable, for practical reasons, to make the comparison between n_1 lamps using one kind of wire and n_2 lamps using the other kind of wire.

If the life values of individual lamps of one group are denoted by x_1 and their mean value by $\bar{x}_1$, and the life of individual lamps of the other group are denoted by x_2 and their mean value by $\bar{x}_2$, t is given by the equation

$$t = (\bar{x}_2 - \bar{x}_1) \sqrt{\frac{n_1 n_2 (n_1 + n_2 - 2)}{(n_1 + n_2)\{\Sigma (x_1 - \bar{x}_1)^2 + \Sigma (x_2 - \bar{x}_2)^2\}}} \quad \ldots (3)$$

For this case

$$\nu = n_1 + n_2 - 2 \quad \ldots (4)$$

Before passing on to the next type of problem, it is worth while to refer to the frequently employed practice of rejecting any value of x_1 or x_2 which exceeds 5 times the "probable error" of the group, which is taken as

$$= 0{\cdot}6745 \sqrt{\frac{\Sigma x^2}{n - 1}} \quad \ldots (5)$$

To the author the wisdom of this practice appears to be exceedingly doubtful. Its basic assumption is that the mere fact that $x > 3{\cdot}3725 \sqrt{\frac{\Sigma x^2}{n - 1}}$ proves that this value of x is due to an interfering factor absent from other observations. This might well be the case, but it need not *necessarily* be the case. The interfering factor may be present in all readings, though for various reasons to a lesser extent. If a reading is to be rejected because it is invalidated by a grossly interfering factor, such rejection must be based upon the knowledge of existence of that factor in the particular case and its absence from other

readings. If such positive knowledge is lacking and, particularly, if several readings differ considerably from $0{\cdot}6745\sqrt{\frac{\Sigma x^2}{n-1}}$, the investigator should not cheerfully adopt the course of rejecting one or more readings which happen to exceed $3{\cdot}3725\sqrt{\frac{\Sigma x^2}{n-1}}$, but consider carefully whether it would not be best to reject *all* the readings, trace the interfering factor, and start again.

The method of dealing with the fourth type of problem is of particular interest. The so-called "factorial design" of experimentation which enables the investigator to deal with the effects of several simultaneously varying factors, is due to Fisher. It represents a remarkable advance in research technique which previously was based on the doctrine that in experiments it was permissible to vary only one factor at a time. Fisher's own comment on the subject is: "In expositions of the scientific use of experimentation it is frequent to find an excessive stress laid on the importance of varying the conditions *only one at a time*. . . . This ideal doctrine seems to be more nearly related to expositions of elementary physical theory than to laboratory practice in any branch of research. . . . In the state of knowledge or ignorance in which genuine research, intended to advance knowledge, has to be carried on, this simple formula is not very helpful."

An example illustrating the problem in a simple general form is that of the effects produced upon the properties of a substance by a number of different ingredients which may be present in various proportions and may, or may not, interact. If the number of different ingredients under investigation is n and each of these may be present in any one of m possible proportions, the total possible number of mixtures, of which no two are alike, is m^n.

It should be observed that the above generalisation does not imply that the *possible proportions* in which an ingredient may be present is the same for every ingredient, but merely that the *number of possible proportions* for each ingredient is the same. Thus, for example, if the number of possible proportions is m, the first ingredient may be present in proportions a, $2a$, $3a$ . . . to m terms; the second in proportions b, $1{\cdot}5b$, $2{\cdot}0b$,

$2 \cdot 5b$ . . . to m terms; and the third in proportions o, c, $3c$, $5c$ . . . to m terms, and so on.

If the test is repeated R times, the total number of mixtures will be Rm^n.

Mixtures involving a particular ingredient in a particular proportion will occur Rm^{n-1} times, and these can be compared, as far as the effect of the particular ingredient is concerned, with $R(m-1)m^{n-1}$other mixtures, making a total of Rm^n mixtures available as a basis for the final conclusion.

For *any* proportion of the particular ingredient, a comparison can be made with an equal number of mixtures containing *one* of the other proportions of that ingredient for Rm^{n-1} *pairs* of mixtures, and the effect of the ingredient can be ascertained from $\frac{m(m-1)}{2}$ *pairs of proportions*, making a total of $\frac{Rm^n(m-1)}{2}$ comparisons. In the simplest case, when $m = 2$, the total number of comparisons is $\frac{Rm^n}{2}$, or, substituting for m, $R2^{n-1}$.

Besides the effects of single ingredients, there are the possible effects of interactions of these ingredients. Taking the case of interaction of two ingredients, A and B, since either of these can be present in any of the m proportions, one particular proportion of A, present in Rm^{n-1} samples, involves Rm^{n-2} samples having a particular proportion of B, which can be compared with an equal number of samples having the same proportion of A but a different proportion of B, giving Rm^{n-2} comparisons. As, however, there are m proportions of B in samples having the particular proportion of A, this proportion of A gives $Rm^{n-2}\,\frac{m(m-1)}{2}$ comparison pairs. But there are altogether m possible proportions of A. Hence the total number of comparisons available for study of effect of interaction of A and B is

$$Rm^{n-2}.\ \frac{m(m-1)}{2}.\ m, \text{ or } R\frac{m^n(m-1)}{2}$$

as in the case of study of effect of a single ingredient. Similarly the number of comparisons available for investigation of effects of interaction of any number of ingredients can be shown to be $R\frac{m^n(m-1)}{2}$.

Altogether the factorial design enables the study of the following effects:

Effects of single ingredient	C_1^n
Effects of interaction of 2 ingredients . .	C_2^n
Effects of interaction of 3 ingredients . .	C_3^n
Effects of interaction of n ingredients . .	C_n^n

The total number of effects which can be thus studied is therefore

$$n + \frac{n(n-1)}{2} + \frac{n(n-1)(n-2)}{6} \quad \ldots 1$$

The important point is that each of these effects can be determined on the basis of Rm^n specimens, although only Rm^n specimens have been used altogether. The advantage of the foregoing may be illustrated by an example of a compound in which 4 ingredients are each present in one of 2 possible proportions, and the test is repeated 3 times. There are in this case 15 effects to be investigated, and, using the factorial design, this can be achieved with 48 specimens. If the old technique of varying "one thing at a time" were employed, the number of specimens necessary to arrive at the result with the same degree of precision would be 720.

