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THE FIRST EDITION OF
THIS BOOK WAS INSCRIBED
TO THE LATE

LORD RAYLEIGH, O.M., F.R.S.

TO WHOSE WORK AND WHOSE
TEACHING THE AUTHOR WAS
DEEPLY INDEBTED

PREFACE

This book is chiefly intended for those who feel interested in the methodical procedure of scientific investigation, and although some parts of it may appeal most strongly to science teachers, yet the fact that scientific method is now destined to play so great a part not only in the whole of the educational field but also in every other field of thought and activity, may suffice to make the book welcome to a much wider circle than those whose interests are confined to the laboratory and the lecture table.

The method adopted by men of science in their work is far different now from what it was in the time of Aristotle or even in the time of Bacon. Fundamentally, the main processes of the method of science are, in character, not direct but inverse, and inverse processes almost always present great difficulties. The method of science has thus been a thing of exceedingly slow growth, and even now is by no means fully developed. We smile at the methods of Descartes, who himself found serious fault with the methods of the ancients, and there is no doubt at all that our descendants will greatly improve on the methods of the present day.

There are, however, a few original thinkers in the world's history who have wrought strongly upon opinion and practice in scientific method, and who, in their day, effected enormous improvements upon the methods of their predecessors. Among these, Aristotle and Bacon stand supreme. But Aristotle was a pupil of Plato, and Plato of Socrates; Bacon's method was in strong contrast to that of his contemporary, Descartes; and Locke and Hume also played important parts in placing the method of science on firm foundations—Locke, in tracing to its origin the knowledge we believe we possess, and Hume in developing correct notions of causation. Without some brief account of the parts all these famous men played in developing

the subject now before us, it seemed hardly possible to give any adequate idea of the extraordinary difficulties that have faced one generation after another of men of science in their endeavour to formulate a method that might be of general application.

All the writers just mentioned wrote on other subjects besides method. They were philosophers. And although the other sides of their work concern us here only indirectly, when they concern us at all, it is not possible, even were it desirable, to avoid touching upon certain differences of philosophic faith, for these lie at the root of difference of method; and thus a preliminary chapter dealing with elementary philosophic notions and terminology seemed to be necessary.

After all, philosophy in some of its aspects is pre-eminently a subject for teachers who would acquire a real mastery of the principles underlying their work. Teachers have too often been content to dip into introspective psychology and deductive logic, but both subjects are now largely discredited, and the teacher would do well to make an attempt to survey a somewhat wider field. (Unless at least science teachers give some attention to philosophy they are hardly likely ever to become conscious of the weaknesses of many of the foundations of their own subjects, weaknesses which the metaphysician is only too quick to discover) No doubt the metaphysician occasionally leaves open to attack weak points in his own defence, but, on the other hand, we cannot deny that he sometimes deals a heavy blow at principles which men of science have regarded as altogether beyond challenge.

Philosophy is an exacting taskmaster, but if the teacher would only take up the work of some one philosopher and really master it, he would probably be astonished at the amount of new light that would be thrown on his professional work. Philosophy is that subject which, above all others, compels us to get down to the very bedrock on which rest the principles we profess to hold. Philosophy compels us to produce the authority for all our knowledge. Bacon, or Hume, or Mill, or perhaps Dugald Stewart, will probably appeal most to the science teacher. Plato will probably be found too visionary, and the German philosophers too obscure. William James will attract all practical men. And signs are not wanting that practical men will soon be able to derive a good deal of help also from the newer school of English philosophers, but most of the

English philosophy of the latter half of the nineteenth century will be found a rather unprofitable study; for, in the first place, it is too esoteric and technical in character, and appeals to few except to its own small circle of votaries; in the second place, it is not sufficiently independent, and has compromised too much with German idealism; in the third, it contrives to conceal its fundamental difficulties in an impenetrable obscurity of language; and, lastly, it seems to show an excessive dread of any kind of scientific law.

The aim throughout the book has been, as far as possible, to let authorities speak for themselves; and although, in quoting from many writers, the actual wording has often been modified for the sake of simplicity, clearness, and continuity, it is hoped that the spirit of the originals has always been preserved. In any case, the references have always been given, so that the reader can easily refer to the originals.

The writer has no wish to impose his own views upon the reader. He rather desires to interest the reader in the works of the various authorities cited, and to induce him to look upon this work as of a purely introductory and preliminary nature. No doubt the writer has allowed his own views to escape him to some extent: this is almost unavoidable when praising or criticizing the work of others. He has not, however, made any attempt to conceal one very strong desire, and that is to convince the reader of the paramount necessity nowadays of being perpetually on the alert to discard superseded methods, and to recognize the inner significance of all that is good in new methods and new ways of approach.

The majority of the illustrations of the principles laid down have necessarily been drawn from science. As, however, during the last ten or fifteen years, the great majority of boys and girls have, before proceeding to the university, been through a fairly substantial course of elementary science, the matter of most of the illustrations will probably be quite familiar to the non-scientific reader.

The chapters on the philosophy and on the logic of scientific method have been followed by examples of actual investigation by eminent men of science.

The section on "Scientific Method in the Classroom" includes a few instances of attempts at investigation by intelligent young pupils. These, it is believed, indicate with some degree of accuracy

the kind of success which may reasonably be expected from good average pupils who are well taught.

The author desires to express the hope that nothing he has said in the course of the book will in any way wound humanistic susceptibilities. He is strongly of opinion that there is probably no greater educational need at the present time than a reconciliation of humanist and realist ideals, If the Humanist and the Realist will but close their ranks, and cease their mutual recriminations, they may even yet succeed in checking the present general drift in the direction of an intense selfishness—a selfishness which, clamouring for its rights while refusing more and more to recognize its duties, seems to be gradually eating its way into every department of the national life.

The writer's thanks are due to Messrs. Macmillan for their kindness in allowing him to make use of (i) Jevons's *Principles of Science*, more especially for the purpose of chapters xxiii, xxiv, and xxv; and (ii) the figure on page 92 of C. L. Dodgson's *Pillow Problems*; also to Messrs A. & C. Black for their permission to make use of Mr. A. Sidgwick's books, *The Process of Argument*, and *The Use of Words in Reasoning*, more especially for chapters ii and xiii; and lastly to Mr. W. W. Rouse Ball for the figure taken from page 32 of his very interesting book, *Mathematical Recreations*.

PREFACE TO THE THIRD EDITION

Since the issue of the first edition, I have been asked from time to time not only to enlarge the scope of the first two sections but also to show how the principles of scientific method may be applied to the working out of human problems. To comply with the first request would mean an expansion of the volume to an unwieldy size, and it is in any case hardly necessary, for the reader can readily take up the references to the original sources. The second request seems to involve a task of peculiar difficulty.

The difficulty arises from the fact that, in attempting to find a solution of any human problem, values have to be considered that cannot be brought under physical law and therefore fall outside the scope of science. When we try to interpret such values in terms of objective fact, we seem to be up against a granite wall. The most abstruse problems of science seem to be simplicity itself compared with problems involving human interests. The human element is incalculable.

It is an impressive fact that the deepest thought, the most brilliant talents, the shrewdest sagacity, the most earnest and scrupulous conscientiousness, have contributed to the problems of philosophy and history, and yet have come to no common agreement. Eternal enigmas are always reproducing themselves in such a form as to defy the logician's challenge.

And yet, just as there are pre-eminently successful investigators in the world of science, so there are authorities equally well recognized for their impartial and searching investigations into problems involving the consideration of human values. Amongst these, Lord Balfour seems to hold a first place. His searching criticisms are always followed by fruitful and acceptable hypotheses. The late Lord Bryce and the late Lord Morley were also in the front rank of such men. It is a valuable exercise to take up some thesis

expounded by one of these writers and to reduce it to a chain of syllogisms. Possibly we may not agree with the ultimate premisses, but with the rigorously logical reasoning, and with the final, scrupulously impartial judgment based on those premisses, we are compelled to agree, even against all our preconceptions. To perform a similar exercise, daily for three months, with the first leading article of *The Times*, would probably conduce towards a better training in rigorous reasoning than would the reading of many books on formal logic. Speeches do not usually lend themselves so readily to the treatment. Mr. Asquith's speeches are, however, clear-cut, and models of logical reasoning. So were those of the late Mr. Chamberlain. But oratory and sound reasoning seem seldom to be good friends. If, for instance, we strip a speech of the late Mr. Gladstone of all its wealth of rhetorical ornament (as of course we must, for the purpose mentioned), the residue is a little disappointing.

I have ventured on the formal examination of three questions, two in history and one in philosophy. The known facts have been set forth, and explanatory hypotheses which seemed to be unsatisfactory have been examined and rejected. Even the hypotheses eventually accepted must be regarded as only provisional, for they are bound to involve some degree of non-calculable probability, and differences of opinion are therefore inevitable.

Finally, a short section on Relativity has been included. With the appearance of Professor Nunn's book, most of the mathematical difficulties in anything like an elementary treatment of the subject have been cleared away, but Einstein's criterion of simultaneity still seems to give trouble, and an attempt has therefore been made to work out that criterion in some detail, according to the principles of logical method. The experience of the last three or four years has shown that much of the prevailing puzzlement concerning Relativity arises from a failure to grasp the logical and historical sequence of the various topics usually included in the ordinary textbooks. In a final chapter these topics have therefore been set out in the order which seems to lend itself best to a rational development of the subject.

The reader's attention may be called again to Sir Richard Gregory's *Discovery*, a companion volume of rare interest.

F. W. W.

1st Jan., 1924.

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Me vero primum dulces ante omnia Musae
Accipiant: caelique vias et sidera monstrent,
Defectus Solis varios, Lunaeque labores:
Unde tremor terris: qua vi maria alta tumescant
Objicibus ruptis, rursusque in se ipsa residant;
Quid tantum Oceano properent se tingere Soles
Hiberni: vel quae tardis mora noctibus obstet.
Felix qui potuit rerum cognoscere causas.

Virg., *Georg.*, ii. 475 ff.

Qui tractaverunt scientias aut empirici aut dogmatici fuerunt. Empirici, formicae more, congerunt tantum, et utuntur: rationales, aranearum more, telas ex se conficiunt: apis vero ratio media est, quae materiam ex floribus horti et agri elicit; sed tamen eam propria facultate vertit et digerit. Neque absimile philosophiae verum opificium est; quod nec mentis viribus tantum aut praecipue nititur, neque ex historia naturali et mechanicis experimentis praebitam materiam, in memoria integram, sed in intellectu mutatam et subactam, reponit. Itaque ex harum facultatum (experimentalis scilicet et rationalis) arctiore et sanctiore foedere (quod adhuc factum non est) bene sperandum est.

Bacon, *Nov. Organ.*, lib. i, Aph. xcv.

BOOK I

THE PHILOSOPHY OF SCIENTIFIC METHOD

ipulated statement of supposed facts, aided by a skilful use of la guage, to give a plausible explanation by means of it. Yet this is exactly the kind of thing in which the shallower type of Humanist takes the keenest possible delight.[1]

It is curious, too, how often the Humanist seems to attach greater importance to the *results* than to the *methods* employed by the Greeks and Romans.[2] He treats a description of nature as *literature*; the words appeal to him far more than the things the words represent. "He never judges a new play by the Greek standard; he never brings his Aristotle to bear upon the politics of the day." And if, by chance, he finds under his microscope a little inexactitude in the phraseology of the Realist, he quite overlooks the deceptive nature of the magnified image, and the story of his great find he tells to all his friends for a week afterwards.

But the average Realist is just as unfair in estimating the worth of his rival's work, and always has available a plentiful supply of recent instances of the "shocking ignorance" of the Humanist over the most commonplace of natural phenomena. Yet it has to be remembered that it is the Realist who is usually on the defensive. The patronizing attitude of the Humanist is sometimes a little provocative, and his wealthier vocabulary gives him an advantage; and of course the Humanist was first in possession, and naturally has a very strong interest in the preservation of the institutions he represents.

It is remarkable how Science is sometimes misconceived, and its functions entirely misunderstood, even by those whose sympathies are known to be on its side. Here are the views of Bain, a great personal friend of John Stuart Mill: "By Science, I understand the artificial symbolism and machinery requisite for expressing the laws and properties of the world, as distinguished from the actual appearance of things to the common eye". "The symbols of Arithmetic and Mathematics generally, the symbols of nomenclature of chemistry, &c., require a peculiar cast of intellect for their acquisition. They are a class of bare forms, not remarkably numerous, which are to be held in the mind with great tenacity, and to be known as the sole representatives of all that is interesting in the world." Bain refers to two classes of "scientific minds", represented by the "extreme terms Mathematics and Natural History, the other artificial, and the concrete or real"; and he admits that,

[1] Dugald Stewart, *Phil. of the Human Mind*, pt. 1-33.
[2] Arnold, *Essays*, vol. i, p. 39, and Huxley, *Science and Education*, pp. 143, 152.

in the latter, some part of the acquisition really consists in storing up the common appearances of animals, plants, and minerals. But he is of opinion that the Chemist and the Physicist, no less than the Mathematician, "concentrate the whole of their brains upon algebraic symbols", and "immerse their minds in cheerless labyrinths of uncouth characters", to the exclusion of "all those things that gratify the various senses and emotions".[1] If a friend of Science can thus hold such extraordinarily incorrect notions, the common enemy's notions are likely to be incorrect to the point of absurdity.

Yet Bain's words should be taken to heart by every young science teacher, who would do well to remember that, once a science lesson arrives at the stage of symbols, it may cease to be Science altogether; once a "law" is established, the subsequent work is likely to take the form of mere Algebra. It is astonishing how few, even of the older pupils of a school, are able to give an intelligent physical interpretation of a formula they have established.

It is a great misfortune that "most of the present science teaching in English universities seems to be directed to cultivating the deductive faculty". In essence, the training is mathematical. The man who is working for a science degree usually takes on trust nearly all he is told in the lecture theatre. He is not put in the position of an investigator at all. He receives his information on authority; his practical work is mainly intended to verify principles he has already accepted; and his whole training is thus very little calculated to imbue him with the scientific spirit. One university is notorious for the high standard it exacts in its paper examinations for a science degree, yet the practical examinations are of so trivial a character that candidates can prepare for them by spending less than six months in a laboratory. The consequence is that many young science teachers form, at the very outset, an entirely wrong conception of what scientific method really is. How can such a teacher be expected to engage in successful heuristic teaching when he himself has never in his life undertaken the simplest piece of research work? His outlook is altogether wrong. He sets to work in school exactly as he was taught to set to work at college. For indeed, can he be expected to do otherwise? He is entirely unaware of the specific functions that science teaching is intended

[1] See Bain, *The Senses and the Intellect*, p. 444.

perform. He teaches Science just as he would teach History. He considers it his sole duty merely to pass on information. The spirit of his work is, "Believe, and ask no questions".

Such teaching is worth little. A boy is doing far more good in working his way, unaided, through a Latin author. His grammar and dictionary supply him with the "facts" he requires, and he learns from experience to suspend his judgment in regard to the author's meaning until he is in possession of these facts. He learns to reason correctly. He learns to test his conclusions. He is really solving problems by scientific method. Such work is of far greater value than the pseudo-science which finds expression in mere "lectures".[1]

It is sometimes urged that research is undesirable in the undergraduate stage, but there can be no doubt that some measure of research ought to enter into the training of every man who intends to take up science teaching. There is ample evidence to show that excellent work in this direction may be done by quite young men. One well-known Professor says, "I have always been struck by the quite remarkable improvement in judgment, independence of thought, and maturity, produced by a year's research. Research develops qualities which are apt to atrophy when the student prepares for examination, and, quite apart from the addition of new knowledge to our store, is of the greatest importance as a means of education." And successful research usually implies a complete mastery of all that is meant by scientific method.

If the young teacher desires to master scientific method, he must follow the history of Science, and see how one faulty method has been superseded by another, which, in its turn, has itself been superseded, so difficult has it been to discover the principles of the true method; he must learn to get at the exact meaning of all his terms; he must learn to eliminate all personal bias from his facts; he must ever be on his guard against hasty generalizations; he must ever be ready to give up a pet hypothesis, once the facts are against him, and he must never, on any account, allow his hypotheses to masquerade as facts,—"hypotheses are cradle-songs which lull the unwary to sleep"; he must acquire a knowledge of the laws of inductive reasoning; he must learn to balance probabilities; he must remember that even the best conclusions of Science are never more than in a high degree probable; and he must study the

[1] That is, continuous dogmatic assertion. A *teacher* (as distinguished from a *lecturer*) at the demonstration table may, of course, do work of the very highest value.

original records of successful investigators. And in all these things the following chapters are intended to help.

Let the young science teacher never scoff at workers in other fields. It is *not* the method of Science to come to conclusions unwarranted by facts, and investigators in the field of psychology have many a weary year of work in front of them before their accumulated facts will enable us to decide the precise value of the different subjects of the school curriculum. It is all a question of opinion now, and those who dogmatize on the matter are putting themselves quite outside the pale of scientific method.

A classical training is admittedly productive of one great advantage over a training in Science, and that is in the power it confers in *the balancing of probabilities in which the human element is dominant.* Science can make no provision for this, though of course both History and modern Literature can.

Science has, however, one enormous advantage over all other subjects. All facts can be obtained at first hand, and without resort to authority. The learner is thus put in the position of being able to reason with an entirely unprejudiced mind. It is this possibility of *self-elimination in forming a judgment* that must be regarded as the greatest possible specific result of science teaching.

The stray Humanist who still claims that a classical education is the only liberal education, we can afford to ignore. Centuries ago the claim must have been admitted, for then a classical education was the only education. But modern civilization has now lifted itself far above the ancient civilization, and its most characteristic feature is the enlightenment that has come from the great mathematical and physical researches of the last two or three centuries, an enlightenment which has permeated not only the practical arts and industries, but is also gradually finding its way into all fields of thought,—Philosophy and History, Sociology and Literature.[1] A Humanist has few claims to culture nowadays, if he confines his tillage to the ancient fields.

Probably the most successful teachers, in any subject, are those who have by research reached the inner life of something, great or small. The golden key of the investigator is mastery. But mastery does not come by listening, while somebody explains; it is the reward of strenuous effort. Mastery comes by attending

[1] Cf. Mach, *On the Classics and the Sciences*, pp. 338-74

long to a particular thing,—by enquiring, by handling and doing, by contriving and trying, by ordered system and good method, and especially by cultivating the habit of distinguishing between the things that signify and those that do not.[1]

CHAPTER II

Words and their Elusiveness

§ 1. Uncertainty in the Meaning of Words

The late Professor William James' squirrel problem[2] appears to be the lineal descendant of one propounded many years ago by a well-known Scottish metaphysician:—

> "A cart-wheel is placed horizontally upon a vertical axle. Seated on the wheel and facing each other are a cat and a dog, the former at the centre and the latter on the circumference. The wheel is made to rotate. Does the dog go round the cat?"

For a moment the problem seems puzzling, but, as its conditions are quite simple, we are driven to suspect an ambiguity in the terms used, and at last the question arises: What are we to understand by *going round*?

If we mean passing from the north of the cat to (say) the east, then to the south, then to the west, and then to the north of the cat again, obviously the dog *does* go round the cat, for he occupies these successive and all intermediate positions. But if, on the contrary, we mean being first in front of the cat, then on his right, then behind, then on his left, and finally in front again, it is quite as obvious that the dog does *not* go round the cat, for the cat's face is turned towards him all the time. Once this distinction is drawn, the difficulty disappears. The verbal phrase "to go round" is

[1] Cf. Miall, *B. A. Report, Section L*, 1908, p. 927. The reader may with advantage read through Strong, *Lectures on the Methods of Science*; Hand, *Science in Public Affairs*; Karl Pearson, *National Life*; Carpenter, *B. A. Presidential Address*, 1872; Carpenter, *Nature and Man*, pp. 195, 217, &c.; Poincaré, *Science and Hypotheses*, Introduction; Huxley, *Science and Education*, p. 129, &c.; Whetham, *Recent Advances in Physical Science*, pp. 30-6; Duke of Argyll, *Reign of Law*, p. 89, &c.

[2] *Pragmatism*, ch. ii. Similar problems, in different dress, are to be found in many old puzzle-books.

lacking in definiteness, and therefore introduces ambiguity into any statement containing it.

Again: how can we "show whether the top part of the wheel of a carriage moves faster than the bottom part"? We first require to know the exact meaning of *moves faster than*. Is the motion to be taken as relative to the axle of the wheel, or relative to the general forward movement of the carriage?

In such cases it is quite easy to see how necessary it is to know the precise meaning of the terms employed, but we are perhaps hardly conscious of the necessity for such precision in the discourse and converse of everyday life. The quarrels of the philosophers and the politicians are largely due to the element of doubt involved in the meaning of the terms peculiar to their respective creeds. So impregnated with indefiniteness, in fact, are most of such terms that their precise definition seems to be impossible.

Who would venture to define, for instance, the term "socialist"? A score of different people might give a score of different answers. Are we supposed to think of the religious, the philosophical, the political, the juridical, the economic, or the sociological side of the term, which, to some people, seems to mean an aspiration, to others a speculation, to others a policy, to others a theory of rights, to others an economic doctrine, and to others a philanthropic movement? or are we to include all these things? or are we to eliminate the differences and base the definition on the collected residue? The term almost defies definition, yet it presents much less difficulty than such words as, say, *civilization*, *wealth*, *government*, *agnosticism*, *truth*, *cause*, and many a hundred more. The rough, working definitions of the dictionary serve to throw some light on the meaning of isolated words, but it is when the words are actually used in assertions that their inherent indefiniteness leads to doubt, and to this is due nine-tenths of the wrangling that goes on in all the Council chambers of the country.

"We cannot", says Sigwart,[1] "have certain and firm possession of an idea, or use it in thought, unless we have a word by which to denote it. When the word is wanting to an idea, we always feel it as a want, and have a difficulty in grasping the idea in its individuality, and in reproducing it." Yet we can easily lend ourselves to the delusion of thinking that the learning of names adds to our knowledge of things, though we really gain very little from being told, for instance, that this plant is called fuchsia and

[1] *Logic*, vol. I, p 41.

that clematis. To a child there is a danger that a new name may be a mere empty sound. A child is little practised in apprehension and has but a scanty store of knowledge; hence, an image which enters into memory and is afterwards reproduced with the word, falls short of being a faithful and exhaustive copy of the thing presented to the senses.[1] Unless the child is trained to observe, he may grow up and pass through life without ever seeing in a presented object more than a small fraction of what he ought to see. The traces which remain in his mind are no more than a rough and faded copy of the thing "in which, as in a hasty sketch, only the more prominent features appear".[2] A teacher can hardly expect to succeed if he fails to realize the importance of finding out, as far as it is possible to do so, what image the spoken word imprints on the child's mind. A child's progress consists, in no small measure, in his learning to apprehend more completely, in distinguishing differences more exactly, and in sorting out essentials more readily.

§ 2. Indefiniteness and Ambiguity

"We do not, as a rule, notice the indefiniteness of a word until it has caused an ambiguity. Wherever a quality is on some occasions difficult to detect, there will be a tendency on the part of the many to identify that quality with its more obvious manifestations only. To a hasty or careless or ignorant view, the good and the bad are the obviously good and the obviously bad, and, to the same sort of view, indefiniteness exists only when it has actually caused trouble. The most widely received opinion appears to be something of this kind: that indefiniteness (or ambiguity) which inheres in a word is either so slight as to be of no account, or else so great as to deserve the attention of sensible men; that theoretical Logic may, if it likes, amuse itself with the search for an accurate definition in either case, but that only where the indefiniteness is great can there be any practical value in the search."[3]

These remarks apply with much force to the Englishman who boasts that he is "practical", and is given to scoffing at what he calls "pedantry". But pedantry signifies a defective sense of relative values,—a very different thing from precision and nicety.[4]

[1] Sigwart, *Logic*, vol. i, p. 43. [2] *ib.*
[3] Sidgwick, *Use of Words in Reasoning*, § 40.
[4] Clifford Allbutt, *Notes on Scientific Papers*, p. 32.

The "practical" Englishman has been defined as one who keeps on the surface of things and never gets inside them. In his careless use of words, he does much to impoverish his mother tongue. He regards synonyms as "different words for what is 'practically' the same idea." He uses indifferently any one of such a group of words as *entreat, implore, beseech*; or *excursion, trip, tour*; or *jeer, gibe, scoff*; or *attempt, endeavour, try*; he says *mistake* when he means *fault, intention* when he means *purpose, result* when he means *consequence, principle* when he means *rule, liberty* when he means *license, nation* when he means *race*. Such instances might be multiplied to an almost unlimited extent. Even recognized masters of style are not guiltless of such offences. One such master, for instance, says,[1] "But then the disciple must be also a critic, not some *relation* or friend". Obviously, he meant *relative*. In this way is our vocabulary reduced from wealth to poverty.

The distinction between indefiniteness and ambiguity is not always so carefully observed as it should be. The distinction is important. All words are alike indefinite but are not alike ambiguous, the latter defect being due not to the words in themselves but to the occasion of their use. Indefiniteness is not itself ambiguity but is only a predisposing condition of it; and ambiguity arises only when indefiniteness is detected in a context.[2] Words with a descriptive meaning, when taken apart from the assertions in which they occur, are always indefinite, or capable of creating an ambiguity, but it is only when they have actually done so, and therefore when they are considered in reference to their context, that their indefiniteness has any effect on the meaning. And the effect it then has is absolutely destructive until the ambiguity is removed.[3]

§ 3. Translating from one Language to another

The elusive character of words is always felt when an attempt is made to translate from one language to another. Referring to the difficulty of translating Shakespeare into French, Maeterlinck says: "Translators face to face with Shakespeare are like painters seated in front of the same landscape. Each will paint a different picture;

[1] *Essays in Criticism*, vol. i, p. 2. [2] Sidgwick, *Use of Words*, p. 185.
[3] *ib.*, p. 203. The reader will find much to interest him in Max Müller's *Lectures on the Science of Language*. See e.g. vol. ii, pp. 247-61.

and for this reason, that around the literal sense of the words there floats a secret life which is all but impossible to catch, and which is, nevertheless, more important than the external life of the words and of the images." The difficulty of translating poetry is admittedly much greater than that of translating prose, for the literal sense of the words is but a small part of what they signify. Just as the strains of a cathedral chant, or the words of a great orator, produce in the mind an emotion which cannot be analysed, so it is with a poem; and the emotional power of the words produces different effects on different minds. Though the logical effect of the words of a poem translated into another language may be preserved, their emotional power may be lost altogether, and will almost certainly lose in force and delicacy.

In theory we may claim that intellectual sense and emotional sense are only different parts of the same thing; but, in practice, when they come to be expressed through the imperfect instrument of language they are often in conflict; and the poet has to decide whether he will sacrifice music to precision or precision to music. Like the artist the poet is incessantly struggling with his material. His finished poem is a mosaic, put together after an infinite number of experiments and rejections.

The translation of prose is, of course, a different matter, but, here again, to catch and reproduce exactly the intended meaning of the author is often sufficient to tax the resources of the ripest scholars. Consider, for instance, the following passages, chosen at random, from Aristotle's *Rhetoric*. The first is a translation by the late Sir R. Jebb, and the second by Bishop Welldon, both classical scholars of distinction.

JEBB

Let happiness, then, be prosperity combined with virtue or independence of life; or that existence which, being safe, is pleasantest; or *a flourishing state of property and of body, with the faculty of guarding and reproducing this*; for it may be said that all men allow Happiness to be one or more of these things.[1]

WELLDON

Happiness, then, may be defined as prosperity conjoined with virtue, or as an independent state of existence, or as the pleasantest life conjoined with safety, or *as an abundance of goods and slaves with the ability to preserve them and make a practical use of them*; for it would be pretty generally admitted that happiness is one or more of these things.

[1] *Rhetoric*, Book I, ch. v.

Or, again:—

The apparent character of the speaker tells more in debate, the mood of the hearer in law-suits. Men have not the same views *when they are friendly and when they hate*, when they are angry or placid, but views either wholly different or different in a large measure. The friendly man regards the object of his judgment as either no wrongdoer, or a doer of small wrong; the hater takes the opposite view. The man who desires and is hopeful (supposing the thing in prospect to be pleasant) thinks that it will be, and that it will be good: *the man who is indifferent, or who feels a difficulty*, thinks the opposite.[1]

The impression of the speaker's character is especially serviceable in deliberation, and the disposition of the audience in forensic matters; for our estimate of a speech is not the same, but either wholly different or different in degree, *according as we regard a person with feelings of affection or dislike*, and are angrily or charitably disposed towards him. If we are friendly to the person upon whom we have to form a judgment, we regard him as either innocent or guilty of a very slight offence; if we are inimical to him, the contrary is the case. Similarly when we are in an eager and sanguine mood, the result which is promised us is probable and advantageous in our eyes; *when we are dispirited and out of humour*, it is the reverse.

Note in particular the striking differences of meaning in the parallel sentences in italics.[2] It matters not that the general tenor of the two renderings is roughly the same. Careful comparison shows marked differences, and different impressions must be left upon a reader's mind.

It may be urged that part of the translator's difficulty in dealing with passages like the above is that the language translated is a dead language; that the evidence as to the meaning of any particular word is therefore largely inferential and does not admit of the same kind of verification as in the case of a living language like French or German. This no doubt is true, and it is interesting to touch upon the kind of evidence actually available. Let us take, for example, any one of the thirty *Characters of Theophrastus*, say the thirteenth. Here is depicted that type of man "who will rise and promise things beyond his power, and who when an arrangement is admitted to be just will oppose it and be refuted. He will insist, too, on the slave mixing more wine than the company can finish; he will separate combatants, even those whom he does not know; he will undertake to show the path, and after will be unable to find his way. Also he will go up to his commanding officer and ask when he means to give battle. When the doctor forbids him to

[1] *Rhetoric*, Book II, ch. i.

[2] The italics do not, of course, appear in the originals.

give wine to the invalid, he will say that he wishes to try an experiment, and will drench the sick man. Also he will inscribe upon a deceased woman's tombstone the name of her husband, of her father, and of her mother, as well as her own, with the place of her birth; recording further that, 'All these were Estimable Persons'. And when he is about to take an oath, he will say to the bystanders, 'This is by no means the first I have taken'."[1]—The desire to please, either by rendering an extraordinary service, or by performing an ordinary one unusually well, is, as Jebb says, present in every act described. The problem is, then, to find an English equivalent for the Greek term (*περιεργία*) used to describe such a man. The nearest equivalent seems to be "officious", the term generally adopted. But, obviously, it is not quite the word we want, and we have to be satisfied with an approximation.[2] But if this be so in the case of a word the exact meaning of which, in the original, is known, how much more difficult it must be to find the precise English equivalent in a case where the evidence of the meaning is mainly analogical, and probably scanty at that.

§ 4. Classes and General Terms

If we ask the pupils of an ordinary Third or Fourth Form, to define the name of some common object, say a *box* or a *ball*, we shall probably feel little surprise at many of the wild answers given. Unless a child has been taught *how* to define, it is absurd to expect a definition with any approach to accuracy. Let us consider for a moment what may at least appear to be a much easier term. What is an *animal*?

If I think of a particular animal, say my fox-terrier Pompey, I have little difficulty in giving an exact description of him; but if I think of a fox-terrier generally, and not of a particular fox-terrier, I have a greater difficulty in finding a description at once clear and distinct. Do I, in these circumstances, describe, consciously or unconsciously, a particular fox-terrier, or do I attempt to give a general description of the many fox-terriers I have seen? Does an ordinary book-illustration of a fox-terrier represent some particular animal, or is it a sort of composite photograph of many fox-terriers? In the latter case, can it be said really to represent a fox-terrier at all?

[1] Jebb's translation.

[2] Healey (1616) translates by "impertinent diligence"; La Bruyère (1687) by "l'air empressé"; Howell (1824) by "Busybody".

Let us now advance a step further, and consider the more general term *dog*. We may be able to describe a dog in tolerably accurate language, but do the words we use convey an accurate representation of the image we are supposed to have formed in our minds? Is our description anything more than mere words, for can we conceive a kind of composite photograph of a "dog"? A book-illustration of a "dog" would certainly be that of a particular breed, for no painter would attempt to produce anything of the nature of a "generalized" picture. A composite picture of a pomeranian, a poodle, a dachshund, and a deerhound would be an absurdity. If, then, it is impossible to obtain a picture of a "dog" on canvas, is a mind-picture of a "dog" possible?

We may regard the fox-terrier as a "variety" of the "species" common dog (*canis familiaris*), this species forming one member of the "genus" *canis* which includes other related species; for instance, the wolves and the jackals. Obviously a mental picture of a *canis* —a composite photograph of, for instance, some common dog, a wolf, and a jackal—is much more difficult to form than one of a "dog" alone. And as we work our way upwards through the "order" *carnivora*, the "class" *mammalia*, the "sub-kingdom" *vertebrata*, to the "kingdom" *animal*, the difficulty becomes greater and greater. When, then, we think of an "animal", we seem to think of a particular animal, or perhaps a succession of particular animals. We may think of a camel, a canary, a turtle, and a grasshopper, but we certainly do not form a composite mental picture of all four.

It would thus seem that if we define an animal in the usual way, namely, "a living being possessing (1) locomotive power, which can be voluntarily exercised, (2) an internal cavity for the reception and digestion of solid food, and (3) a distinct nervous system",[1] we are using a form of words which, though approximately accurate as a description, corresponds to no distinct mind-picture. Apparently, then, a "general term" is little more than a convenient label for tying on to a group of similar things. Now, in a group of things which, because they are "similar", are brought together into a class, there will almost inevitably be "differences" of some kind; and these differences, whether undiscovered, overlooked, or ignored, are nearly certain to lead to ambiguity in a greater or less degree.

Sidgwick defines a class-name as any imagined group of individual cases, whether material things or immaterial, whether real or

[1] This is a sufficiently accurate definition at this stage.

unreal,—a group in which every individual is supposed to resemble all the others in some respects though differing in other respects; in fact any name which is used so as to admit of a plural.[1] And he points out that the defect of the popular view about classes is that it conceives them too rigidly, recognizes too little that the grouping is imaginary, changing with the changing purposes for which it is wanted. "There is a disinclination either to recognize any artificial element in 'natural' classes, or to admit the continuity of nature, or to admit that all classes are indefinite."[2]

The Scholastic logicians knew so little of the laws of nature that they were unconscious of the real difficulties underlying definition. They professed to be able to distinguish between those qualities which belonged to the "essence" of a class, and those which were only "properties" or "accidents". "Natural classes, it was then habitually supposed, had no dependence upon man's way of regarding facts. The received idea was that natural classes were simply made *for* man, and made with clear-cut edges; and that man had nothing to do but to accept them and learn their names and their definitions. Doubts as to the application of a class-name were supposed to be the fault or misfortune of the doubter, not of the name."[3] It was forgotten that the invention of names was of human origin.