The validity of conclusions with regard to all the effects investigated can be established by application of the *t*-test, in the manner already described, for each ingredient and for each interaction of several ingredients, precisely in the same way and with the same degree of certainty as would be obtained if the effect in question were the *only* effect under investigation, and all the samples employed served *only* to elucidate that effect.

So far, the types of problems considered in this chapter have been those in which the validity of the conclusions could be established by statistical methods, subject to acceptance of certain conventions. There are, however, other types of problems in which it is not possible to establish the validity of the conclusions reached by application of any rigorous mathematical treatment, but it is possible to arrive, on the basis of a minimum number of essential observations, at conclusions which are likely to be valid.

The following problem, with which the author had to deal a good many years ago, furnishes an example of the method. The problem was to ascertain the correct operating temperature for tungsten filaments of gas-filled lamps, corresponding to maximum efficiency compatible with specified useful life. The data available consisted of measurements of voltage, current and candle power of various lamps throughout their

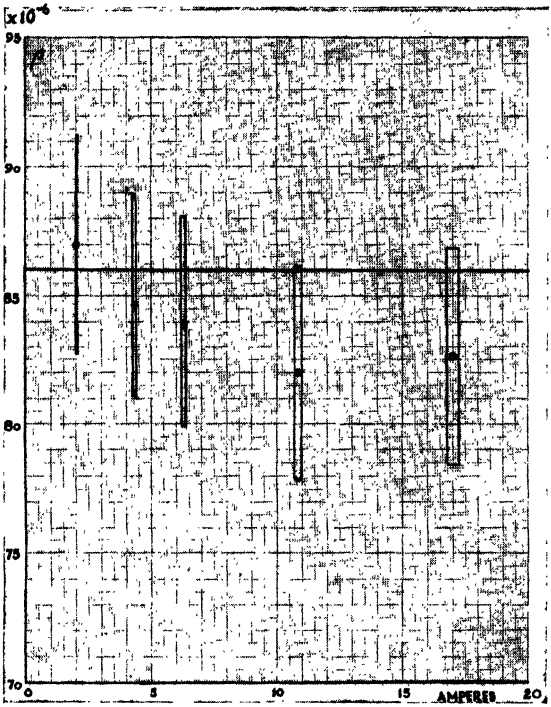


FIG. 7.

life, and of unused sample filaments belonging to batches of filaments employed in manufacture of the particular lamps. The relationship between ϱ, the specific resistance of tungsten, and temperature was known and a formula was available for correcting the observed values of ϱ for filament end losses. The correction formula was however an expression involving both filament voltage, which was known, and temperature of the middle section of the filament, which had to be determined and was merely known to lie within certain limits. Average

values of ϱ for filaments of batches of lamps exhibiting maximum efficiency at specified useful life, uncorrected for filament end losses, were plotted against average values of initial current for these batches of lamps. Five points, shown in Fig. 7, were obtained. The batches of lamps were relatively small, and the points were obviously subject to errors. The question was, could this data be made to yield adequate information about the value of ϱ corresponding to the middle region of the filament of each type of lamp, and so provide information regarding the temperature in that region?

It occurred to the author that the desired value of ϱ could be obtained from the data if allowance were made for the magnitude of possible errors in determination of ϱ and of filament current. The only assumptions that were necessary were that, since gas-filled lamps fail not through evaporation of tungsten but through distortion of the filament coils, ϱ should be a constant for *all* lamps which exhibited a maximum efficiency at specified useful life, and that since losses due to leads and supports were visibly greater with heavier filaments, observed ϱ for these was more likely to be too small than for finer filaments.

The errors in measurement of current were $\pm$ 0·125%. The errors in calculated values of ϱ were made up of errors in determination of filament voltage, current, diameter and length. Furthermore, ϱ was subject to an error due to end losses. Altogether ϱ could be regarded as correct to within $\pm$ 5%. The points on the graph could not be regarded as satisfactory presentations of actual data, since they took no account of the above-mentioned limits of accuracy. Each point was therefore enveloped by a rectangle whose boundaries were determined by the limits of accuracy and within which the true point might be considered to lie. A line parallel to the X axis was now drawn *so as to pass through all the rectangles and so as to be consistent with least percentage error for finer filaments and maximum negative percentage error for heaviest filaments.* The value of ϱ given by this line was taken to correspond to the true value of ϱ for the middle region of the filament in all types of lamps represented by points on the graph. Subsequent experiments showed that this result, which at the time of the experiment could be regarded as only likely, was in fact valid for all

practical purposes. An interesting aspect of this particular problem is that a mean curve through the experimental points would have given an entirely misleading result.

The last example does not, of course, represent more than a method of scientific guesswork. In this it differs sharply from the previously considered statistical methods. But it has a very definite value in research, for in cases such as that given in the example, scientific guesswork does lead to conclusions which can be verified in a subsequent test, while in the absence of such guesswork there can be no conclusion worth verifying.

One more type of problem deserves consideration: a problem of the kind in which results obtained cannot be predicted and where the number of available observations is small and does not enable a statistical test of the validity of the conclusions reached, but in which it is possible to form an opinion as to the likelihood of correctness of the conclusions by analysis of the data.

An example of the last problem is provided by a small experiment which the author carried out some years ago. It was desired to ascertain quickly and with the minimum amount of trouble and expense how the covering power of a double coat of paint, composed of a powder suspended in a nitrocellulose medium, would be affected by additions of varying quantities of a certain solvent, having no effect upon the suspended powder, the coating being carried out under certain standard conditions of application and drying. Only ten samples were prepared. Five of these were single-coat samples, each corresponding to a different percentage content of added solvent. The other five samples were double-coat samples, corresponding in percentage content of added solvent to the single-coat samples. All samples were prepared on a glass surface, by flowing the paint over the surface at a uniform rate and drying under standard conditions, with a fixed time interval between application of the second and the first coats in the case of double-coat samples. The quantity of powder deposited per unit area in the middle region of the coated surface was approximately determined for each sample, and powder quantities so determined plotted against percentage content of added solvent. The results obtained are shown in Fig. 8. It was observed that while values obtained for a single coat were

represented by points on a smooth curve, the values obtained with two coats were either subject to very large errors, or corresponded to a complex curve with a pronounced minimum. If the first interpretations were adopted, the experiment was useless, and a more elaborate and extensive investigation would be necessary to arrive at any conclusions. The author took the view, however, that the consistency of the results obtained with a single coat indicated that the results with a double coat were not due to very large errors, but to the complexity of the effect, and that the values obtained indicated that, for

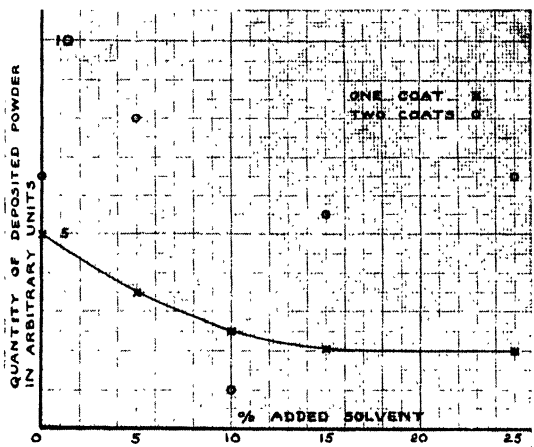


FIG. 8.

a double coat, additions of the solvent up to 5% were actually beneficial, but harmful for larger additions. Such a conclusion would, of course, be far too speculative, unless strong supporting evidence could be extracted from the experimental values obtained. Powder quantities corresponding to the second coat were obtained by subtracting single coat from double coat values for various solvent additions. The results are shown in Fig. 9. These results, *assuming that very large errors were absent*, indicated that the application of the second coat had two opposing effects—the effect of deposition of additional material on the surface, and the effect of dissolving and possible removal of the first coat. Such conflicting effects might

reasonably be expected. The next stage was to consider the implications of data presented in Figs. 8 and 9.

The first step was the interpretation of the curve for a single coat, in Fig. 8. Why did the quantity of deposit decrease with addition of solvent? Obviously, this effect could not be due to the addition of solvent making the paint more capable of retaining the suspended powder. It was quite probable, however, that the addition of the solvent made the paint *less* capable of retaining the suspended powder, with the result that *by the time the flowing paint reached the middle region of the surface, it was partly denuded of powder.* This would explain the

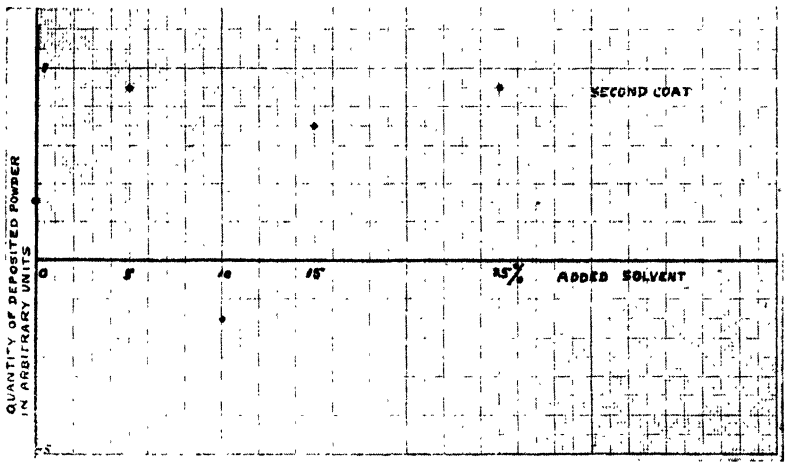


FIG. 9.

shape of the curve for a single coat and would indicate that, with addition of the solvent, the rate of powder deposition increased, and that, at the same time, the rate of powder deposition was dependent on the powder content of the paint, so that when larger particles had been deposited the finer particles would be retained even in spite of increase of the amount of the solvent. This would be consistent with the results obtained for a single coat at 15% and 25% solvent content, on the assumption that at and above 15% solvent content all the heavier powder particles left the solution immediately the paint began to flow, and finer particles were left in suspension and deposited uniformly over the time interval during which

the paint flowed over the surface. It would also be reasonable to assume that a *very small* addition of the solvent would not produce much effect, since there must be a certain degree of tolerance in composition of any paint.

The solubility of the deposit by the second flow of paint would obviously depend on the solvent content. It would be reasonable to assume that *small* additions of solvent would not

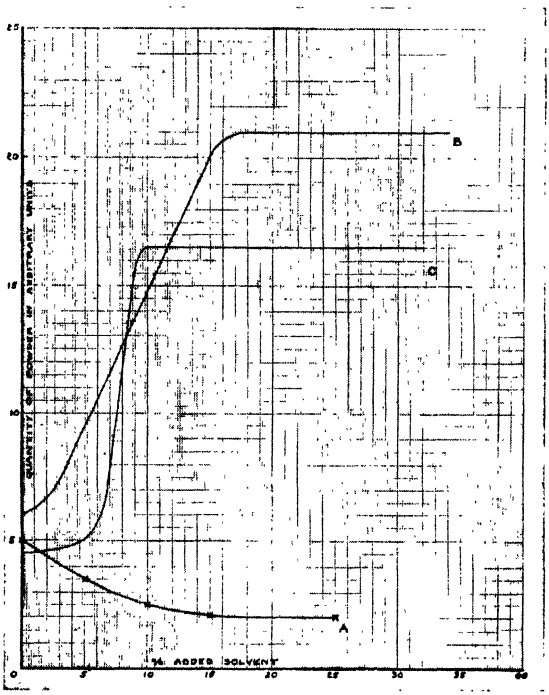


FIG. 10.

increase the rate of solubility very much, but that as the solvent content increased the rate of solubility would increase rapidly *until the deposit was so far affected that further increase of solvent content could not make any appreciable difference.*

The *gross* deposition of powder in the middle region of the surface by the second flow of paint would be higher at any value of solvent content than for the first coat, since the second flow of paint would be enriched by powder from dissolving of the first coat. A sensibly constant rate of *gross*

deposition might be reasonably expected to occur at and above 15% solvent content. All *gross* deposit values for the second coat would, according to this argument, be higher than for the first coat at the same solvent contents, and, for higher solvent contents, considerably higher.