Some classes, of course, are artificial, for instance, telescopes, teaspoons, and Territorials; for we know all about the process involved in their production. But many people are still unwilling to admit that there is anything artificial about the so-called natural classes. Now natural classes as opposed to artificial ones can only mean those the precise limits of which are so clearly marked out that it is not optional but imperative to recognize them,[4] but when we ask what these classes are, our answer must largely depend on our adopted standard of clearness of division. Very few names are really safe against future changes of definition, for such changes almost always become necessary with increased knowledge. Usually, our knowledge is so imperfect that it is folly to attempt to draw between classes anything like a rigid line of demarcation.

Since, as Sidgwick says, the world is, on the whole, wiser than the individual, the accepted sense of a word carries a presumption in its favour. There is thus something in the contention that class-names have a "correct" meaning. Yet, however widely the mean-

[1] See *Use of Words*, pp. 149-51. [2] *ib.*, pp. 151-3. [3] *ib.*
[4] *ib.*, pp. 153-9.

ing of a name may be accepted by the best authorities, it by no means follows that this accepted meaning is so far satisfactory that we can with certainty learn the facts about any particular case where the name is applied. It may be a help in cases where precision is not of much consequence, but it is certainly likely to be often a hindrance to the progress of real knowledge.[1]

§ 5. Connotation, Generalization, and Specialization

Some further light may perhaps be thrown on the inner significance of general names by considering the old logical term "connotation".

"A *connotative* term is one which denotes a subject and implies an attribute. A *non-connotative* term is one which signifies a subject only, or an attribute only." 'London', or 'England', or 'John', are names which signify a subject only. 'Whiteness', 'length', 'virtue', signify an attribute only. None of these names are therefore connotative. But the word 'man' denotes Smith, Brown, Jones, and an indefinite number of other individuals, of whom, taken as a class, it is the name. But it is applied to them because they possess, or to signify that they possess, certain attributes; for instance, corporeity, animal life, rationality, and a certain external form. The word 'man' is therefore connotative; it signifies the subject *directly*, the attributes *indirectly*; it *denotes* the subject, and *implies* or *connotes* the attributes.[2]

Obviously, all concrete general names are connotative. But proper names are not connotative; they merely denote the individuals that are called by them. Professor Welton makes this point clear: "When any general name is restricted in its application by some limiting word or phrase, of course its implication is not lost. Indeed, that implication is increased, and thus we have the class of significant individual names, which, though they denote only one object, yet imply the possession of many attributes by that one object. Thus, if we speak of a mountain, we imply the attributes 'height', and 'composition of rock'; if we add 'in Asia', we increase the number of characteristics, though we limit the number of things to which the name applies; by adding 'high', we carry both these processes a stage further; and if, finally, we make the

[1] *Use of Words*, p. 159. The reader may usefully compare the views of Mill, Lotze, Carveth Read, and Bain, on general and abstract terms. See the respective treatises on Logic. Cf. also Locke, vol. i, p. 275, &c.

[2] Mill, *Logic*, Book I, ch. ii, § 5.

term singular and speak of 'the highest mountain in Asia', we manifestly retain all the attributes previously implied, and add to them uniqueness. All these attributes are *implied* by the name, and anybody using the name must be supposed to intend to convey them to his hearers. But were we to use, instead of the significant name, the proper name 'Everest', which we believe to be the name of the same object, no such information would be given. To anybody who knew the geographical fact that Everest is the highest mountain in Asia, the name 'Everest' would doubtless *suggest* all that the words 'the highest mountain in Asia' *imply*. But a word is not connotative because it may suggest facts or attributes otherwise known, but because it implies them."[1]

One of the chief sources of lax habits of thought is the custom of using connotative terms without a distinctly ascertained connotation, and with no more precise notion of their meaning than can be loosely collected from observing what objects they are used to denote. It constantly happens that some general resemblance which a number of objects bear to one another leads to their being familiarly classed under a common name, although it is by no means immediately apparent what are the particular attributes upon the common possession of which their general resemblance depends. When this is the case, people use the name without any recognized connotation, i.e. without any precise meaning; they talk, and consequently think, vaguely. New objects are continually presenting themselves to people who class them on no other principle than that of superficial similarity.[2]

Most writers on Logic emphasize the fact that all cases of ambiguity of language are really instances of indeterminate connotation. "If", says Lotze, "we pass our mental world in review, we shall be surprised to find that words of great significance betray an imperfect apprehension of their objects; for the more complex and important any matter is, the more easily will persuasive impressions derived from repeated observations awaken the feeling of its individuality, completeness, and self-inclusiveness, without

[1] *Logic*, vol. i, p. 53.

[2] Mill, *Logic*, Book I, ch. ii, § 5. Bain mentions many interesting examples, amongst others the word *stone*. "It is applied to mineral and rocky materials, to the kernels of fruit, to the accumulations in the gall-bladder and in the kidneys; while it is refused to rocks that have the cleavage suitable for roofing (slates), and to baked clay (bricks). It occurs in the designation of the magnetic oxide of iron (lodestone), and not in speaking of other metallic ores. Such a term is wholly unfit for accurate reasoning, unless hedged round on every occasion by other phrases, as building-stone, fruit-stone, gall-stone, &c." (*Logic*, Induction, p. 72.)

necessarily giving any real insight into its structure."[1] It is the unconsciousness of our ignorance of the terms in most frequent use that is the danger.

Two counter movements are always taking place in language; one of *generalization*, by which words are perpetually losing portions of their connotation and becoming of less meaning and more general acceptation; the other of *specialization*, by which other, or even these same, words are continually taking on new connotation, acquiring additional meaning by being restricted to a part only of the occasions on which they might properly be used before.[2] If, then, we define "Denotation" as "the number of individual things to which the term is applicable in the same sense", it will be clear that the denotation of a term is logically fixed by the connotation. Practically, connotation and denotation help to determine each other, and a modification of the one usually leads to a modification of the other. As we augment the number of attributes implied by a name we diminish the number of things to which that name is applicable; there are, for instance, fewer trained teachers than teachers. Conversely, if we wish to include under a name a group of things not before included under it, and so to enlarge the borders of the class which the term denotes, we can, usually, only do so, by removing from the connotation of the name those attributes which before marked the difference between the two classes; for instance, if we wish to include both Elementary School teachers and Secondary School teachers under one common name, we must omit the points of difference, "Elementary School" and "Secondary School", and retain only the term "teacher", which will be applicable to all members of both classes, but which implies or connotes less than the separate name of either. In short, generally speaking, the less a name connotes the more groups of things it is applicable to; and the more it connotes, the narrower is its range of application.[3]

It will be seen, then, (1) that when we add an attribute not common to the whole class, we exclude some members of the class from participation in the class-name, and so decrease its denotation; and (2) that when we introduce into a class some things not possessing all the attributes connoted by the class-name, we have to omit

[1] Lotze, *Logic*, p. 29. [2] Mill, *Logic*, Book IV, ch. v, § 2.

[3] Cf. Welton, p. 64. The demand for a formal rule of interdependence between connotation and denotation was satisfied in traditional logic by the doctrine that one varies as the other. But the term "varies as" suggests a mathematical relation, and such a relation is, of course, impossible. See Bosanquet, *Logic*, vol. I, pp. 58, 60, 67.

part of its meaning, that it may cover the whole of the more extended class; and thus we decrease the connotation.[1]

Thus *specialization* is increasing the connotation of a term and thereby limiting its application; while *generalization* is the opposite process of decreasing the connotation so as to embrace a larger number of objects.

Enough has been said to show how variable is the precise connotation of even the most ordinary terms. It remains to consider briefly Definition and its functions.

§ 6. Definition

Although the whole object of any class-name is to group together similar individuals, yet a class may always be regarded as a *sect*,—a portion cut out of a larger whole and placed in imaginary isolation.[2] In order to describe or to define a thing, we must first recognize clearly the class to which it belongs. As a rule, however, complete description or perfect definition is impossible. There comes a point at which even the most inquiring mind is willing to cease, for the time, from collecting further detail.[3]

Sidgwick draws a happy distinction between what he calls a "translation" and a definition proper. By a translation he means the general account of the meaning of a word, such as we find in a dictionary. Such translations are usually vague, as, naturally, no account of the meaning of the term can be given with any reference to the particular statement in which the term occurs. A general description or definition of this kind must inevitably leave some particular difficulties untouched; like all other general statements it suffers from the defect of generality or "abstractness".[4]

If we consider the various ways in which the meaning of any word can be verbally explained, the "translation" takes its place at the lower end of the scale. At the other end of the scale, where explanation is most precise and definite, there is the process of discussion by which a special difficulty of application (an "ambiguity") is dealt with after it has arisen. Between these two extreme types come the better kind of generally-useful definitions, which have evidently something in common with both ends of the scale. In face of a particular ambiguity which they foresee and remov make good their claim to be called definitions; in fa

[1] Sidgwick, *Use of Words*, p. 166. [2] *ib.*, pp. 152–7.
[3] *ib.*, p. 175. [4] *ib.*, p. 148.

particular ambiguity which they do not foresee and remove, they become, for the time, analogous to "translations". If we want the word "luxury" defined in order to apply in some doubtful case the rule that luxury is to be avoided, it is no use to be told that luxuries are the opposite of necessaries. That answer leaves our difficulty exactly where it was. It gives us merely a verbal expression for an equivalent to "luxury"; and perhaps we knew this before.[1]

The point of the distinction intended is that "a translation is not wanted except when a word to be translated is unfamiliar, while a definition is not wanted except when the rough meaning of a word is already known,—and only then if an actual difficulty is felt in applying a word correctly in a given case or cases".[2]

It follows, therefore, that if words are taken out of their context, the "definitions" of such words can, as a rule, only be of a rough-and-ready character.

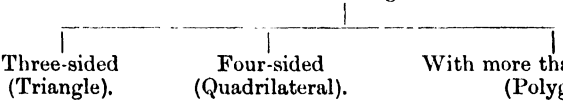
To define a name, then, is, manifestly, to fix its connotation. This, of course, supposes a comparison of things, feature by feature, and property by property, in order to ascertain in what attributes they agree. Having discovered in what they agree, we have to determine which of these common attributes shall be selected to be associated with the name. The framing of a good definition is thus a matter of discussion, and the definition is always liable to improvement as our knowledge increases.[3]

Perhaps the best means of obtaining reasonably good definitions in the classroom is to fall back upon the method of Formal Logic, —*per genus et differentiam.*[4]

In Logic, any class of things may be called a *genus* if it be regarded as made up of two or more *species.* "Line", for instance, is a genus as regards the species "straight" and "curved". On the other hand, species is any class which is regarded as forming part of the next larger class. Thus the terms genus and species are correlative, the genus being the larger class which is divided, and the species the two or more smaller classes comprising the divisions of the genus. It is evident that the connotation of the species contains more qualities than that of the genus, for the species must contain all the qualities of the genus, as well as a certain additional quality or qualities by which the several species are distinguished from each

[1] *Use of Words*, pp 48–9. [2] *ib.*, p. 49. [3] Mill, *Logic*, Book IV, ch. iv, § 3.
[4] From what has been said, the somewhat artificial nature of this method will be readily seen.

other. These additional qualities form the *difference*, which may be defined as the quality or sum of qualities which mark out one part of the genus from the other part or parts.[1] Consider, for instance, the genus rectilineal figure:—



Obviously, triangles have all the properties of rectilineal figures, and "three-sided" as well. Quadrilaterals have all the properties of rectilineal figures, and *four*-sided as well.[2] Thus triangles and quadrilaterals are *alike* in being rectilineal figures, but they differ in the fact that the former are *three*-sided and the latter *four*-sided. We are thus able to define the "species" *triangle* by "adding" the "difference" *three-sided* to the "genus" *rectilineal figure*,—"a triangle is a rectilineal figure with three sides". We now see the meaning of the old rule for framing a definition: Consider the thing the name of which is to be defined as a *species*; place this *species* under its *genus* and determine the *difference*; the genus added to the difference will give the definition.[3]

Children soon learn to use this rule with reasonably good results. Here are one or two actual examples given by girls in a Form II (average age, eleven years). They were asked to define the name "hat"; a few questions from the teacher elicited the *genus* "articles of dress", and the *difference* "for wearing upon the head". Thus the definition given was, "a hat is an article of dress for wearing upon the head". A little girl of nine in the same class defined a "chair" as "an article of furniture used for sitting on". For such a young child the definition was good, but the difference "for sitting on" is obviously too vague, for the same definition would apply to other kinds of seats. The definition of "book" proved more difficult, and the teacher was a considerable time before she could obtain a suitable genus. When at last the genus "a number of printed sheets of paper of the same size" was suggested, the

[1] See Jevons, *Logic*, pp. 98-100.

[2] Attributes of things are very often found in groups, so that where one is found, others are found too. Thus when a triangle is equilateral it is also equiangular, and to speak of an equiangular equilateral triangle does not therefore limit the denotation given by equilateral. (Welton, *Logic*, vol i, p. 63)

[3] Imperfect and rather misleading as this statement is, it is in common use. A warning has already been given about the use of mathematical terms in connection with connotation. For Mill's definition of "Difference", see *Logic*, Book I, ch. vii, § 5.

teacher wisely refrained from quibbling over the suitability of the term "sheet" or of the words "the same size", and accepted the suggestion except as regards the word "printed". The *difference* finally decided upon was "bound together", though one child objected to this on the ground that bound together might mean bound at all four edges and thus signify a "drawing-block". The whole discussion was interesting, and showed how keenly even young children can be induced to try to enter into the real significance of words.

Of course the definitions of the names of common objects like *chair* and *book* present more difficulty than the definitions of such terms as *triangle* or *electrophorus*. In the former the denotative element predominates, and we are in the habit of using the names without any distinct idea of their connotation; in the latter, the connotation is the more important element, for the sole value of technical terms lies in a knowledge of their exact meaning.[1]

It should not be forgotten that the same class of things may be both a genus and a species at the same time. If, for example, we had to define "isosceles triangle" we should regard it as a species of the genus "triangle"; but if we had to define "triangle" we should regard it as a species of the genus rectilineal figure. It must also be borne in mind that, in logical division, we must never proceed from a high or wide genus at once to a low or narrow species.[2] The species should always be those of the *proximate* or next higher genus.[3] A child might be tempted to define the species *chair* by falling back upon the genus *wooden thing*, or even *thing*, in which case the "difference" is bound to be imperfect since many necessary attributes would be omitted. As Professor Welton puts it: "If we define a class-term whose connotation is *ABCD* by referring it to the genus *A* (instead of to the proximate genus *ABC*) and adding the difference *D*, we plainly omit the attributes *BC* from our definition".[4] A definition should always state the essential attributes of the species defined.[5]

We may regard the definition of a connotative name as the proposition which declares its connotation,[6] and this is the real advantage of a definition. The framing of a definition compels us to examine the precise connotation of a word,—to get inside it, so to speak, and know it thoroughly.

[1] Cf. Welton, *Logic*, vol. i, p. 111.
[2] "*Divisio non faciat saltum.*"
[3] Jevons, *Logic*, pp. 100–1.
[4] Welton, *Logic*, vol. i, p. 109.
[5] For an account of the "Predicables", including "Property" and "Accident", see Mill, *Logic*, i-vii, or any other standard work on Logic.
[6] See Welton, *Logic*, vol. i, p. 109 *et seq.*

The practical impossibility of obtaining perfect definitions is, ultimately, due to the necessary indefiniteness of all general names. "Since the definition of any name P is an attempt to describe the difference between those things which are P and those which are not P, and since that difference, like everything else, can only be described in general terms and therefore incompletely, no definition can *perfectly* define. To put it another way, definition is an attempt to decide what amount and kind of difference is allowable between members of the class P, or what departure from the normal type is required to destroy a doubtful member's right to the name. There is no end to the inquiry except where we choose to make one."[1]

The definite connotation of most scientific terms gives a considerable advantage to men of science, whose statements, however much lacking in literary form, are at least clear-cut, precise, and definite. This is less frequently the case in other departments of knowledge.[2] The lesson for the teacher is that he should always use words the exact significance of which he is certain his pupils fully apprehend. How often one hears in the classroom such terms as the "constitutions" of Clarendon, the "causes" of the Reformation, "gerundial" infinitive, "varies as" the pressure, chemical "affinity", and scores of others, which have but the slightest significance to the children.

Few people are the masters of the words they use; most people are their slaves.[3]

[1] Sidgwick, *Use of Words*, p 178

[2] But men of science occasionally accept terms, the exact meaning of which is open to considerable doubt. The writer recently asked half a dozen chemists to define the term "ionization"; all six definitions differed in a most remarkable way. Then, again, the term "anticyclone" (invented by Sir Francis Galton in 1862; see his *Memories of my Life*) suggests, to the popular mind, some sort of opposition, the prefix being considered to signify the same meaning as in "antipodes" or "anti-vivisection", whereas the actual phenomenon referred to is of the most benign character. The prefix imports, not opposition, but alternation, as in "antiphon" and "antistrophe".

[3] The reader may spend a useful half-hour in referring to pp 321-35 of Part II of Herbert Spencer's *Principles of Psychology* Spencer takes from Berkeley's *Principles of Human Knowledge* the sentence "By sight I have the ideas of light and colours". The various words of this sentence are subjected to minute analysis, and the exact significance of the assertion is thus microscopically examined.

CHAPTER III

Philosophers and some of their Problems

§ 1. Philosophy and its Sub-divisions

What is a Philosopher? In the *Republic*, Plato defines him as one who gets inside things and discovers the nature of their reality, and contrasts him with those who are content with mere appearances and with ready-made opinion. The philosophers, he says, are those who are able to grasp the permanent and immutable.

But Plato makes no clear differentiation of the subsidiary inquiries by which the question of the ultimate constitution of things may be approached. He fuses together in a semi-religious, semi-ethical fashion, Logic, Physics, Psychology, Metaphysics, and the Theory of Knowledge; and it was left to Aristotle to draw the necessary lines of separation. Aristotle was by nature much more methodical than Plato, and he may be regarded as the founder of Logic, Psychology, Ethics, and Æsthetics, as separate philosophic disciplines. Those first principles which are common to, and presupposed in, those narrower fields of knowledge, he usually called "first philosophy", but this term has since given way to the term *Metaphysics*.[1]

Aristotle does not restrict Philosophy as a term of general application to the subjects just mentioned; under the title he includes Mathematics and Physics. But, as the mass of knowledge accumulated, it gradually came about that the name Philosophy ceased to be applied to inquiries concerned with the particulars as such. The details of Physics, for instance, were abandoned to the Science specialist.

In its modern sense, Philosophy is a term with a very variable connotation, but Mr. Balfour's definition is perhaps as acceptable as any. "Multitudes of propositions, all professing to embody knowledge belonging to Science, Metaphysics, and Ethics, are being continually put forward for our acceptance. And as no one believes all of them, so those who profess to act rationally must hold that there are grounds for rejecting the propositions they disbelieve and for

[1] τὰ μετὰ τὰ φυσικά. The term means, merely, "the writings that came after the physics".

accepting those they believe. The systematic account of these grounds of belief and disbelief makes up what is here called Philosophy."[1] Sidgwick points out that Philosophy thus understood considers the fundamental principles of all departments of systematic thought, but considers them with the special object of examining their validity and evidence. He does not much care for Mr. Balfour's grouping, but admits how wide is the variation, how vague the connotation, of nearly all philosophic terminology. Philosophy, as defined by Mr. Balfour, would seem to correspond to what is called Epistemology, or Theory of Knowledge, and is perhaps a rough equivalent to the Logic of Mr. Bradley and Mr. Bosanquet.[2]

§2. The Borderland between Philosophy and Science

It is a natural thing for the instructed plain man to place implicit reliance on the evidence of his senses. He sees the sun in its daily journey from east to west across the sky, and, like the ancient astronomers, he makes the assumption that the sun goes round the earth. To him the assumption involves no element of doubt; to him it is not an hypothesis—it is a fact. When it is pointed out to him that the heliocentric hypothesis provides a simpler explanation of the celestial motions and is more consistent with ascertained facts, he is puzzled, and his respect for authority may make him feel that his senses, at all events his sense of sight, may sometimes deceive him. If he becomes a student of Science he finds that many of his established notions are hopelessly wrong. In thinking about ordinary material things, for instance, he had always thought that they were coloured and resonant, quite independently of their relation to himself. The evidence of his senses he soon learns to accept with greater caution, and he comes to understand that, so far as Physics distinguishes reality and appearance, its criterion is not sense-perception alone, but consistency with an elaborate and complex system of more or less definitely established facts which embody the combined results of many perceptions and inferences. Science has continually to explain to uninstructed common sense that what really happens is often something quite different from what appears to happen.

The chemist performs a number of quantitative experiments,

[1] A. Balfour, *Defence of Philosophic Doubt*, pp. 1, 2.

[2] H. Sidgwick, *Philosophy, its Scope and Relations*, p. 112.

examines his results, and detects amongst them certain common quantitative relations, sums up these constant relations as "generalizations", and so establishes the "laws" of constant, multiple, and reciprocal proportion, and the "law" of Gay Lussac. These laws constitute important principles of chemistry, and form the basis of the theory of the subject. Their justification is a great number of definitely established facts. They involve no assumption, no hypothesis, save that of the great induction of the uniformity of nature.

But the chemist now seeks for an "explanation" of these four laws. The atomic hypothesis covers and explains all the facts of the first three, and Avogadro's hypothesis covers and explains all the facts of the law of Gay Lussac. But these hypotheses are *assumptions*; they are constructions of the chemist's mind; they may or may not correspond to objective fact. In making these assumptions, the chemist is trying to get behind his observed facts, behind his phenomena, in order to discover the hidden secrets there. In doing this he is passing over the border-line between the domain of Science and the domain of Philosophy.

Such assumptions often prove to be wrong. Again and again in the history of Science, one hypothesis has been discarded in favour of another. But each hypothesis served at the time to cover all the facts then known and to link them up. Sometimes a new hypothesis has superseded an old one because the latter did not cover new facts, and was therefore obviously wrong; sometimes an old hypothesis has been discarded because seen to be held on insufficient grounds; sometimes an old hypothesis has been reduced to a simpler form: the mind always prefers a simple explanation to an elaborate one. Around and beneath the more settled portions of Physical Science, in the region where knowledge is growing in range and depth, there is constant conflict and controversy as to the truth of new conclusions, for the controversy centres round assumptions which are unproved, and often seem unprovable. Natural Science, so recent a growth, is necessarily infected with error.

It has been said that the truths of Philosophy bear the same relation to the highest truths of Natural Science as each of these bears to the lower truths of Natural Science. But the term truth is hard to define, and it would be safer to say that, as each widest generalization of Science embraces and consolidates the narrower generalizations of its own division, so the generalizations of Philosophy embrace and consolidate the widest generalizations of Science It has, however, to be borne in mind that the main concern of Science

is with phenomena, for the investigations of Science yield mainly phenomenal knowledge. Philosophy aims at a knowledge of the concealed realities behind phenomena. There is, however, a great deal of common ground between Science and Philosophy, and the purely speculative side of Science properly belongs to Philosophy. A philosopher unversed in Science is like a man of Science unversed in Philosophy: neither can claim to be an authority in his own subject.

§3. Psychology, Metaphysics, and Logic

Nominally, Psychology differs from Physical Science only in the nature of its subject-matter, and not in its method of investigation. Regarded as an empirical study of the mind, it proceeds by methods of observation, experiment, and induction analogous to those used in Natural Science. But the phenomena of the mind—thoughts, cognitions, judgments, beliefs, the facts with which Psychology deals—are obtained by introspection, not, as in the case of the phenomena of Natural Science, through the senses. The difficulties of ascertaining the facts are therefore greater, and psychological interpretation is not always easily distinguished from metaphysical reflection.

The psychologist cannot begin at birth to register the history of his mental operations; he has to begin when a grown man, and the more cultivated his mind the farther away he is likely to be from the primitive mental operations of his early infancy. The system of knowledge which he attempts to formulate is thus of a highly problematical character, for about the beginnings of knowledge there can be no certainty.

Textbooks on Psychology usually encroach on Metaphysics. For instance, they sometimes attempt to investigate valid beliefs as conceived to exist for an ideal mind independent of the peculiarities of development of particular minds. There is, in fact, often such an admixture of metaphysical speculation with the empirical facts of Psychology, that the intelligent reader is apt to attach a very sceptical value to the whole subject. Many of the ultimate problems of Psychology are, however, necessarily metaphysical, and are never likely to be brought within the range of experimental investigation and solved by the methods of Science. The newer experimental Psychology is laboriously accumulating valuable facts, but many competent authorities are of opinion that it is attacking an insoluble

problem. At bottom, it is based on the fundamental hypothesis that every phase of consciousness has its counterpart in nerve changes. That our conscious life is inseparably associated with the changes that go on in the grey matter of the brain there is now hardly any room for doubt, but *how* the two are connected is unknown, and all explanations are conjectural. That our thoughts, cognitions, judgments, and beliefs are *nothing more* than mere molecular changes in the grey matter of the brain is an hypothesis unsupported by any acceptable evidence.

Unlike positive Philosophy, which contemplates the world as a whole from the point of view of Natural Science, and is satisfied with empirical evidence and with such inferences as can be drawn therefrom, Metaphysics aims at ascertaining facts concerning matter and mind, and their relations *beyond* such knowledge as is based upon or is verifiable by particular empirical cognitions. The method of Metaphysics is a distinctive dialectical method; it begins by making *a priori* pronouncements, and, by applying to these the rules of formal Logic, arrives at final conclusions which do not admit of any form of methodical proof or any sort of appeal to experience. Such conclusions, based as they are ultimately upon hypothesis which cannot be verified, are necessarily always uncertain.

In mediæval times, the most implicit trust was placed in formal Logic, and a correct chain of deductive reasoning from some original hypothesis dogmatically asserted was quite sufficient to stifle any doubts about strange conclusions; and gradually the opinion became almost universal that the most important truths concerning reality could, by mere thinking, be established with a certainty that no subsequent observation and experiment could shake. And even at the present day there are philosophers who claim that *a priori* reasoning can reveal otherwise undiscoverable secrets about the universe, and that, therefore, reality can be proved to be quite different from what by direct observation it appears to be.

In the light of modern science, great numbers of old *a priori* errors have been refuted, and it is now natural to expect a fallacy in any deduction of which the conclusion appears to contradict patent facts. The fallacy is not usually in the actual chain of reasoning: philosophers do not often make elementary blunders of that kind. It is traceable rather to an untenable major premiss, adopted, perhaps, because of the royal confidence felt in some unexamined intuition, or because of some unsuspected prejudice, political, social, or theological. This major premiss, the original hypo-

thesis adopted, may look plausible enough, but if the consequences logically traceable from it violate the first principles of common sense, the hypothesis must, without hesitation, be rejected. A conclusion is by no means necessarily correct because the rules of formal Logic have been exactly observed. The unacceptable conclusions of educated men are far more frequently traceable to false premisses than to false reasoning.

The formal Logic of tradition is merely a logic of consistency. As a well-known writer[1] on modern Logic says: "The trivial nonsense embodied in this tradition is still set in examinations, and is defended as a propædeutic, that is, a training in those habits of solemn humbug which are so great a help in later life". Modern Logic is something very different. Its chief business is to examine the validity of premisses, and it deserves the closest attention.

In ancient Philosophy the fundamental contrast was between things as they appear and things as they are supposed to be in themselves; between appearance and reality. In modern Philosophy the fundamental contrast is between mind and matter, between man who knows and the things known to him.

§4. The Contrast of Subject and Object

All consciousness must, in the first instance, present itself as a relation between the distinguishable parts of a duality, the person who is conscious and the thing he is conscious of. In order to be conscious at all, a person must be conscious of something. This contrast has been indicated, directly or indirectly, by various names: mind and matter; person and thing; subject and object; self and not-self; the ego and non-ego. Mind, the ego, as knowing subject, may therefore be at once connected and contrasted with its known objects. That an external material world exists independently of our knowing it, and that its existence is not affected by our knowledge of it, is a belief that seems at once instinctive, inevitable, and necessary.

Introspectively, at any moment I am aware that I exist and continue to exist through changing states of consciousness. I know that I exist, but what I am, how my ego is constituted apart from my material organism I do not know. I am not justified in assuming, from the evidence of introspection alone, that my ego is, for

[1] Bertrand Russell.

instance, a self-existent entity indestructible by the forces that ultimately destroy my material organism, or that my consciousness is to be attributed to anything of the nature of a phantom-like double of the body. All that I can with certainty say is that, when I concentrate my attention on the simplest act of perception, I have the irresistible conviction that I exist and that something else exists, and that I am conscious of both existences at the same moment.

We may therefore lay it down as a necessary conviction that consciousness gives us as an ultimate fact, a knowledge of both the ego and the non-ego in relation to and in contrast with each other; and it gives these elements in equal independence. In other words, mind and matter present themselves in absolute co-equality. This fact, however, is by no means universally accepted, and even when it is accepted it is accepted with such qualifications as it suits a particular philosopher to devise. In short, there are almost as many philosophic systems originating in this fact as it admits of various possible modifications. As might be expected, therefore, no consistently logical classification of the different schools of Philosophy is possible. We may, however, give some indication of the broad distinctions amongst them.

§5. The Different Schools of Philosophy

The first distinction may be drawn between those who accept, wholly and without reserve, the fundamental fact that mind and matter are separately distinct to consciousness, and those who do not. Thus we have—

A. *Natural Dualists*, who regard mind and matter as real entities distinct and separate from each other.

B. Those who do not so accept the fact.

Now it is undoubtedly true that the only *positive* knowledge we have of mind and matter is a knowledge of *phenomena*, and we may therefore suppose and consequently assert that all our knowledge of mind and matter is only a consciousness of various groups of mere appearances. On the other hand, we may assert that the known phenomena of mind and matter must necessarily be referred to underlying substances or substrata of some kind, though actually unknown. Thus our class B may be subdivided into—

I. *Nihilists*, who deny that the testimony of consciousness can

guarantee a substratum or substance underlying the phenomena of either the ego or the non-ego, and who assert that perceptions and ideas are the only realities.

II. *Realists*, who affirm that the testimony of consciousness can guarantee the existence of a reality, a substance or substratum, underlying the phenomena of the ego and also of the non-ego.

Realists are of many kinds, but they may be grouped into two main classes:—

1. *Hypothetical Dualists*, who accept the testimony of consciousness as to the ultimate duality of the ego and non-ego, but maintain that our consciousness gives us no *direct* knowledge of anything beyond phenomena; that we therefore have no immediate knowledge of the existence of matter or of mind, though we are compelled to assume both the existence of a substance or substratum in which the qualities of matter inhere, and also of an entity—mind, subject, or spirit—which perceives the facts of consciousness, though the nature both of the substance and of the perceiving entity is unknown.

2. *Monists*, who reject the testimony of consciousness as to the ultimate duality of the subject and object, the ego and the non-ego. Monists fall into two classes, according as they do or do not preserve the equilibrium of subject and object:—

(i) *Objective Idealists*, who hold the doctrine of Absolute Identity. They admit the testimony of consciousness as to the co-equality of mental and material phenomena, but not as to the antithesis of mind and matter as existent entities. They maintain that mind and matter are only phenomenal modifications of the same unknown absolute reality; for, since the impenetrability of matter is intelligible only as a mode of resistance, the essence of matter must be some kind of power which it possesses in common with spirit. Matter and mind, or body and spirit, are therefore different aspects of a common substratum.

(ii) Those who deny the evidence of consciousness as to the co-equality of mental and material phenomena, and subordinate the one to the other entirely. Thus we have:—

α. *Idealists*, who maintain that the subject, the ego, was the original, and is the only fundamental; the object, the non-ego, being evolved from it as its product. The fundamental reality is psychical; all matter is, at bottom, of the nature of thought.

β. *Materialists*, who maintain that the object, the non-ego, was the original, and is the only fundamental; the subject, the ego,

being evolved from it as its product. There is nothing but matter. Mind, thought, consciousness, are all by-products, epiphenomena, mere debris resulting from material processes. Life and consciousness cease absolutely with the disintegration of the matter with which they are associated.

Thus both Idealists and Materialists believe in a reality, but in a single reality. They are, therefore, at the same time Monists and Realists.

It will be observed that all the different schools mentioned, Nihilists excepted, are Realists of some kind. The four main schools may be grouped in this way:—

1. Dualists.
    (*a*) Natural Dualists (sometimes called simply Realists).
    (*b*) Hypothetical Dualists (sometimes called Phenomenalists).
2. Monists.
    (*a*) Idealists.
    (*b*) Materialists.

But the dividing lines are by no means so clear cut as this simple classification would seem to indicate. One school tends to shade off into another, and sometimes they are scarcely distinguishable. Indeed, the terminology is most confusing, and is of a very varying connotation.

A few other terms require brief explanation.

Sensationalism maintains that all our knowledge comes to us through the senses, and refuses to admit that the mind is a co-contributor. *Empiricism* is sometimes confused with Sensationalism, but Empiricism admits that the mind must be something endowed with power to form judgments by comparing and contrasting the data supplied by the senses. All evidence derived from the senses is of particular truths. In every general truth there is an element of knowledge independent of such evidence, that is, independent of the data of the senses. Contrasted with Sensationalism is *Rationalism*, which asserts that the knowledge which comes to us through the senses is fallacious, for perception and experience can give us information concerning only particular instances, and can therefore never provide us with universal truths. The rationalist claims that *reason* is the sole source of real knowledge. Metaphysical Rationalism must not be confused with Theological Rationalism, which is the doctrine that denies the existence of any supernatural revelation. But in

both cases Rationalism is an uncompromising assertion of the absolute rights of reason throughout the whole domain of thought. Both Sensationalism and Rationalism are dogmatic, as with both it is an article of faith that we have the power of acquiring complete knowledge, in the one case exclusively by perception, in the other exclusively by reason. In contrast with this dogmatism is *Scepticism*, which always doubts, and sometimes denies, the possibility of our acquiring true knowledge at all.

Agnosticism asserts that our knowledge is limited to the phenomena of the external world and of the mind; and that we know nothing of the reality which may lie behind phenomena. The agnostic disagrees both with the man who asserts and with the man who denies the existence of reality underlying phenomena, for neither can prove his case. The agnostic says he "does not know" whether it exists or not. He will not agree even with the hypothetical dualist, who assumes an unknowable. Agnosticism is negative. It differs from *Atheism*, which positively denies the existence of a personal God. Agnosticism "does not know" whether there is a personal God or not.

The less liberal type of theologian naturally dislikes not only Atheism but also Materialism, for a materialist is necessarily an atheist. And he has no great love for agnostics, or even for phenomenalists, and he invariably speaks of them in disparaging terms and of their "materialistic tendencies". Towards Idealism he is much less hostile, though this attitude he finds it impossible to defend logically.