The *net* deposit values for the second coat would be the differences between the *gross* values and the *amounts dissolved* and should correspond to data in Fig. 9. Since *gross* deposition of powder and its removal by solution during second flow of paint took place simultaneously, the maximum values of either could exceed the powder deposit values for the first

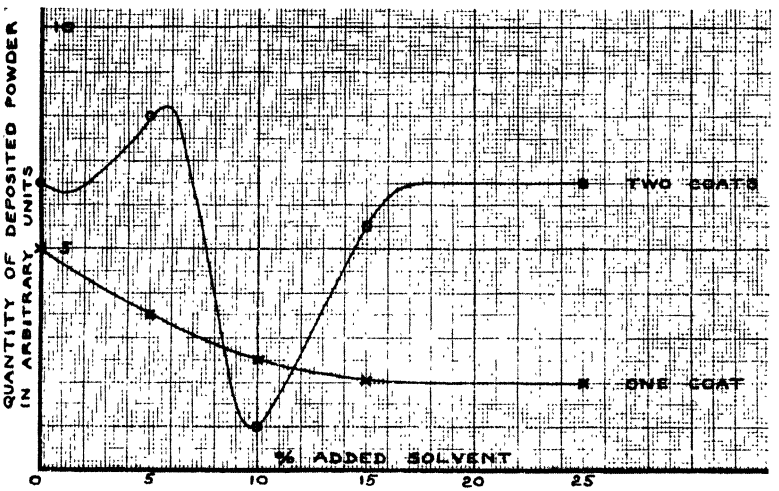


FIG. 11.

coat and might be several times greater, provided the *net* deposit did not have a negative value numerically greater than that of the first coat powder deposit at the same solvent content, or a positive value greater than *any* value obtained for the first coat.

Fig. 10 shows three curves of which A is the actual observed curve of powder quantity variation with solvent content for a single coat, B is a hypothetical curve showing the relationship between quantity of powder removed by second flow of paint and the solvent content, and C is a hypothetical curve showing the relationship between *gross* powder deposition and the solvent content for the second flow of paint. The question is,

can B and C be so drawn that their differences give results in agreement with Fig. 9? Apparently this is quite possible, as may be seen from Fig. 11, in which the computed curve for a double coat, derived from B and C, is superimposed on the experimental points for a double coat.

The result shown in Fig. 11 was reached by logical reasoning concerning the experimental data available. It cannot be interpreted as a *proof* of the validity of conclusions reached by the author in the first place. *But it does show that such conclusions were likely to be correct.* Indeed the likelihood might be assumed to be high, since the chances of fitting a theoretical curve of such complex shape to observed points in spite of the theory being erroneous would obviously be remote.

Needless to say, the technique described above must not be used indiscriminately, and, if at all possible, its results should be subjected to confirmatory tests. But, even if confirmatory tests are for some reasons impossible, the technique has a definite value, since it may be the only alternative to complete rejection of experimental data involving a small number of observations, or to embarking on experiments for which neither time nor resources are available.

CHAPTER XI

PATRONS

SCIENTISTS do not conduct their researches in a social vacuum. Research, however imperishable its achievements, cannot itself create for the scientist engaged in it the barest minimum of food, shelter and other necessities of life. The means of livelihood, the comforts and the safeguards against destitution in old age, come to the scientist from patrons who, for good or bad reasons, think it worth while to support him while he expends the resources they have provided on research work of which they approve. The nature of this relationship between a scientist and his patrons is unique in that it differs qualitatively from the relationship between any other productive worker and those who pay for his labour. A farmer, a factory worker, a doctor, a sailor, a teacher, a bricklayer, all do work the value of which can be immediately assessed. Whether it is or is not fairly rewarded is beside the point. The point is that anyone paying any of these producers of wealth can judge fairly accurately what the payment is for and whether it is worth more or less than the work performed. A ship owner can estimate his profits, and so can a factory owner or a builder. A patient knows whether the doctor whose fees he pays is doing something to cure or mitigate an illness. A teacher is known to teach so many pupils in return for his salary. But the results of research are at best only vaguely predictable, and may indeed be wholly unpredictable. Patrons who support scientific research are in effect buying tickets in a lottery in which they may win a great prize, a consolation prize, or nothing at all. No doubt there are people who buy lottery tickets for the sake of the object for which the lottery is promoted and do not worry unduly about the smallness of their chance of winning any prize. Similarly there are patrons who support scientific research without any hope of benefiting therefrom. But such disinterestedness is not commonplace—

which is the reason why lotteries for noblest objects offer handsome prizes.

Any men or women who wish to devote themselves to scientific research must therefore seek to know and understand the ways of patrons without whom they cannot hope either for research facilities or a livelihood, unless they are prepared to earn their living in some other way, and do research in their spare time with their own meagre resources. Who are these indispensable patrons, what do they want, what are they prepared to do to help research, and what conditions do they attach to such help? To-day such patrons may be roughly divided into national bodies, such as governmental or local organisations interested in promoting certain kinds of research; industrial organisations; organisations partly private and partly public; and private individuals. The assistance which these patrons give to scientific research may take the form of endowments, of special grants, or of continued financial support constantly reviewed in regard to amount and objectives. Conditions vary greatly from country to country and, in the absence of a specific statement to the contrary, readers may take it that the remarks in this chapter relate to conditions in this country and to the present period.

There are cases when resources allocated for research are allocated practically unconditionally; in such cases all a research worker need do is to satisfy the responsible authority, usually a university professor, that the proposed research is worth doing, and that the would-be investigator has the necessary capacity for the proposed investigation. In other cases, notably in industrial research, resources are allocated conditionally, and the conditions are closely specified and rigidly enforced. In still other cases, resources are apparently allocated unconditionally, but a closer investigation reveals implied conditions which must be observed: this is the position, for example, in the case of grants by an industrial organisation for university research, when the grants are accompanied by an expression of hope, formal or informal, that certain scientific problems might receive attention—such "hopes" must not be disappointed, or the grants will dry up.