§ 6. Hypothetical and Natural Dualism

We have referred to the unknown real thing, the substratum or substance, which the Hypothetical Dualist assumes to underlie phenomena, the substance in which phenomena are supposed to "inhere". The term "phenomenon" is equivocal. In Science it refers to the positive facts of perception, as distinguished from their causes. Scientific thought, in dealing with the concrete things of Physical Science, investigates their nature, their causes, and their effects, and so goes beyond mere sense-perception. It assumes, for instance, the existence of atoms and of the æther, neither of which can be directly perceived at all. The atoms and the æther are inferred from a combination of observations and hypotheses. This

inferential process is an imaginable one, for any conceptual region is necessarily conceived as though it might be perceived; and by its means the atoms and the æther may be seen as if under an indefinitely powerful microscope. We can verify perceptually only up to a certain point; the weakness of our senses leaves a great deal unperceived and imperceptible. This conceptual region would, if our inferences and hypotheses are correct, and if our senses were sufficiently keen, be perceptible. It has to be admitted, unfortunately, that, while the human understanding attempts to construct conceptual systems because it is not satisfied with the contents of sense-perception alone, it sometimes uses these conceptual systems of its own construction for the purpose of disparaging sense-perception as an illusion, although aware, of course, that the suppositions of the conceptual system derive, from the data of perception, the whole of their vitality. Sometimes attempts are made to construct conceptual systems which are not clearly imaginable: that way lies inevitable danger.

(*a*) *Substance*

The Hypothetical Dualist's "Substance" is not phenomenal, for it cannot be made to appear to the senses. But although it is claimed to be more real than phenomena, its existence is merely inferred. Since, however, the inference is not verifiable, we may deny its legitimacy, especially as the "substance" cannot be made part of any conceptual system, for it is wholly unimaginable; and this really means denying the existence of the substance, and therefore the existence of matter. Such a denial admits of no answer, though it certainly carries no conviction. It carries no conviction because we cannot bring ourselves to believe that the external world would cease to exist if our minds were annihilated. We feel bound to believe in the existence of an external world quite independent of any percipient.

"Substance", then, is the term given by the Hypothetical Dualist to that elusive yet necessary something, that obscure substratum, common to all material things without being discoverable in any one of them, something which is never actually seen or in any other way experienced, yet something which is thought into things; nevertheless, a real thing though a transcendant thing. It is some kind of undiscovered basic reality, and the æther of Space may perhaps be *régarded* as the first stage of its phenomenality.

(*b*) *Primary and Secondary Qualities*

All the "phenomena" of the external world known to us in sense experience are logically reducible to a comparatively small number of common "attributes" or "qualities" which are (so the Hypothetical Dualist claims) inherent in the assumed underlying "substance". These qualities are distinguished as primary and secondary. Primary qualities are those derived from our muscular sense of resistance, such as solidity, extension or size, and motion. They are those attributes of the external world that are regarded as independent of the observer. Secondary qualities are those derived from our other senses, such as colour, sound, taste, smell, temperature. Science teaches us that the things of the external world have only the primary qualities, and that these are among the causes of the secondary qualities, though the secondary also depend upon the existence of a sentient being. The primary world is the seat of energy; permanent in time; the same whether we are present or absent; objective, and that about which there is no difference of opinion; measurable in three dimensions of Space, in duration of existence, and in energy. The secondary world is not permanent in time, does not exist in the absence of a sentient being, and is not satisfactorily measurable in any way except by referring its phenomena to primary standards. The secondary qualities have no independent existence in the external world: this is a scientifically established fact. If we think of the world or any part of it in the absence of all sentient beings, we think of it as absolutely dark and absolutely silent.

The secondary qualities are really subjective reactions excited by the primary qualities and objectified by association with them. The primary qualities are the most constant and unconditional in experience. Illusions are chiefly of seeing and hearing, whereas to touch or grasp a thing usually produces conviction. Since it is in their primary qualities that things are most exactly measurable in dimensions, weight, and movement, it is natural that Science should regard the primary qualities as pre-eminently real. But it must never be forgotten that the primary qualities are, as truly as the secondary, grounded in sensations, and therefore liable to misinterpretation.

The nature of the Hypothetical Dualist's "substance" is, admittedly, extraordinarily elusive; an unperceivable support of per-

ceivable qualities necessarily seems to be something without assignable character, resembling nothing in experience, and therefore explaining nothing. But if it exists at all, there is one quality we are bound to ascribe to it, and that is permanence in time, for this corresponds to the continuity of the experienced external world in the past and in the present. But, even so, permanence in time does not seem to help us establish any connection between the other primary qualities and the assumed substance, and such terms as "underlies" and "inhere" are metaphors, having no significant meaning. Still, the notion, though necessarily vague, of permanent transcendent substance does give coherence and unity to the phenomena of the external world we are familiar with.

If we accept Hypothetical Dualism, it is best to regard "substance" as a category—as one of those unverifiable, unanalysable, fundamental, ultimate, concepts which the mind is driven by necessity to try to form. If we reject the category as illusory, the argument as to the possible coherence of phenomena is reduced to nothing. If we accept it, that is, if we recognize substance as a category indicating the reality which is not immediately given to us in perception, yet felt to be necessary for the understanding of phenomena, and accept it either *a priori* or as the result of reflection upon experience, the term seems to suggest something which is not very far removed from the ordinary matter of Natural Dualism after all. Yet the distinction may be usefully preserved. The distinction is just what is required to make intelligible the doctrine of transubstantiation, for a change in the *substance* of the sacramental elements, though unimaginable, would then not be inconceivable, all the *qualities* of the elements, primary and secondary, remaining unchanged. Ultimately, perhaps, Physical Science will solve the problem of matter and substance, and there can be little doubt that the primary qualities of matter—resistance, extension, weight, motion—will give the key to the solution.

The position of the Hypothetical Dualist, as contrasted with that of the Natural Dualist, ought now to be clear. Both Dualists are Realists. But in the case of the Hypothetical Dualist, the real is only inferred, and perceptions are the perceptions of qualities only. In the case of the Natural Dualist, the real is apprehended immediately; he takes the common-sense view; he kicks against a stone and perceives it immediately and objectively—it is something solid and extended before him, and it can be measured and weighed: that object he takes to be matter. Common sense revolts against regard-

ing the object merely as an idea, or as nothing beyond an integrated heap of sensations; and this is the view of Science. But the view is difficult to maintain in its entirety, for it is certain that our positive knowledge, the knowledge that admits of no question, of external reality, is limited to our perception of qualities. Whatever we know beyond these qualities is known by inference only, and inferences of this kind seldom admit of complete verification.

§7. Monism: its Logical Consequences

The greatest antithesis in present-day Philosophy is that between the two monistic systems, Idealism and Materialism. Between these there is an unbridgable chasm. A particular system is, however, sometimes prevented from falling into absolute Idealism or absolute Materialism, and is held in a kind of vacillating equilibrium, because in some of its opinions an idealistic tendency is counteracted by a materialistic tendency in others.

It will be understood that the term Monism applies to any philosophic system which seeks to exhibit all the complexities of existence, both material and mental, as modes of manifestation of *one* fundamental reality. Idealism assumes that all fundamental reality is psychical—is, in fact, consciousness or mind. Materialism assumes that consciousness or mind is a mere by-product of the one fundamental reality—matter. Idealism reduces matter to mental elements. Materialism identifies thought or feeling with the nerve process which accompanies it.

(*a*) *Idealism*

We may consider Idealism first. Idealism maintains that whatever we know directly is reducible to ideas, and that ideas have an existence more real than the fleeting transient objects of sense: the existence of matter is nothing but an illusion. But there are so many forms of Idealism, and its terms are used in so many senses, that it is difficult to come to close quarters with its fundamental assumptions.

If "consciousness" be regarded as denoting the recognition by the mind of its own acts, and "mind" as that which thinks, wills, and feels, it may be said that consciousness is to the mind what extension is to the body, Though the analogy is imperfect, it is suggestive, for both consciousness and extension are essential qualities; we can neither conceive mind without consciousness nor

body without extension. But "mind" is sometimes spoken of as if it were precisely synonymous with "spirit". Yet while mind, like spirit, is always regarded as an unknown conscious something, mind is never conceived as extended in Space; whereas spirit, though incorporeal, immaterial, and invisible, is usually conceived as so extended, to be invested in human form, and to be a personality somehow associated with the body; it is always thought of as a substantial though immaterial entity which thinks, wills, and feels; it is regarded as invisible merely because our human sense of vision is limited to material things. But whereas a spirit is always conceived as an entity distinct from, though during life closely associated with, the body, the mind is seldom spoken of as if it were something that could exist independently of the body. The reader who takes up a book on Idealism must assure himself of the precise meaning attached to these terms by the writer.

All Idealists deny the existence of matter, though some of them say that all they really deny is the unknown substratum, "substance". Some Idealists recognize the existence of spirit as an entity which thinks, wills, and feels. Others limit their recognition to the much vaguer thing, mind, still conceived, however, as an entity of some sort. Still others assert that, since all we positively know of mind are the facts of consciousness, we are not justified in assuming the existence of mind as any sort of separate entity; and they maintain that the only real things in existence are mental facts, ideas. They make vague statements about a universal consciousness, all men's minds being alike, and each mind being a sort of temporarily separated portion of this universal consciousness.

Now if we are sure of anything, it is that consciousness is personal and individual; men's minds may in many respects be alike, but their differences are great and fundamental. A common consciousness is not only unimaginable, it is inconceivable. But more than this: Idealism altogether fails to explain the primary qualities of matter—extension, inertia, impenetrability. Despite his clever paradoxes, the Idealist cannot get rid of matter by dissolving it in mind. When material objects are in question, common sense refuses to admit that *esse* and *percipi* are identical. It is impossible to accept the ultimate logical conclusion of Idealism, that, with the expiring breath of the last sentient being, the whole universe disappears into nothingness.

(*b*) *Materialism*

To Materialism the only real world is the world of matter, the world of atoms with their primary qualities and motions. Life and consciousness are the products of matter, and manifest themselves in complexities of atoms. From such complexities life is, in favourable circumstances, spontaneously generated, and spontaneously generated living matter has, by blind chance, passed through the various stages of evolution until the human being reached his present state of development. There is no God, no soul, no freedom, no immortality. All psychical activity, all consciousness, is ultimately nothing more than a motion in and amongst the cells of the grey substance of the brain, possibly some form of wave-motion or of radiation set up by the movement. All thoughts and feelings are not merely accompaniments, but are identical with these nervous processes. The mind is nothing more than a function of the brain. All psychical facts are merely effects, though unexplained effects, of cell-movements in the brain. Thought bears much the same relation to the brain as bile does to the liver.

The weakness of Materialism lies in its vast assumptions. It gives no explanation of the ultimate origin of either matter or motion. It is impossible to believe that the thinking, feeling self of which each one of us is conscious, is only an automaton, and even the materialist is forced to admit that whether there is any causal connection between the psychical facts and the physical changes which accompany them, is wholly unknown. Materialism fails to give any satisfactory explanation of the nature and origin of life and consciousness. Common sense refuses to admit that consciousness is nothing but a movement of matter.

The claims of Materialism lead to far-reaching logical consequences. For the materialist asserts that all our volitions are mere links in mechanical chains of blind causes and effects. Now men act in consequence of motives, and their motives are thus the results of preceding facts, so that if we knew the antecedents of these facts and the laws that connect them, we could with infallible certainty predict the consequences, immediate and remote. If Adam had been a super-mathematician he might, automaton though he was—assuming that he was acquainted with all molar and molecular masses, their initial positions, direction of motions, velocities, and accelerations—have predicted the whole course of the world's history.

He might have written out a complete account, complete to the last detail, of the great European War; he might have predicted the date, place, and manner of death of the world's last mosquito; nay, he might have foretold the very terms of the marriage contract between the Tellurian Kaiser and the Martian Queen to be sealed a couple of centuries hence. To the materialist, the human will counts for nothing, and can affect nothing; our every decision is the infallible consequence of particular cerebral changes. The individual who, while balancing two courses, is under the impression that he is at liberty to pursue either, is completely under a delusion. The most calculating selfishness, the most heroic self-sacrifice, equally have been determined by chance aggregations of molecules. Newton did not write the *Principia*, or Shakespeare *Hamlet*; they were not creative personalities; they merely looked on while blind causes were at work. They were merely chance aggregations of molecules, constituting automata with fortuitously specially active cerebrations. So with all things that ever have been or ever will be produced. It is mere fancy, says the materialist, that we ever act from rational motives. No criminal is morally reprehensible; he is simply morally irresponsible. How can a materialist give his support to any sort of penal code? But to this question he can only logically answer that he, too, is irresponsible for his actions.

Conclusion

It cannot be said that either Idealism or Materialism is a fundamentally illogical system. Each is logically worked out, but neither is accepted because of the ultimate consequences traceable from its hypotheses; in each case the consequences are such that common sense declines to accept them, and this really means a rejection of the hypotheses on which the systems are constructed. In fact, every system breaks down that refuses to accept the cardinal facts of the duality of consciousness. Mind and matter are two entirely distinct things present to our consciousness; they cannot be reduced the one to the other, in the first place because resistance is incompatible with the attributes of mind or spirit, and in the second because consciousness is inexplicable by the qualities of matter. We may, if we like, recognize Materialistic Monism of body and an Idealistic Monism of spirit, combined in a unified Dualism of substance, namely, the unified substance of body and spirit, or matter

and mind, in the single personality of man. But the refusal to accept the great underlying fact of duality of consciousness is an act of philosophic suicide.

There is not a philosophic system but is open to attack, for every system rests on hypotheses which, ultimately, are not verifiable. Dualism of both kinds are attacked: Natural Dualism because it takes too much for granted, Hypothetical Dualism because of the assumption of unknown and apparently unknowable entities. Still, the ultimate consequences of Dualism are not so destructive as are the consequences of Monism.

No philosophic system is closed and final. Philosophic finality is still a philosophic dream.

There are some philosophers who are less anxious to understand the world of Science than to convict it of unreality. They shrink from the laborious study of the detailed knowledge derived from the senses, and prefer to pin their faith on the wisdom, sudden and penetrating, which they believe will reach them by reflection and reasoning. In their more emotional moods, a belief in the unreality of the world of Science arrives with irresistible force, and when this emotional intensity subsides they seek for logical reasons in support of that belief.

The attitude is altogether wrong. Like the man of Science, the philosopher must lay aside his hopes and wishes when he studies his subject. There must be no shrinking from hard facts, no demand in advance that the world shall conform to preconceived desires. Knowledge of the universe is not hidden by a flimsy veil that can be easily torn aside; it is very hard to come by.

Common opinion prevails that metaphysical disquisition is idle, because the problems discussed are really never solved. It is quite true that Philosophy has made greater claims and achieved fewer results than any other branch of learning. It has made many rash assertions and many rash denials. Yet some of the greatest thinkers since the age of ancient Greece have devoted their lives to philosophical problems, and no one would dream of calling them either shallow or insincere. That progress has been slight is inevitable, for the great mass of Philosophy is necessarily purely speculative. There is very little philosophical truth finally established, and additions can be made only at the cost of much labour, very slowly, and only then if the method of Science is made the method of Philosophy. Existing systems are often ingenious, even sublime, but they nearly

always lay claim to finality and completeness. And it is for this reason that many philosophers are still the playthings of the gods.[1]

CHAPTER IV

Opinion and Truth

§1. Belief and Testimony

All beliefs[2] are to some extent influenced by the wish to believe, and the wish to believe has the strongest influence in many matters which closely concern us. "It has always been open to remark," says Bain, "how completely human beings are the slaves of circumstances in the opinion that they entertain upon all subjects that do not appeal directly to the senses and daily experience. We see in one country one set of beliefs handed down unchanged for generations, and in another country a totally different set equally persistent."[3] There is very little independent judgment among mankind, and evidently, therefore, it must be possible by some means to make one opinion prevail rather than another. The arts of swaying men's convictions are, of course, numerous and well-known. "Look at the whole army of weapons in the armoury of the rhetorician. Look at the powers of bribery and corruption in party warfare. Consider also the effect of constantly hearing one point of view to the exclusion of all others."[4] Then, again, any strong feeling possessing the mind gives such a determination to

[1] The reader may usefully consult the following volumes: A. J Balfour, *Defence of Philosophic Doubt*, and *Foundations of Belief*; T. Brown, *Lectures on the Philosophy of the Human Mind*; Sir W. Hamilton, *Lectures on Metaphyics and Logic*; G W. Leibnitz, *New Essays concerning Human Understanding*; G. H. Lewes, *The Physical Basis of Mind*; J. M'Cosh, *Examination of Mill's Philosophy*; J. S. Mill, *Logic*, and *Examination of Sir W Hamilton's Philosophy*; K. Pearson, *Grammar of Science*; H. Poincaré, *Science and Hypothesis*, and *Science and Method*; Carveth Read, *The Metaphysics of Nature*; F C. S Schiller, *Riddles of the Sphinx*; H. Sidgwick, *Philosophy, Its Scope and Relations*; H. Spencer, *First Principles*; Dugald Stewart, *Philosophy of the Human Mind*; J. Ward, *Naturalism and Agnosticism*.

Also W B Carpenter, *Nature and Man*; J. Grote, *Exploratio Philosophica*; I. Kant, *Critique of Pure Reason* and *Kritic of Judgment*; H. L. Mansel, *Metaphysics*, and *Philosophy of the Conditioned*; J. M'Cabe, *Evolution of the Mind*; T. Reid, *Active Powers of the Human Mind*; A. Riehl, *Science and Metaphysics*; J. Veitch, *Dualism and Monism*.

[2] It should be noticed that the real opposite of belief, as a state of mind, is not disbelief, but doubt, uncertainty; and the close alliance between this and the emotion of fear is stamped on every language.—Bain, *Emotions and the Will*, p. 535.

[3] Bain, *ib.* p. 522.

[4] Bain, *ib.*

the thoughts and the active impulses as to pervert the convictions, and to dispose us to trust or distrust at that moment things that we should not trust or distrust at another time. In the elation of successful enterprise just achieved, we are apt to have a degree of confidence in our own powers that we do not feel in ordinary times, and very much in contrast to what we feel at some miscarriage or failure, or at a time when we are affected by some bodily ailment.[1]

Belief in testimony contains all the elements of experience, intuition, and emotion, in varying degrees, but there is also a more subtle element of influence, arising from the peculiar power exerted by one man upon another. All those circumstances which lend impressiveness to a speaker, and render the orator an artist, dispose the hearers to accept his statements with more than the deference due to the testimony of a single person. Emotion, in such cases, exercises an interference of its own kind.[2] The preacher and the politician often take an unfair advantage of this, and purposely forget that it is their duty to lead their followers to the truth. As Guyau says,[3] charlatans and orators are generally familiar with the contagious power of affirmation. It is interesting to watch the gradual development of emotion, becoming more and more uncontrolled, as a clever party politician addresses a meeting. The psychology of a multitude is always an instructive study.[4]

"Few, indeed, are the beliefs", says Mr. Balfour, "which any individual thinks himself called upon seriously to consider with a view to their possible adoption. The residue he summarily disposes of, rejects without a hearing, or, rather, treats as if they had not even that *prima facie* claim to be adjudicated on which formal rejection seems to imply.

"Now can this process be described as a rational one? That it is not the immediate result of reasoning is, I think, evident enough. All would admit, for example, that once the mind is closed against the reception of any truth by 'bigotry', or 'inveterate prejudice', the effectual cause of the victory of error is not so much bad reasoning as something which, in its essential nature, is not reasoning at all. But there is really no ground for drawing a distinction as regards their mode of operation between the 'psychological climates' which we happen to like and those of which we happen to disapprove. For good or for evil, in ancient times and in modern, it is ever by the identic process that

[1] *ib.* p. 544. [2] *ib.* p. 549. [3] *Education and Heredity*, p. 17.
[4] Cf. Matthew Arnold on "Tolstoi", *Essays*, Second Series, p. 291.

they have sifted and selected the candidates for credence, on which reason has been afterwards called to pass judgment; and that process is one with which ratiocination has little or nothing directly to do."[1]

Professor Carveth Read points out how Philosophy, coming to us late in life, meets at the outset with a great difficulty,—how to begin the discrimination of truth and error, what to accept, what to reject. The mass of beliefs, ingrained in childhood and youth, abides with us and is necessarily amongst the foundation of reason. "The influence of social life in the various forms of education, tradition, authority, common sense, confirm alike our sciences and our superstitions. Family, school, church, and state, instruct the boy and the man what to think and what to do. Inheriting a nature fit for such a life, his instincts of imitation, honour, sympathy, reverence, and the rest, co-operate in delivering him over to the great tutor or arch-sophist (however you regard it) Society. The habit of believing assertion becomes almost instinctive, gives opportunity to liars and other imaginative persons. Falsehood and romance, imperfectly differentiated, flourish amongst children and savages; and this is quite natural, for deceit is common in organic nature."[2]

Most of our prejudices have become so ingrained that we are hardly conscious of them. And almost every day we are adding to them, for we cannot completely rid ourselves of the old habit of hastily taking up opinions which we have not properly examined, —opinions from the daily press, opinions derived from the assertion of others, and opinions due to superficial observation. Unreasoned opinions are curiously infectious, and the presumption that any current opinion is not wholly false gains in strength according to the number of its adherents. Herbert Spencer thinks we must admit that the convictions entertained by many minds in common are the most likely to have some foundation,[3] but this is a dangerous doctrine to apply to the unreasoned opinions of the mob. As Dugald Stewart says, if no test or criterion of truth can be pointed out but universal consent, may not all those errors which Bacon has called *idola tribus* claim a right to admission among the incontrovertible axioms of Science?[4] We cannot, however, always avoid following current opinion, even if we would, and sometimes we follow it from deliberate choice. In this respect, fear of possible

[1] *Foundations of Belief*, p. 205.
[2] *Metaphysics of Nature*, pp. 12-3.
[3] *First Principles*, p. 4.
[4] *Works*, p. 322.

ridicule is often the determining factor. Mr. Balfour, speaking, for instance, of æsthetic, remarks that the inclination to admire what squares with some current theory of the beautiful rather than what appeals to any real feeling for beauty, is so common that it has ceased even to amuse.[1] Opinions are influenced by the prevailing fashion. Men fear singularity more than error; they accept numbers as the index of truth; and they follow the crowd. The dislike of labour, the fear of unpopularity, the danger even of setting up individual opinion against established convictions and the voice of the multitude, contribute to strengthen this inclination.[2]

§ 2. Authority and Reason

It seems clear that our opinions and beliefs are for the most part borrowed, and that the common tendency is to repose blindly on authority. Now *ought* we to put our trust in authority, or ought we always to appeal to Reason? To what extent can we accept Huxley's dictum—that the mental power which will be of most importance in our daily life is the power of seeing things as they are without regard to authority?[3]

The argument of those who urge that we cannot trust our Reason, and that we must inevitably fall back upon authority, is something of this kind: since we cannot throw over the engineer, the lawyer, the doctor, and others with expert knowledge, are we not *bound* to resort to authority? how can Reason possibly help us in cases where a lifetime must be spent in acquiring a knowledge of the technicalities in every one of a score of different professions? But such an argument is beside the point; the illustration is misleading, as illustrations often are. Expert opinion is one thing; opinion on everyday affairs another. The man in the street willingly accepts on trust the work of the mathematician, the man of science, the historian, the engineer, the jurist, and the medical man; but as long as human nature remains what it is, the opinions of the social reformer, the politician, and the theologian, will never be accepted by a large number of people, who, rightly or wrongly, think they are entitled to their own "opinion" on all such subjects as social matters, politics, and religion,—subjects which, as we all know, form topics of perennial discussion.

Now the statement that the rival opponent of authority is

[1] *Foundations of Belief*, p. 254.
[2] Sir G. C. Lewis, *Influence of Authority in Matters of Opinion*, p. 15.
[3] *Science and Education*, p. 96.

Reason seems to most persons, says Mr. Balfour,[1] equivalent to a declaration that the latter must be in the right and the former in the wrong; while popular discussion and speculation have driven deep the general opinion that authority serves no other purpose than to supply a refuge for all that is most bigoted and absurd.

The current theory by which these views are supported may be summarized in this way. "Everyone has a 'right' to adopt any opinions he pleases. It is his 'duty' before exercising this right critically to sift the reasons by which such opinions may be supported, and to adjust the degree of his convictions in such a manner that they shall accurately correspond with the evidences adduced in their favour. Authority, therefore, has no place among the legitimate causes of belief. If it appears among them, it is as an intruder, to be jealously hunted down and mercilessly expelled. Reason and Reason only can be safely permitted to mould the convictions of mankind."

"Sentiments like these are among the commonplaces of political and social Philosophy. Yet, looked at scientifically, they seem to me," Mr. Balfour continues, "to be not merely erroneous but absurd. Suppose for a moment a community of which each member should deliberately set himself to the task of throwing off as far as possible all prejudices due to education, where each should consider it his duty critically to examine the grounds whereon rest every positive enactment and every moral precept which he has been accustomed to obey; to dissect all the great loyalties which make social life possible, and all the minor conventions which help to make it easy; and to weigh out with scrupulous precision the exact degree of assent which in each particular case the results of this process might seem to justify. To say that such a community, if it acted upon the opinions thus arrived at, would stand but a poor chance in the struggle for existence, is to say far too little. It could never even begin to be".[2]

Thus does Mr. Balfour fill would-be consistent thinkers with feelings of discomfort. Must we, then, forbear to question our prejudices lest we should bring about a social cataclysm? But is Mr. Balfour's supposition, with all its dreaded consequences, so destructive of the main argument as at first it seems? Is he not himself an excellent example of such a community as he imagines? Does *he* accept authority? Has he not spent his life in bidding people appeal to Reason?

[1] *Foundations of Belief*, p. 195. [2] *ib.* p. 196.

But let us hear him again: "The identification of Reason with all that is good among the causes of belief, and authority with all that is bad, is a delusion so prevalent that a moment's examination into the confusions which lie at the root of it may not be thrown away. The first of these confusions may be dismissed almost in a sentence. It arises out of a tacit assumption that reason means *right* reason. Such an assumption, it need hardly be said, begs half the point at issue. Reason, for purposes of this discussion, can no more be made to mean right reason than authority can be made to mean legitimate authority".[1]

Here we are all bound to agree with Mr. Balfour. And, as he points out elsewhere, when we reason we are the authors of the effect produced; we have ourselves set the machine in motion, and, for its proper working, we alone are responsible. If, therefore, the machine is imperfect—if our reasoning is faulty or our knowledge defective—we should probably be wiser to put our trust in authority. But the important thing is to choose safe and able guides in matters which we cannot or ought not to judge for ourselves.[2] It has, however, to be remembered that, in choosing our guides, we are choosing them by the light of our own reason, and, more probably than not, we shall allow our prejudices or our affections to operate even in making this choice.

We are sometimes deceived when we think that certain beliefs we entertain are the rational product of strictly intellectual processes. We have, in all probability, only got to trace back the thread of our inferences to its beginnings in order to perceive that it finally loses itself in some general principle which, describe it as we may, is in fact due to no more defensible origin than the influence of authority. For authority moulds our ways of thought in spite of ourselves and usually unknown to ourselves. "The power of authority is never more subtle and effective than when it produces a psychological 'atmosphere' or 'climate' favourable to the life of certain modes of belief, unfavourable and even fatal to the life of others."[3]

To Spencer's mind, remarks Professor Sadler, scientific teaching seemed inseparably connected with discovery, with the overthrow of false authority, with liberation. But it is quite conceivable that, in future, when physical science sits on the throne of undisputed authority, she may impose on education as heavy a weight of pre-

[1] *Foundations of Belief*, p. 202. [2] Cf. Sir G. C. Lewis, *Influence of Authority, &c.*, p. 65.
[3] Cf. *Foundations of Belief*, pp. 203, 206, 228.

scribed belief as did any earlier régime. All true education strikes a balance between authority and independent research. It is therefore expedient always to have a due place in education for that subject or group of subjects in which the most strenuous movement of enquiry and investigation is going forward. The spirit of search is caught from teachers who are themselves researching, not simply by artificially reproducing the methods of search in provinces of knowledge already accepted.[1]

It is the claim of authority to unchallenged acceptance that we are all bound to dispute. In the Middle Ages the search after a causative explanation of phenomena was represented by its opponents as hostile to the authority of the State and subversive to the Christian faith, and such views are not entirely unknown even at the present day.[2] It is perhaps natural that persons in authority should dislike their authority questioned, and here perhaps lies the clue to the dislike, felt in some quarters, to any general adoption of scientific method, since such method cannot but result in independent judgment. Children who are taught to think for themselves, to sift evidence, to get at all essential facts, are likely, later on, to prove formidable opponents to illogical systems. But many a generation will pass away before Mr. Balfour's community of intellectual anarchists will be born. The mass of mankind will never have any ardent zeal for seeing things as they are; very inadequate ideas will always satisfy them. Whoever sets himself to see things as they are will find himself one of a very small circle, but it is only by this small circle resolutely doing its own work that adequate ideas will ever get current at all.[3]

Authority, then, we must treat with respect; yet we must always be on our guard against its too arrogant claims. Whilst we may credit it with being sincere, we must ask it to produce its credentials. We should beware of the limitations of Reason, but we should never hesitate to use Reason. We should always make an insistent demand for facts, and do our utmost to search amongst them for the Truth. As Schiller says, the real philosopher is one who always loves truth better than his system. We should, then, throw away our systems as soon as we have convicted them of error; we should never hesitate to revise our opinions in the light of fresh facts. Change of mind is not inconsistency.[4]

[1] *Science in Public Affairs*, p. 58. [2] Cf. *Lectures on the Method of Science* (Strong), p. 85.
[3] Cf. Matthew Arnold, *Essays*, vol. i, p. 25; and Locke, *Conduct of the Understanding*, § 12.
[4] "*Nemo doctus unquam mutationem consilii inconstantiam dixit esse.*" (Cicero). Cf. Arnold, *Essays*, vol. i, p. 31.

§ 3. The Nature of Truth

Perhaps a teacher's most imperative duty is to inculcate the desire of probing a thing to its depths. The desire for thorough work is one and the same thing with the desire of finding the truth, for a little experience forces us to recognize that the truth is never near the surface, and that we must always dig and labour before reaching it.[1]

To define Truth is difficult and is really unnecessary, for, in its more general sense, it is perfectly well understood. Professor Baldwin tells us that he once put to a sixteen-year-old girl the question, "What would you say it is that the true is true to?" After some thought she said, "It is true to itself." "Why?" "Because the truth is that which doesn't pretend to be anything else."[2] There it is, in a nutshell.

The cultivation of such a cardinal virtue as truth must tell in every department of life, and its cultivation depends, in no small degree, upon the work of the Science teacher. The moral disposition to veracity can avail little without the tests and methods of distinguishing true from false, while men well versed in these seldom quarrel about matters of fact. The disputes of the scientifically educated are narrowed to some very special and difficult issues.[3] The Science teacher should tell his pupil that it is his duty to doubt until he is compelled, by the absolute authority of nature, to believe what other people tell him about Science. Pursue this discipline carefully and conscientiously, and we may make sure that, however scanty may be the measure of information which we have poured into the boy's mind, we have created an intellectual habit of priceless value in practical life.[4]

"The philosopher", says Faraday, "should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biased by appearances; have no favourite hypotheses; be of no school; and in doctrine have no master. He should not be a respecter of persons but of things. Truth should be his primary object. If to these qualities be added industry, he may, indeed, hope to walk within the veil of the temple of nature." The ideal intellect is a clear cold logic engine, as Huxley would say.[5]

Every teacher should realize that all Truth is relative and cannot

[1] Cf. Guyau, *Education and Heredity*, p. 175
[2] *Thought and Things*, vol. ii, p. 367.
[3] Bain, *Science of Education*, p. 162.
[4] Cf. Huxley, *Science and Education*, p. 128
[5] *Science and Education*, p. 86.

be absolute. As our knowledge grows we have to revise our ideas. The truth of yesterday may be falsehood to-day. Meanwhile we have to live to-day by what truth we can get to-day, and be ready to-morrow to call it falsehood. "Ptolemaic astronomy, Aristotelian logic, scholastic metaphysics, were expedient for centuries, but human experience has boiled over those limits, and we now call these things only relatively true, or true within those borders of experience. 'Absolutely', they are false, as everyone now knows."[1] The history of thought consists of little more than an accumulation of abandoned explanations, yet these explanations passed as Truth within the limitations of the experience of their own day.

"The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It becomes true, it is *made* true by events. Its verity *is*, in fact, an event, a process, the process, namely, of its verifying itself.

"The 'absolutely true', meaning what no further experience will ever alter, is that ideal vanishing point, towards which we imagine that all our temporary truths will some day converge. It runs on all-fours with the perfectly wise man, and with the absolutely complete experience; and if these ideals are ever realized, they will all be realized together."[2] But that goal is a long, long way off.

The teacher should bear in mind that Science is the most perfect embodiment of the Truth, and of the means of getting at the Truth. More than anything else does it impress the mind with the nature of evidence, with the labour and precautions necessary to prove a thing. It is the best possible corrective of the laxness of the natural man in receiving unaccredited facts and conclusions. It exemplifies the devices for establishing a fact or a law in every variety of circumstances; it saps the credit of everything that is affirmed without being properly attested.[3] The method of Science is, in short, above all things the method of discovering the truth.

Let the Science teacher, then, be on his guard against dogmatizing. His chief business is to teach, not to lecture; to guide, not to tell. Let him remember the maxim, "*In primis, hominis est propria veri inquisitio atque investigatio*".[4] To lead his scholars to the pursuit and investigation of Truth should be his highest aim.

[1] *Pragmatism*, p. 223.
[2] *ib.* Cf. also William James, *The Meaning of Truth*, especially ch. ii.
[3] Bain, *Education as a Science*, p. 147.
[4] Cicero, *De Officiis*, i, § 13.

CHAPTER V

The Sophists and Socrates[1]

§ 1. First Attempts at Investigation

It was with the most unbounded confidence that the early philosophers of Greece entered upon the work of physical speculation, and they quite expected to be able to divine, at a single glance, the whole import of Nature's book. The smaller problems of Science they usually scorned to try to solve; they aspired to a complete and immediate knowledge of the origin and the controlling principles of the universe itself. According to Thales, *water* was the origin of all things; according to Anaximines, *air*; and Heraclitus considered *fire* as the essential principle of the universe. And about these conclusions they had no doubt at all.

There are, however, to be found more limited examples of inquiry concerning the causes of natural phenomena, and in these we are able occasionally to discern some slight anticipation of the true spirit of the scientific investigator. One of the most striking instances of this kind is to be found in the speculations which Herodotus records relative to the cause of the floods of the Nile.