A brief survey of the general position soon shows that it is by no means easy to estimate the total magnitude of resources

allocated to research or the manner in which they are apportioned. It is possible to make some estimate of the total amount of money spent in national research organisations, such as the Atomic Research Station, the Post Office Research Department, and the Royal Naval Scientific Service, though in this case a satisfactory total could not be obtained without access to documents not open to public inspection. In regard to university research, far more detailed information is available, but the expenditure on this research is only a comparatively small part of total research expenditure for the whole country. As for industrial research, the information concerning the funds expended upon it is wholly inadequate; one need only refer to the "Industrial Research" Year Book to confirm this fact.

Various attempts have been made to construct a complete picture from this fragmentary data. For example, attempts were made to divide research into fundamental and non-fundamental, and assess the amount of each by the number of scientific papers published. This method appeared to have some arguments in its favour prior to inception of atomic research and the enforced secrecy that accompanied it, but even at that time it involved two serious misconceptions. It involved an assumption that fundamental research was confined to universities and government establishments, and that industrial research was limited to non-fundamental problems; it also involved an assumption that published papers and patent specifications emanating from industrial research establishments presented a fair and complete picture of what was being done there.

The error implicit in the first misconception is readily revealed. It is obviously impossible to confer the title of fundamental research on work done by every post-graduate university student which finds publication and deny this title to work done on spectro-photometry at G.E.C. or on anti-malaria drugs at I.C.I., let alone such classical work as that of Langmuir at the research laboratories of American General Electric.

The second misconception is based on the assumption that industrial organisations are as eager as universities to publish all their discoveries. Nothing could be further from the truth.

Industrial organisations permit publication only of papers which do not give away anything advantageous to important competitors. Their patent specifications are framed so as to comply with requirements of patent law in regard to validity without giving away more than may be essential. It is a condition of validity of a patent that its specification should describe a process or a device in a manner which would make the working of the patent practicable to anyone "skilled in the art", but the term "skilled in the art" is delightfully vague. It is not necessary to incorporate in the patent either the theory or the description of the experimental research which led up to the final result, and, indeed, the Patent Office discourages such verbosity. Last, but by no means least, industrial concerns have invariably an accumulation of process data which they do not find desirable to publish even in patent specifications, either because it is not patentable or because infringement would be difficult to prove.

Research work of industrial organisations is like an iceberg—only one-eighth exposed to view. Since the government has gone in for atomic research in a big way and has imposed rigid restrictions on publication, this iceberg quality has been imparted to research work at government establishments and even at university laboratories—excepting, of course, matters of no conceivable importance in connection with military or civilian war-time problems. All this makes estimation of amount of support, given by various patrons in various fields of science, extremely difficult.

The latest and most reliable report of the financial position of scientific research has been published by the Association of Scientific Workers in August, 1948. According to this the expenditure figures for research and development are:

Government expenditure on military research	£67,185,500
Government expenditure on industrial, agricultural and medical research	£11,284,451
Industry's expenditure on research and development (F.B.I. estimate)	£30,000,000
TOTAL	£108,469,951

According to the same report, however, the figures of scientific staff occupied in research and development during 1947–8 are:

Government military research . .	2,836
Government, industrial, agricultural, and medical research	1,608
Industrial (research associations) . .	1,175
Private industry (F.B.I. estimate) . .	10,000
TOTAL	15,619

It is difficult to reconcile the scientific staff figures with the expenditure figures.

Perhaps a more useful approach to the question of patrons is to leave aside the question of *how much* research different patrons support and to confine oneself to examination of *what kind* of research they support, and upon what terms.

As far as the government is concerned, its support of research is primarily associated with war problems, though a part of such research, such as radar, has important peace-time applications. The conditions attached to "war problems" research are pursuit of allocated investigations, absolute secrecy, and reliability of character, the last qualification being at present interpreted as not holding certain political views. Research workers to whom this type of research, and the terms upon which it may be carried out, appeal, should find no difficulty in getting posts in appropriate organisations, subject to satisfactory qualifications, and should not find it hard to satisfy their patron. Other research workers, who do not like war research, or the terms attached to it, will no doubt look elsewhere.

While "war problems" occupy first place in government-supported research, an imposing, though secondary place, is accorded by the government to research designed to aid industry, and a rather less important place to agricultural and medical research. These secondary and tertiary objectives of government research policy are principally pursued through research establishments of D.S.I.R.—the Building Research Station, the Chemical Research Laboratory, the Fire Research Organisation, the Food Research Organisation, the Forest

Products Research Laboratory, the Fuel Research Station, the Pest Infestation Laboratory, the Road Research Laboratory, the Water Pollution Research Laboratory, the Geological Survey of Great Britain, and the National Physical Laboratory. In addition, there are a number of government organisations not under D.S.I.R., such as the Agricultural Research Council, the Forestry Commission, and the Medical Research Council, who are concerned with the government's tertiary research objectives. Finally, there are a large number of organisations, classified as private, but under the aegis of D.S.I.R., each concerned with specialised research on behalf of firms who finance it and who belong to the same branch of industry: such organisations are, for example, the British Cast Iron Research Association, the British Cotton Industry Research Association, the British Electrical and Allied Industries Research Association, the British Refractories Research Association, and the British Scientific Instrument Research Association. All these government, or private but government-supported organisations have certain features in common—they all favour fundamental research, and they all encourage publication of the results of research conducted, except for research relating to war problems. The important difference is that while wholly governmental organisations not only permit but encourage their scientific staff to publish their researches to the world, the private organisations under the aegis of D.S.I.R. confine their publications to reports circulated among their members. All these organisations, however, with the exception of matters connected with national health, are guided by the needs of industry, and though the scientists within these organisations may to some extent impose upon industry a wider and more progressive view of purposes and scope of research, industry, which thus emerges as a patron of these organisations, is in a far better position to impose *its* views on the subject.