Although Herodotus questioned the Egyptians closely about this matter, he learned very little from them. They appear not only to have had no theory about the cause of the floods, but to have felt no want of a theory. The Greeks, on the other hand, had shown more curiosity, but their hypotheses were so far unacceptable to Herodotus that he substituted one of his own. Not only, however, did Herodotus' own hypothesis not cover all the facts, but it spoke of the sun *drawing* the water, and exactly what was meant by this ambiguous term, the historian neglected to say. It is, in fact, obvious that he had but the vaguest notions of evaporation, and we are forced to conclude that, despite his inquiry into the facts, his hypothesis was merely a loose conjecture. And this was typical of the Greek speculators. As soon as they had formed any general conceptions, they proceeded to scrutinize these by the internal light of the mind alone, without any longer looking abroad into the world of sense. They quite underestimated the value of observation,

[1] Socrates, about 468–399 B.C.

and quite neglected verification. They put their faith in *a priori* methods, and they failed utterly.[1]

§ 2. The Sophists

The Sophists were teachers of Rhetoric, and the litigious quibbling nature of the Greeks was the soil on which an art like that of disputation was easily made to flourish. But the only testimony we have of the Sophists is that of the Philosophers, their opponents, and as a strong dislike is so often the inevitable consequence of any marked difference of creed, we feel bound to accept with considerable hesitation the evidence by which alone we are able to judge of them.

Certain passages from Plato suggest that the Sophists were held in profound contempt generally, but it is very doubtful if this was actually the case. They were certainly wealthy and powerful; they were men of quite exceptional ability; they were, in large measure, the intellectual leaders of their age; and they were often selected as ambassadors on very delicate missions. No doubt they were objects of aversion to some—successful men always are; and no doubt they were more than a little unscrupulous. But the general feeling towards them was probably one of dislike, possibly also of fear, rather than of contempt, except, of course, on the part of the Philosophers, who despised them for their insincerity, superficiality, and shallowness, and hated them for the popularity which their specious and dazzling rhetoric easily secured and maintained.

The great boast of the Sophists was that they taught the art of "making the worse appear the better reason", but in this it is doubtful if they can be said to have been guilty of anything specially reprehensible in a Greek, however much such serious thinkers as Socrates and Plato might despise the shallow philosophy from which it sprang.

Lewes puts in a strong defence of the Sophists, and calls to his aid Macaulay's essay on Machiavelli, in order that we may see how their doctrines might have been held by very virtuous men. "Habits of dissimulation and falsehood, no doubt, mark a man of our age and country as utterly worthless and abandoned; but it by no means follows that a similar judgment would be just in the case of an Italian in the Middle Ages." Lewes also bids us look at home, and asks, Does not every barrister exert his energy, eloquence,

[1] Cf. Whewell, *History of the Inductive Sciences*, i, pp. 19–28; and *Herodotus*, Book II.

subtlety, and knowledge "to make the worse appear the better reason"? Indeed there seems to be little to choose between the Sophists and the present-day pleaders in our courts of justice. If a barrister has a bad case, does he not set himself deliberately to deceive the jury? does he not use every device known to Rhetoric to appeal to the emotions and to obscure the facts? does he not try to injure the character of his opponent's witnesses? He is not paid to establish the truth but to win his case, and, if he does this, no matter how unscrupulous his methods, few will think any the worse of him. In private life he may be the most moral, perhaps the most religious of men. His professional work is supposed to meet a public want, and in any case it forms an admirable training for the political platform. Who, then, would deny him his reward? If the honours due to an honourable profession are thrust upon him, may we not assume that the Sophists were similarly esteemed?

The Sophists appear to have given up Philosophy because they had come to the conclusion that there was no possibility of discovering Truth; any attempt to penetrate the mysteries of the universe they believed to be utterly vain. It was this that caused them to begin to consider their relations to other men, and so it came about that they devoted themselves to Politics and Rhetoric. If, they thought, there was no possibility of Truth, there only remained the possibility of Persuasion. They were convinced that there were no such things as Right and Wrong by nature, but only by convention. As orators they treated Truth with disdain, and devoted their talents to perfecting the mere outward form of expression. They were, it is true, a standing protest against the absurd metaphysical science of their day, but there can be no doubt—even if Plato is guilty of great exaggeration—that they were pretentious and insincere to the last degree.[1]

§ 3. Socrates and his Method

Whilst the Sophists were teaching the word-jugglery which they called Disputation, there suddenly appeared among them a strange antagonist. Outwardly the stranger's appearance was mean. Short of stature, thick-necked and somewhat corpulent, with prominent eyes, with nose upturned and nostrils outspread, with large mouth

[1] According to Plato the office of the Sophist was to teach immorality and openly to avow immorality. But this statement carries its own contradiction, since no set of men could preach doctrines acknowledged to be subversive of all morality, without incurring the heaviest penalties of the State.

and coarse lips, he seemed the embodiment of stupidity. "But, inwardly, he was so just that he never did an injury to any man; so temperate that he never preferred pleasure to right; so wise, that in judging of good and evil he was never at fault." His self-control was absolute; his powers of endurance were unfailing; he had so schooled himself to moderation that his very scanty means easily satisfied all his wants. With a shrew for his wife, he submitted to her violent temper with an equanimity which is proverbial. Such was Socrates; and his intellectual gifts were hardly less remarkable than his moral virtues. Naturally observant, acute, and thoughtful, he developed these qualities by constant and systematic use. But though intellectually the acutest man of his age, he liked to represent himself in all companies as the dullest person present.[1]

Socrates soon became known to every citizen in the market-place, but he always declared that he knew nothing. "When you professed knowledge on any point, especially if admiring crowds gave testimony to that profession, Socrates was sure to step up to you and, professing ignorance, entreat to be taught. Charmed with so humble a listener, you began. Interrogated, you unsuspectingly assented to some very evident proposition; a conclusion from that, almost as evident, received your assent. From that moment you were lost. With great power of logic, with great ingenious subtlety, and sometimes with daring sophistication, he weaved a web from which you could not extricate yourself. Your own admissions were proved to lead to monstrous conclusions, and you could not see where the gist of the sophism lay. The laughter of all bystanders bespoke your defeat. Before you was your adversary, imperturbably calm, apparently innocent of all attempt at making you ridiculous. Baffled and disgusted you left the spot indignant with yourself, and vowing vengeance upon your adversary."[2]

Socrates found men confidently making affirmations with words which they had never troubled themselves to define, and persuaded that they required no further teaching, yet at the same time unable to give clear or consistent answers to his questions, and so showing themselves to be destitute of real knowledge.[3] Socrates did not attempt to dogmatize; his system was simply that of question and answer. By a systematic cross-examination, his opponent was compelled to pass judgment upon himself, and perhaps induced to substitute a better opinion for a worse. If, as often happened, the

[1] Cf. Lewes, *Biog Hist. of Phil.*, pp. 130–9; and Adolph Harnack, "Socrates", *Ency. Brit*
[2] Lewes, *Biog. Hist. of Phil.*, pp. 139–40. [3] Cf. Grote's *Plato*, Preface, p. lx.

respondent, defeated, withdrew from the inquiry, he had in Socrates' judgment gained something, for he had in some measure become conscious of his ignorance. If, however, he did not shrink from a new effort, Socrates was ready to help him with further questions of a constructive sort. Such were the peculiar features of the Socratic method, and the method was adopted by Plato. Plato's writings are thus cast in Dialogue form, and the introduction of Socrates himself into the Dialogues seems to invest them with special interest and authority.

The Dialogues are seldom easy to follow, and it will suffice here to give an imaginary one, expressed in terms as simple as possible. We will suppose Socrates engaged in discussion with Euclid.[1]

Soc. I am afraid, Euclides, I know nothing of Geometry, though the subject interests me greatly, for I believe you said that the whole science has for its basis a number of axioms.

Euc. That is so.

S. You said, did you not, that these axioms carry with them an inherent authority, that their truth is self-evident?

E. Yes.

S. So that if anyone ventured to deny the truth of an axiom, if, for instance, anyone said that the whole is not always greater than its part, you would say——

E. I should say he was a fool.

S. Evidently your convictions are planted on a rock. Few branches of knowledge appear to have such firm foundations as Geometry.

E. That is generally admitted.

S. I feel so much interested that I should like to refer to the problems you were working with your pupils this morning. You were making squares, bisecting lines, and so on, afterwards proving that your constructions were correct. The work was, I suppose, comparatively simple?

E. It was of the most elementary character.

S. Let me then draw the square ABCD.—Does my figure satisfy you?

E. You are a most promising pupil, Socrates.

S. I may, perhaps, vary the problem you were working in your last lesson, and will therefore draw AE outside the square, equal to AB, and making an acute angle with AB. And I will join CE.

[1] That is with Euclid the mathematician, not with Socrates' own pupil.

E. Your construction is perfectly clear.

S. Suppose, now, I bisect CB in H, and through H draw HO at right angles to CB. Suppose, too, that I bisect CE in K, and through K draw KO at right angles to CE. Since CB and CE are not parallel, the lines HO and KO must meet, must they not, in some point O?

E. Evidently.

S. Let me give the figure a more finished appearance by joining OA, OE, OC, and OD. I will now try to examine this figure, as nearly as possible following your own method. Perhaps you will help me if I get into difficulties?

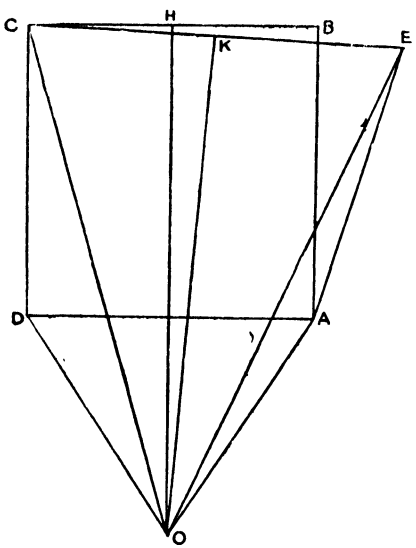


Fig. 1

E. Willingly.

S. Since KO bisects CE and is perpendicular to it, OC is equal to OE?

E. Excellent, so far.

S. And since HO bisects CB and DA, and is perpendicular to them, OD is equal to OA?

E. You are quite correct.

S. And, by construction, DC is equal to AE?

E. Certainly.

S. Therefore the three sides of the triangle ODC are equal, respectively, to the three sides of the triangle OAE, so that, by the 8th proposition of your First Book, the angle ODC is equal to the angle OAE.

E. I have never known a clearer demonstration.

S. But since OD is equal to OA, therefore the angle ODA is equal to the angle OAD?

E. Obviously.

S. Hence the angle ADC (the difference of the angles ODC and ODA) is equal to the angle DAE (the difference of the angles OAE and OAD)?

E. I suppose I must admit that.

S. But is not the angle ADC equal to the angle DAB, both being right angles?

E. I cannot deny it.

S. Therefore the angle DAB is equal to the angle DAE, or the part is equal to the whole?

E. It certainly appears so.

S. So that at least one of your axioms is not true?

E. That, you have demonstrated.

S. You will admit, then, Euclides, that some of the claims of mathematicians cannot be substantiated?

E. You have clearly proved, Socrates, that mathematical truth is sometimes, if not always, a fiction.[1]

Socrates became the most formidable antagonist the Sophists ever encountered. While the Sophists denied the possibility of Truth, Socrates sought to make Truth evident, and he never neglected an opportunity of refuting them. In a manner at once playful, ironical, and often quibbling, he seemed to take a delight in covering them with ridicule.[2]

"Socrates, though esteemed and admired by a select band of adherents, incurred a large amount of general unpopularity. The public do not admit the claim of independent exercise for individual reason. In the natural process of growth in the human mind, belief does not follow proof, but springs up apart from and independent of it; an immature intelligence believes first and proves (if, indeed, it ever seeks proof) afterwards. The community, themselves deeply persuaded, will not hear with calmness the voice of a solitary reasoner, adverse to opinions established; nor do they like to be required to explain, analyse, and reconcile those opinions."[3]

Socrates was a reformer, and therefore had to combat with existing prejudices. Pure as his intentions were, his actions and opinions were offensive. He incurred the hatred of party spirit, and by that hatred fell. At the age of seventy-two he had to appear before his judges to answer the charges of impiety and immorality. His condemnation was a foregone conclusion.[4]

Socrates is usually credited with having produced a revolution in thought, in consequence of which he is regarded by some as the founder of Greek philosophy, properly so called.

As Socrates reflected, he began more and more clearly to perceive that words, besides being the instruments by which we govern others, are means by which we may become acquainted with our-

[1] With apologies to Mr. Rouse Ball, and all other mathematicians. The fallacy is, of course, obvious The interested reader may now turn to some of Plato's Dialogues, where he will find some much harder nuts to crack. See the next chapter.

[2] *Biog. Hist. of Phil.*, pp. 138-140. [3] Grote, *Plato*, vol. i, p. 251.

[4] Cf. Lewes and Harnack.

selves. In trying really to understand a word, to ascertain the precise meaning which he himself gave it, he found that he gained more insight into his own ignorance, and at the same time that he acquired more real knowledge, than by all other studies together. He therefore decided that he must lead his disciples to inquire what they actually meant by the words of the propositions they were using, and no time could possibly be wasted which was honestly spent in such labour.

No doubt an opponent who had adopted a certain proposition and was provided with abundance of arguments in defence of it, would be irritated and vexed beyond measure by finding himself not fairly encountered upon those arguments, but led back into a question which he had assumed, forced to give account of a word which he fancied everyone was agreed upon, and not permitted after all to bring any of his own resources into play. It was most perplexing for a disciple who had come expecting that a certain doctrine would be either established or refuted, to find that he got no decision either way, and moreover that he had been talking all his life in a language which he did not understand, and using words as if they were algebraic characters.[1]

The aim and purpose of Socrates was confessedly to withdraw the mind from its contemplations of the phenomena of nature and to fix it on its own phenomena. He sought truth by looking inwards, not outwards. His main instruments were Definitions.[2] By Definitions he separated the particular thought he wished to express from the myriad of other thoughts which clouded it.

Socrates did not occupy himself with any particular branch of Science, but directed his attention to Science in general,—to method. "Man is the measure of all things," said Protagoras; "and as men differ there can be no absolute Truth." "Man is the measure of all things," replied Socrates; "but descend deeper into his personality, and you will find that underneath all varieties, there is a ground of steady Truth. Men differ, but men also agree; they differ as to what is fleeting; they agree as to what is eternal. Difference is the region of opinion; agreement is the region of Truth: let us endeavour to penetrate that region."

Socrates did not invent systems but only a method. He believed that in each man lay the germs of wisdom. He believed that "no science could be *taught* but only *drawn out*". "To borrow the ideas

[1] Maurice, *Moral and Metaphysical Philosophy*, vol. i, pp. 126-7.
[2] Cf. Aristotle, *Met.* xiii, c. 4; and G. H. Lewes, *op. cit.* p. 152.

of another was not to learn; to guide oneself by the judgment of another was blindness." Each man must conquer truth for himself, by rigid struggle with himself. And this is Socrates' great lesson for the teacher. It is not what the teacher does for the pupil, but what the pupil does for himself, that matters.

Socrates' method, which constitutes his real philosophical importance, has long since been discarded. Science was bound to discard it. Distinctions in words were mistaken by Socrates for distinctions in things. The nature of a thing can never be adequately exhibited in a Definition. We must go to the thing itself. This Socrates failed to do. And this the modern metaphysician also sometimes fails to do.[1]

CHAPTER VI

Plato

(427–347 B.C.)

§ 1. Investigator or Dogmatist?

From the age of twenty to the age of thirty, Plato was the devoted pupil and friend of Socrates. The discourses of such a master naturally gave the pupil's studies a definite direction, and determined the course of his after-life. From him Plato learned to understand himself, and thence to understand his predecessors and contemporaries. So completely, in fact, has Plato identified himself with his master that it is difficult to discover with any certainty the events and circumstances of his own life.[2]

We sometimes hear Plato described as the great Idealist. He was, however, anything but an Idealist, as that phrase is usually understood. He was a great dialectician; he was a severe and abstract thinker; his metaphysics were of the most subtle kind; and his morals and politics were hard and uncompromising to the last degree. Plato had learned to look upon human passion almost as a disease, and human pleasure as a frivolity: the only thing worth living for was the truth. His opposition to poets was deep and constant, for he saw in them an indifference to truth, and a

[1] Cf. Lewes, *Biog. Hist. of Phil.*, pp. 152-64.