Probably the happiest place for scientific research is at the universities. This is demonstrated by data given by Bernal, in his *Social Function of Science*, concerning the number of scientific publications emanating from academic, governmental and industrial sources. Bernal's figures, unfortunately pre-1939, show an overwhelming preponderance of

publications from academic sources over publications from all other sources. Actually, for reasons already given, it is impossible to say exactly how much research, fundamental or non-fundamental, is being carried out in the country at present—it is only possible to comment on the appearance of the visible part of an iceberg. But it is still true that of the research work *published*, by far the most emanates from university laboratories. And that means that in one respect at least universities are the happiest place for scientific research, since no one who does creative work can fail to be delighted if it is known and of value to many, or fail to be disappointed if it must remain hidden.

There is another reason why universities may be regarded as the happiest places for scientific research. Their funds for research come from three sources: ancient bequests, grants from government and municipal bodies, and private firms and individuals. Of the first source it can only be said, that of all patrons, dead patrons are the least likely to criticise the professors' choice of subject and manner of research. Of the second source it may be said that, while government and municipal bodies may hold varying views, they all agree in regarding the universities as imparters of higher education, and the research conducted by them, excepting research on war problems, as interlinked with such education—a far more progressive attitude than that adopted by the same municipal and government bodies with regard to the establishments designed to aid industry, since there is more in common between great research and the learning of science than between great research and high industrial dividends. Of the third source—the private firms and individuals—it may be said that their motives are mixed; sometimes their support is intended to help the solution of problems of their industry, sometimes it is designed to aid the training of young men for their own laboratories, sometimes it is hoped to enhance reputation or gain honours, sometimes it may even be a completely disinterested gift. This is very different from the attitude of the same firms and individuals when they finance such organisations as private research establishments under aegis of D.S.I.R., in which case they expect direct benefits to be shared with as few others as possible.

There is yet another reason which makes the universities the happiest place for scientific research. A great proportion of research conducted there is carried out by young men and women supported by grants (which are frequently meagre and never available for more than a few years.) As Sir William Bragg said in a presidential address to the Royal Society, these young scientists "are encouraged by these financial aids to devote the most ingenious years of their life to scientific research" and include "some of the most brilliant young men in the Empire". These young scientists are fortunate to be so employed under the guidance of university professors who have devoted their life to science, and the professors are fortunate to have such research workers under their guidance.

Yet, with all these features, which help to impart an oasis-like quality to university research, such research is not completely divorced from industry, since it is industry which provides most of the financial resources for its prosecution. The "Industrial Research" Year Book gives a comprehensive list of research grants which industry has placed at the disposal of the universities. Some of these are substantial, and enable a young scientist to pursue research in comfort for a number of years; within this category come, for example, 80 I.C.I. fellowships of £600 per annum tenable for five years. The majority of the grants are, however, much smaller, many of only £150 per annum, and tenable for only a year or two. Bernal, examining the position, remarks "actually the first, and in many ways the most fruitful, years of research are clouded for the great majority of research workers by complete material insecurity". Young research workers know only too well that their association with university research can only be regarded as a prelude to their absorption in government or industrial establishments, or in the teaching profession. And this not only affects their attitude to the future, but encourages them, for the sake of that future, to publish many mediocre papers instead of one or two really first-class ones, since this is likely to improve their future employment prospects.

All this is quite understandable when one takes into consideration the main motive which inspires industry to provide the grants. To industry, research by young scientists in university laboratories is primarily of interest in so far as it

imparts to such young scientists a training calculated to make them valuable to industry in a few years' time—the fewer the better. Industry can employ such scientists at salaries greater than those corresponding to the grants, and derive a direct benefit from so employing them.

So far very little has been said in this chapter about industrial research. What has been said about research conducted by non-industrial organisations shows, however, that of all patrons whom a scientist concerned with research must consider, industry is by far the most important. It is the industrialists who, in the final analysis, pay the piper and can call the tune. It is therefore the ways of industry that the scientist must seek to understand, if he is to hope to get the conditions necessary for pursuit of scientific research on an adequate scale and in reasonable comfort.

What sort of people, then, are the industrialists? What do they want? In particular, what do they think of research, and why? The first, most fundamental thing about industrialists is the reason why they are in industry at all. In political publications, their motives for being in industry are described in terms of high praise or strong condemnation, according to the political views of the writer. This, however, is not a political work, but a book about research. If the readers want to know whether industrialists have wings or cloven hoofs and tails, they must search for enlightenment elsewhere. What they will get here will be merely facts without moral judgment. The first fact is that men do not become industrialists for their health, or to confer benefits upon humanity, but to do business at a profit, preferably at a very good profit. If an industrial concern cannot produce and sell at a profit, it must cease to exist unless it is nationally owned. British Railways might go on operating at a loss because the government could subsidise them in direct or indirect ways. But no private enterprise can run at a loss, or even at a negligible profit, for any length of time. Naturally, not all men who become industrialists have the capacity to make profits; but those who do not have that capacity soon cease to be industrialists. Industrialists are therefore men whose principal object is to make profits and whose chief gift is an ability to achieve this object. Many scientists are aware of this fact, but frequently imagine that

industrialists do not understand how much more profit they could make if they gave science much greater support—it is only necessary to open the industrialists' eyes to this truth and, hey presto! money will simply pour into research organisations of every kind. Nothing could be more erroneous, as a brief examination of the situation will demonstrate.

In the first place, scientists should realise that they can teach industrialists nothing about how profits can be made. With a few exceptions, such as that of Edison, who could make money when he wanted to but preferred to make it only in order to spend it on research, men with great gifts for scientific research simply do not possess the capacity for running a business at a high profit. Their whole attitude to life has a quite different basis. To a scientist money is a means towards certain ends, the most important of these being the pursuit and advancement of science. To an industrialist, money is a means of making more money—money is to be made for its own sake. A scientist will think his money well spent if it produces a great scientific discovery. An industrialist will happily forgo the manufacture of a superior product if an inferior product will give him a higher rate of profit. Scientists who, in their mind, link scientific and technical perfection with business success, remember the luxury liners and the Rolls-Royces, but completely ignore fortunes made out of jerry-built houses, cheap mass-produced goods, and adulterated foodstuffs. Improved quality *may* mean a higher rate of profit—and it may *not*. A successful industrialist knows just how much an article should be improved, or made worse, as the case may be, to give the best profit—and acts accordingly. A scientist may point out to an industrialist, with perfect justice, how ten or fifteen years of extensive scientific research may make the world a far more prosperous place—but an industrialist will not be interested in a plan which will deprive him of a profit for years and perhaps mean the extinction of his business, no matter what sort of an earthly paradise the scientist may foretell as due *after* such catastrophe. The truth is that a scientist is no more qualified to teach an industrialist how to increase his rate of profit, than an industrialist is to teach a scientist how to make scientific discoveries.

To understand the attitude of British industrialists towards scientific research, it is necessary to consider how British industry has developed. Up to the First World War, British industry enjoyed an unique position, not merely on the grounds of its financial strength but also, in the case of many products, on the grounds of excellence of quality. This position had been achieved without the aid of organised scientific research. Industry in other countries, notably in Germany, was compelled to effect drastic improvements in order to be able to compete at all. Hence, even prior to the First World War, science was called in by German industry to help it in its quandary, with the result that it was able to defeat British industry in many fields, such as that of synthetic dyes. But, on the whole, British industrialists, with the advantage of the Colonial Empire, could hold their own. Science, in their eyes, was a gentleman's pastime, unrelated to profits. After the First World War, this tradition was severely shaken. British industry began to realise that, in the face of world competition, it must revise its attitude to research, and use science more and more to aid it. Even during this period, however, many British firms made fortunes in the old way—that is, without any organised aid of science It was only the Second Wo War which finally sounded the death knell of the old tradition. In 1944, the London Chamber of Commerce, thoroughly alarmed at the state of affairs, said in its report on scientific industrial research: "In the years between the wars we were spending on research a fraction of the amount being spent in the U.S.A., the U.S.S.R., or Germany. This country cannot afford to offer the world obsolete products; it cannot, therefore, afford to neglect research. Its wares must be more attractive, more varied, and better value for money. On no other terms can it hope to support the present population of these islands."

It is one thing, however, to recognise something in principle, and quite another thing to put it into practice. Assuming in principle that scientific research was desirable, how much money should industry devote to it, in what direction, upon what conditions, under whose guidance? From the point of view of the readers it is not so important to know how industry *should* answer these questions, as how industry *is* answering them.

How much money should industry devote to scientific

research? The answer is not given by industry collectively, but by each firm individually. In each case the guiding consideration is the immediate rate of profit, or the rate of profit in the near future. A large organisation can afford, without detriment to its rate of profit, to set aside large sums for scientific research. The small firm cannot afford to do so, since its total profits are not great enough to be unaffected by such expenditure. No doubt, if *all* the firms were to devote a great deal of money to research, the total benefit would be enormous. But they are not doing this for the reasons stated, and in their present frame of mind are not in the least likely to do so, unless obliged under an agreement or by legislation. Of such agreements or legislation there is at present no sign. Hence it is only the larger firms, and particularly the largest of them, who are making really substantial contributions to research expenditure. The smaller firms are still relying on "the old tradition", on getting at the information by employing one or two chemists, engineers, or physicists with large firm experience, who might be tempted by more substantial salaries which the smaller concerns offer in such circumstances, and on luck. Even the smallest firms can have the benefit, for a small fee, of the assistance of government research organisations, such as the National Physical Laboratory, or become members of organisations such as the British Scientific Instrument Research Association. But they are tardy even in this. It is highly probable that, with increasing competition, the smaller firms will find survival increasingly difficult and will either adopt a more progressive policy towards research, or, what is more likely, simply go under. Altogether the funds allocated by industry to research are wholly inadequate. The condition to which the London Chamber of Commerce pointed with alarm in 1944 cannot be remedied on the old lines, by financing research out of income on a scale not likely materially to reduce profits. Eventually industry will be compelled to treat research on the same basis as that on which it now treats the problems of equipment or of labour costs—as an item of primary expenditure. Nothing else will suffice to meet the existing, and still increasing, superiority of U.S.A. industry. Whether the eventual conversion of industry to adequate support of research will come in time, remains to be seen.

In what direction should industrial support of research be directed? Though the answer in this case too is not given by industry collectively, but by each firm individually, the answer is unanimous. Industrialists definitely favour research specifically applied to their own particular problems, and of such research prefer that the benefits of which are, as far as possible, confined to their own organisations. Fundamental research in pure sciences does not yield any profits until its results have been absorbed and utilised by applied research. There is, therefore, inevitably a considerable interval between such fundamental research and any benefits which industry can hope to gain from it. It follows that, on the one hand, large-scale support of fundamental research in pure science is not an attractive business proposition for industrialists, and, on the other hand, there is no reason why the results of such research should not be published to the entire world; in fact, perhaps it is better from the industrialists' point of view that they should be so published, since such publication might lead to further work in the same field and bring the knowledge available nearer to the stage at which it might be turned to advantage by applied research in the industrialists' private laboratories. Only big industrialists, or big industrial enterprises, lend support to this type of research, for reasons and in ways already mentioned. Such support ranges from endowments, such as those for medicine made by Lord Nuffield, to grants such as Fellowships and scholarships. Beyond purely disinterested gifts made by individuals, or gifts made for the sake of reputation or honours, support by industry of fundamental research in pure science is limited to provision of facilities for the training of young research workers, many of whom are later absorbed by industrial organisations.

Research in applied science, or in pure science closely linked with industrial problems, attracts industrialists much more strongly. In the largest industrial organisations, research may be carried out on a scale which not only includes a good deal of fundamental research, but some fundamental research of the highest order. In such organisations there are frequently senior scientists who are allowed a great deal of freedom in the choice and method of their work. Publication of such work, however, is not free of restrictions. Industrial organisations

realise that such work may include information which further research may make very valuable in the industrial field. Less fundamental research, more closely allied to specific problems in hand, is conducted on a much wider scale, and is within the means of all large and many smaller firms. Here there is a somewhat hazy division into research which may be covered by patents and research which cannot result in such protection; the first kind includes manufacturing processes and products, while the second is chiefly concerned with methods of measurement and with investigation of properties of devices and materials. The guiding principle is at all times the possibility of immediate, or more or less immediate, profit. A very long-term policy is too much of a gamble. The problem at all times is to meet existing needs within the organisation, and existing or potential requirements of the market. Hence the guiding force for such research originates principally in the sales department, and to a less extent in the works. The salesman is regarded as the most reliable authority on what the public wants—on whether the product must be improved, or superseded by a better one, or whether the product is too highly priced and must be cheapened, even at the expense of deterioration of quality. The demands of the works are relatively of less importance, and are frequently solved without the aid of the research laboratory. Since the public generally takes time to decide that it wants something new, great innovations are less profitable than a succession of small improvements. Furthermore, another strong practical argument against great innovations is that these generally produce obsolescence of existing equipment and stocks. The largest concerns are in a better position to introduce innovations, since they can choose the time to coincide with reduction of original stocks and necessity of overhaul of equipment. Smaller firms generally have innovations forced upon them by the necessity of keeping pace with their more powerful competitors. Even the largest concerns, however, are reluctant to plunge into something radically new, however attractive.

Fear of competitors has resulted in a degree of secrecy which is positively harmful to industry. Even in the case of groups of enterprises united by agreements with regard to such matters as nature, quality and price of marketed products,

there is no interchange of research information. Consequently a piece of research completed by one firm is not merely duplicated by another, but triplicated, quadruplicated, quintuplicated—according to the number of firms interested and capable of conducting research on their own account. Eventually all firms interested rediscover what the first firm has discovered, having wasted money and time in the process, with the first firm as sole beneficiary. By this time, however, firm No. 2 has discovered something of value, and the whole process of rediscovery starts again, with firm No. 1 among the losers. In the long run firms with the best research organisations maintain an advantage over their competitors, but the industry as a whole loses.

There is, from the point of view of industry, a difference between invention and discovery when both relate to methods of production or to the product. Discoveries are not patentable. Consequently the only way to keep their benefits exclusive is by keeping them secret. Inventions are patentable, but infringement cannot always be proved. It is possible to be certain of patent protection only when the evidence of infringement can be demonstrated by examination or test of the marketed product. Consequently there are many inventions which are not patented but kept secret. In the case of patented inventions, industrialists are faced with a complex problem. Every industrial enterprise of any size has patents of its own, and has opportunities of purchasing patents or acquiring licences for patents relating to outside inventions. All industrial enterprises, particularly large ones, consequently possess and maintain a number of patents. In the case of the largest enterprises, very many patents are taken out, bought or licensed from others, and kept in force. Only a small fraction of such patents are however actually used. The patents which are not used frequently include those covering the most advanced ideas and practices. This anomaly is so pronounced that it has led many investigators to the conclusion that large concerns deliberately acquire rights to inventions in order to suppress them. Actually the position is not so simple. No industrial enterprise either takes out a patent or purchases patent rights unless it thinks the patent is of value; and if it is of value, the first thing an industrial organisation considers is whether it

can use it profitably. But as soon as such considerations start, the arguments against pile up heavily. There is the question of existing plant and stocks. There is the question of how the market will react. Therefore, while directors of an enterprise may be favourably impressed by potentialities of an invention, they have to weigh against them the warnings of the sales department and of the works. The result generally is that minor inventions, or inventions which, in another way, achieve something already achieved in patent products already on the market, get preference over really important inventions of revolutionary character, which get shelved "for consideration at a later date".

The result of this state of affairs is that the majority of great inventions which have revolutionised both industry and our ways of living have been the results of research not by large industrial enterprises but by lone inventors. Dr. Stafford Hatfield in his *The Inventor and his World* says:

"In a tabulation made by Dr. Grosvenor, only twelve out of seventy-two outstanding inventions made since 1889 have been produced by 'corporation' research. Into this period falls the invention by independent individuals of such first-rate things as monotype, case-hardening of steel, photogravure, moving pictures, dial telephones, calcium carbide, Diesel engines, carborundum, wireless telegraphy and telephony, electric train control, electric car-starters, submarines, safety razors, aeroplanes, flotation process, bakelite, gyro-compasses, etc., etc. We may add to Dr. Grosvenor's list the auto-gyro, the triode valve, Haber's ammonia synthesis, Bergius's hydrogenation of coal, television, gas-light papers (offered by their inventor, Baekeland, to the greatest photographic corporation in the world, and refused by them, only to be afterwards purchased at great expense when Baekeland had made a success of them). On the other side, we have of course a number of achievements in organic chemistry, the half-watt lamp, the Coolidge tube, and an enormous amount of improvement in materials such as photographic emulsion, steel, light alloys, cellulose and its products, few of which can really be regarded as pioneering work.

"A striking example of the victory of individualism in invention is television, worked out by a young Scotsman, with the slenderest resources, in a cellar in Soho."

The picture given in this chapter is not very encouraging to young scientists who dream of epoch-making discoveries and revolutionary inventions. But young scientists who have a real love of research, which is the only good reason for devoting one's life to it, must not be discouraged. Their road to success is not easy, but it is not impassable. The important thing is to have determination and patience. Senior research workers *do* get opportunities of carrying out first-class research at universities and publishing it. Senior research workers in large industrial organisations *do* get opportunities for first-class research. A young scientist who wants to startle the world by his first paper is courting failure. But if he takes the required steps to recognition as a capable investigator, first by publishing papers jointly with a senior, and then papers in his own name which are not particularly startling but sound and uncontroversial, and *then* presents his first really important paper, he has a reasonable chance of success. A young scientist who, either within an organisation, or as a lone worker, tries to complete and get accepted a revolutionary invention, is almost certainly facing frustration. But if he is prepared first to establish his reputation in industry by doing things the industry requires, and which may appear to him of mediocre calibre, and *then* attempt to get support for his revolutionary invention, he has a better chance. Altogether the world which is here presented may not seem to the young scientist the kind of place he would like it to be—but it is the world he lives in. If he does not like it, he must remember that it is the world made by people, of whom he is one. If it is not the world he wants, he must strive to change it.

INDEX

The author and his publishers have agreed that this book does not lend itself to a satisfactory indexing of its subject matter. Passages taken out of the context, of which they form an organic part, would produce in many cases impressions quite different from those intended.

The author feels, however, that an index of names may prove useful, not so much as an index of the book as a help to those who might wish to find how different scientists have contributed to principles of scientific research.

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