[2] Cf. Maurice, *Moral and Metaph. Phil.*, vol. i, pp. 138-9; and *Diog. Laer.*, lib. III, c. i, s. 7.

preference for the arts of expression; poetry was therefore a dangerous rival to Philosophy.[1]

Plato is not to be measured, says Jowett, by the standard of any philosophical system. "He is the maker of ideas, satisfying the wants of his own age, providing the instruments of thought for future generations. He is no dreamer, but a great philosophical genius struggling with the unequal conditions of light and knowledge under which he is living. His truth may not be our truth, and nevertheless may have an extraordinary value and interest for us."[2]

There is a common spirit in the writings of Plato, but not a unity of design. The hypothesis of a general plan worked out is an after-thought of the critics, who have attributed a system to writings belonging to an age when system had not as yet taken possession of Philosophy.[3] It is, indeed, questionable whether Plato ever attempted to elaborate a consistent doctrine, and thus to cull passages here and there from his writings with the object of making up a doctrine must inevitably lead to error. Like Socrates, Plato occupied himself with method, rather than with results. Like Socrates, he devoted his life to the search for Truth.[4]

We saw in the last chapter that, as a teacher, Plato adopted the method of his master, and that he not only cast his writings in dialogue form, the better to suit the method, but invested the Dialogues with a peculiar authority by introducing into them Socrates himself. It is characteristic of the Dialogues that the notion of authority, instead of being invoked and worked up, as is generally done in Philosophy, is formally disavowed, and practically set aside. "I have not made up my mind; I give you the reasons for and against; you must decide for yourself." And again: "Why are you so curious to know what *I myself* have determined on the point? Here are the reasons *pro* and *con*; weigh the one against the other, and then judge for yourself."[5]

Some critics of antiquity regard Plato as essentially a Sceptic,[6] —that is, a Searcher or Investigator, not reaching any assured or proved result. They deny to him the character of a dogmatist; they maintain that he neither established nor enforced any affirmative doctrine. This latter statement is, however, carried too far. Plato is sceptical in some Dialogues, dogmatic in others, though

[1] Cf. Lewes, *Biog. Hist. of Phil.*, pp 185-92. [2] Preface to *Plato*. [3] *ib.*
[4] Cf. Lewes, *op. cit.* p. 201. [5] Cf. Grote, *Plato*, i, p. 239; and *Protag.*, 314 B.
[6] Not to be confused with the sceptic in the sense of an unbeliever.

the sceptical Dialogues (Dialogues of search or investigation) are more numerous than the dogmatic (Dialogues of exposition),—as they are also, speaking generally, more animated and interesting.[1]

To teachers, the truths *arrived at* by Plato are of much less value than the *mode* in which the truth is sought. As Mill says, although Plato continually starts most original and valuable ideas, it is seldom that these, when they relate to *results* of inquiry, are stated with an air of conviction, as if they amounted to fixed opinions. But when the topic under consideration is the proper *mode* of philosophizing, then the views inculcated are definite and consistent and always the same. The inference seems to be that, in regard to the investigation of Truth, Plato had not only satisfied himself that his precedessors were in error, and *how*; but had also adopted definite views of his own; while on all or most other subjects he contented himself with confuting the absurdities of others, pointing out the proper course for inquiry and the spirit in which it should be conducted, and throwing out a variety of ideas of his own.[2]

§ 2. Plato's Doctrine of Ideas: Elementary Notions

Plato was convinced of the fundamental necessity for an untiring investigation into the nature of general terms.[3]

Long before Plato's time, meditative men had perceived that knowledge derived from the senses can only be knowledge of appearances,—of phenomena. Now phenomena, Plato said, are, by their very nature, fleeting and transitory; they are not true existences, they are only *images* of true existences. We must therefore examine and classify them; discover what qualities they have in common; discover that which is invariable and necessary amidst all that is variable and contingent. Discover *the One in the Many*, and we have penetrated the secret of true existences.[4]

Everyone will admit that we can form some kind of conception[5] of a genus,—that we do think of and reason about "man" quite independently of Smith and Brown. If we have such a conception, whence did we derive it? Our experience has only been with the Smiths and Browns; we have known only *men*. Our senses tell us nothing of *man*. Individual objects give only individual knowledge. According to the Realists, our knowledge of *man* is derived from

[1] Grote, *Plato*, i, p. 212.
[2] Cf. Lewes, *op. cit.* p. 215
[3] Or, as the Schoolmen would say, abstract terms.
[4] Cf. Lewes, p. 207; and *Philebus*, 233-6.
[5] Cf. ch. ii, § 4.

a different source altogether, namely, from the mind. Now *man* is a general term, and general terms, Plato said, stand for the only real existences. The separate existences denoted by general terms he called *Ideas.*[1]

The Realists found the One in the Many,—in other words, found certain characteristics common to all men, and not only common to them but necessary in their being men; they abstracted these *general* characteristics from the *particular* accidents of the individual men, and out of these characteristics made what they called *Universals.* These Universals existed *per se.* They were more than conceptions of the mind; they were, so to speak, projected out of the mind and so became images or entities which could be looked upon; perceptions of them were formed in the same manner as perceptions of things. If, then, the conception of *genera* be thus rendered objective—transformed into perceptions of real existing things—we have, as before, Plato's *Ideas.*[2]

These Ideas Plato maintained to be the only things that had a real existence; they were the *noumena*[3] of which all individual things were the *phenomena.* The Platonic Idea is thus a kind of "Substantial Form".[4]

Practically, the whole doctrine is comprised in Plato's answer to Diogenes, who thought he demolished the theory of Ideas by exclaiming, "I see a table and a cup but I see no Idea of a table or a cup". Plato replied, "Because you see with your eyes and not with your Reason".[5]

Thus, according to Plato, the phenomena which constitute what we perceive by means of our senses are but the participations of matter in "Ideas". In other words, Ideas are the "Forms" of which material things are the copies; they are the *noumena*, of which all that we perceive are the Appearances (*phenomena*). But we must not suppose these copies to be exact; they not only do not participate in the nature of their models; they do not even represent them, otherwise than in a superficial manner.[6]

[1] This term Idea must not be confused with the term in the modern popular sense.
[2] Cf. Lewes, *op. cit.* pp. 209–11. [3] Cf. chap iii, § 2. [4] Cf. Lewes, p. 209.
[5] Cf. Lewes, p. 211; and Bosanquet, *Companion to Plato's Republic*, p. 16.
[6] Cf. Lewes, p. 211.

§ 3. Elusive and Unacceptable Aspects of the Doctrine

Walter Pater reminds us that the expression "theory of ideas" is due to Plato's commentators rather than to himself, and he considers that Platonism (as he calls it) is not so much a formal theory or body of theories as a tendency or group of tendencies,—a tendency to think or feel and to speak about certain things in a particular way. It is a fashion of regarding and speaking of all those terms or notions which represent under general forms the particular presentations of our individual experience; or to use Plato's own frequent expression, which reduces "the Many to the One".[1]

The actual relationship of these general terms and abstract notions to the individual, the unit, the particulars they include, is the great problem. Realism supposes the general name *animal*, for instance, to be not a mere name, as with the nominalists, nor a mere subjective thought, as with the conceptualists, but to be *res*, a thing-in-itself, independent of the particular instances which come into and pass out of it, as also of the particular mind which entertains it.[2]

A modernized view of the doctrine might be obtained, perhaps, by imagining, with Pater, the existence of a kind of permanent common sense, independent, indeed, of each one of us, but with which we are, each one of us, in communication. It is in this that those general ideas really reside. Abstract or common notions come to the individual mind through language, through general names, into which one's individual experience, little by little, drop by drop, conveys their full meaning or content; and by the instrumentality of such terms and notions, thus locating the particular in the general, and mediating between our individual experience and the common experience of our kind, we come to understand each other and assist each other's thoughts, as in a common mental atmosphere, an "intellectual world", as Plato calls it.[3]

Plato's Realism, however, seems, at times, to pass into such an extreme form that it becomes absolutely unacceptable. "From the simple and easily intelligible sort of Realism, seeking in ideas only a serviceable instrument for the distinguishing of what is essential from what is unessential in the actual things about him, Plato passes by successive stages, which we should try to keep distinct as we read him, to what may be rightly called a 'transcendental', what

[1] See Pater's *Plato*, p. 136. [2] *ib.* [3] *ib.*

to many minds has seemed a fantastic and unintelligible habit of thought; those abstractions, indeed, seem to become for him not merely substantial things-in-themselves, but little short of living persons, to be known as persons are made known to each other by a system of affinities, these persons constituting together that common intellectual world, a sort of divine family or hierarchy, with which the mind of the individual, so far as it is reasonable or really knows, is in communion or correspondence. And here certainly is a theory about which the difficulties are many."[1]

Plato's Ideas as transcendental existences have been compared to objects of contemplation in painting, music, poetry, sculpture, where the representations of art, framed apart by themselves, induce in the spectator that dream-image of reverie, which was previously the ideal-pattern of the artist himself. Plato, with his extraordinary powers of visualization, seemed to individualize his abstract concepts and to give them a fantastical embodiment.[2]

In its essence, the doctrine of ideas may be regarded as an attempt to solve a problem which in all ages has forced itself upon the notice of thoughtful men,—How can certain and permanent knowledge be possible for man, since all his knowledge must be derived from transient and fluctuating sensations? And the doctrine answers the question thus,—that certain and permanent knowledge is *not* derived from *Sensations* but from *Ideas*.[3] To what extent the doctrine is acceptable by other Philosophers, we shall see later.

§ 4. Plato's Method not really Scientific

The reader of Plato may at first fail to discover any evidence of a mastery of what we now call scientific method, but a careful search will reveal a very considerable insight into such method. Mill found the materials of his experimental methods in Herschel's Natural Philosophy, but at least the germs of these methods were created by Plato. For, sometimes, Plato's Idea is equivalent to what the man of science calls "Cause", "Explanation", "Law", and the like. But in addition to such Ideas as these—Causes, Explanations, or Laws, to be *discovered* by Science—Plato deals with "Ideas which are not to be discovered but are in our possession to start with", the native "Categories of the Mind", which are employed

[1] Cf. Pater, pp. 136-9. [2] *Classical Review*, Aug. 1910.
[3] Whewell's *Philosophy of the Inductive Sciences*, p. 12.

in the process of discovering the Ideas before mentioned. These native Categories, supposed to be apprehended by the Mind itself, are given as "Unity, Plurality, Identity, Difference, Similarity, Dissimilarity, Rest, Motion". It is by scrutinizing these in the data supplied by our senses that we discover the particular cause, or explanation, or law, belonging to the data, it being always assumed that the data are manifestations of real existences. "It is in much the same way as that suggested by Plato here that modern science employs its methods of Agreement, Difference, and Concomitant Variations, to explain the data of sense, to discover the Laws of Nature governing them."[1]

In one sense, then, the doctrine of Ideas may be regarded as a *method*,—the method of discovering *special Ideas* or Laws of Nature, discovered always by means of the application, to the phenomena presented, of certain *general Ideas*,—Unity, Plurality, Identity, Difference, Similarity, &c. But the important point is that experience is apprehended in a *scientific* way only when the Categories (Unity, Plurality, &c.) are made clearly explicit, as they are in what we now speak of as the method of Agreement, the method of Difference, &c. These formal realizations of his Categories Plato is evidently feeling his way to and Aristotle actually reached.[2]

But the statement that the Categories are "apprehended by the mind itself" opens up a very large question; the man of science is always suspicious of intuitive methods. The Science teacher must certainly not assume that the Categories are clearly "apprehended" by the minds of his pupils.[3]

Though Plato saw that scientific truths of great generality might be obtained, he overlooked the necessity of a *gradual* and *successive* advance from the less general to the more general. Whewell at first ascribed this to Plato's "dimness of vision", but afterwards acknowledged that the phrase was not very appropriate, since no acuteness of vision could have enabled Plato to see that gradual generalization in Science of which, as yet, no example had appeared.[4] But Plato did certainly fail to see the extent to which experience and observation are the basis of all our knowledge of the universe. The ascent from the particular to the general must be gradual, and each step upwards requires time and labour, and a patient examination of actual facts. But Plato, by a pure effort of

[1] See J. A. Stewart's *Plato's Doctrine of Ideas*, pp. 119–23; and Mill's *Logic*.
[2] Cf. Stewart, *ib.*; and Aristotle, *Topics*, ii, 10 and 11.
[3] See the chapter on Locke.
[4] Cf. Whewell's *Philosophy of Discovery*, p. 13.

thought, always seized upon the highest generality at once, and afterwards filled in all the intermediate steps between that and the special instances. This was his cardinal error.[1]

§ 5. Plato's Works.—(*a*) The Republic

Brief reference may now be made to such of Plato's works[2] as most nearly concern those who are interested in scientific method.

However great may have been Plato's speculative interest, it was his practical enthusiasm as a reformer that lay deepest within him. He imagined a form of society in which the ideal man might find himself at home, and so the *Republic* was constructed.

Although the *Republic* is rather long, and sometimes a little tedious, it will repay careful reading. But no part of it is likely to be of greater interest to teachers than Book VII, wherein is contained the famous Allegory of the Cave. Here Plato brings out clearly the false sense of reality which uncritical associations acquire for a mind which has never been led to feel their inconsistency; and he shows how strong is the reciprocal repulsion between the man who himself gets to the heart of things and the mob who are contented with traditional opinion and conventional views.[3]

§ 5. (*b*) The Timæus

The *Timæus* has been described as "an outbuilding of the great fabric of original Platonism". Of all the writings of Plato, it is the most obscure, and the obscurity is due to Plato's scanty knowledge of Physical Science. Physical Science at that time was in its infancy, yet Plato attempted to give a detailed account of the structure and nature of the universe.

"The time had not yet arrived", says Jowett, "for the slower and surer path of the modern inductive philosophy. Although the ancient philosophers no doubt often fell into strange and fanciful errors, it remains to be shown that they could have done more in their age and country, or that the contributions which they

[1] Whewell, *Phil. of Disc*, pp. 9–17.

[2] Jowett's translation is considered the best; and his introductions to the different Dialogues should be read again and again.

[3] Cf. Lewis Campbell on "Plato" in *Ency. Brit.*; Maurice, *Moral and Metaph. Phil.*, vol. i, p. 166; *Republic* by Davis and Vaughan, pp 235–8; Jowett's translation of and introduction to the *Republic*; Nettleship's *Lectures on Plato's Republic*, p. 261, &c.; Bosanquet's *Companions to Plato's Republic*, p. 263, &c.; also Grote's *Plato*.

made to the sciences with which they were acquainted are not as great upon the whole as those made by their successors."[1]

No doubt Plato intended to include in the *Timæus* such knowledge as he had then acquired concerning the various parts of the universe, and he gives us an extensive scheme of mathematical and physical doctrines. The Dialogue treats, in fact, not only concerning the laws of "harmonical sounds", of "visual appearances", and of the motions of planets and stars, but also concerning heat and light; water and ice; iron, rust, gold, gems, and other natural objects; concerning odours, tastes, hearing, sight, and the powers of the senses generally; and concerning practically all the obvious points of Physiology.[2] Now while those of the doctrines which depend upon geometrical and arithmetical relations are either portions or preludes of those branches of knowledge which have since assumed a mathematical form for the expression of truth, the opinions on such subjects as Physics, Chemistry, and Physiology will bear hardly a moment's examination. Plato's notions of Physiology are of the most fantastic kind. It seems almost incredible that such an explanation as that given, for example, of the causes of respiration could have emanated from such a mind as Plato's.

Here and there may be found elements of truth in his hypotheses, but these must be ascribed to his previous studies in other directions. His hypotheses as a whole involved such tremendous assumptions that the necessary conclusions which logically followed from them did not and could not possibly square with the facts. Instead of accumulating facts first and then framing hypotheses to cover the facts,—the method of modern investigation—Plato ignored the facts and started off with the hypotheses. Such a method could not but end in failure.[3]

§ 5. (*c*) The Theætetus

The *Theætetus* is an inquiry into the nature of knowledge, and the greater part of the Dialogue is devoted to setting up and throwing down definitions of Science and Knowledge.[4] It is remarkable, how, in this Dialogue, Plato holds the balance between experience,

[1] Jowett's Introduction to the *Timæus*.
[2] Cf. Whewell's *History of the Inductive Sciences*, vol. i, p. 349.
[3] Cf. Maurice, *Moral and Metaph. Phil*, vol. i, p. 175.
[4] Cf. Jowett's Introduction to *Theætetus*; and Whewell's *Hist. of the Induc. Sci.*, vol. i p. 349.

imagination, and reflection. He seems almost to have made "a compact with himself to abstain rigidly from snatching at the golden fruit that had so often eluded his grasp, and to content himself with laboriously cutting steps towards the summit that was still unscaled".

The part played by the mind and the part played by the senses in the acquisition of knowledge are minutely examined, but the light which Plato throws on the subject is indirect, and we only catch occasional glimpses of the probable truth. The theory that "knowledge is sense-perception" seems to be regarded as the antithesis of that which derives knowledge from the mind, but Socrates is designedly held back from giving us any final positive solution of the problem. The saying of Theætetus that "knowledge is sense-perception" is probably responsible for much of the misunderstanding of the empiricist position. The modern term "experience" while implying a point of departure in sense, and a return to sense, also includes all the processes of reasoning and imagination which have intervened. But the man of science is under no illusion that if he looks into the mind he will find there accurate records of perfect knowledge. We ought, in fact, to be on our guard against using the phrase "looking into the mind". It is a figure of speech, very misleading in character. It would be more correct to say looking "out of" the mind. Recognition of anything within us is of the most shadowy and fleeting character. We must not be misled by the terminology of a worn-out introspective Psychology.[1]

§ 5. (*d*) The Parmenides

In the *Parmenides*, Plato assails his own theory of Ideas, and does this so vigorously that many of his interpreters have considered the Dialogue to have been written by another hand. Whewell is amongst the number. The arguments advanced in the Dialogue are nearly, if not quite, those of Aristotle; they are the objections which naturally occur to a modern student of Philosophy. But Jowett is undoubtedly right when he says that the objection to Plato as the writer is really fanciful and rests on the

[1] See *Theætetus*, especially §§ 184–6, and Jowett's Introduction (Jowett, vol. iv). There is, of course, no doubt whatever about Plato's general views. Throughout his works he draws a strong distinction between those aspects of things which are sensibly perceived, and that which is not seen and heard but thought and understood, and he regards the latter as more trustworthy than the former. His whole tendency is to exalt Ideas above Facts, to find a Reality which is more real than phenomena, to take hold of a permanent truth which is more true than the truth of observation. (Cf. Whewell's *Phil. of Disc.*, p. 424.)

assumption that the doctrine of ideas was held by Plato throughout his life in the same form. The truth is that the Platonic ideas were in constant growth and transmutation.[1]

Grote is of the same opinion. It is true, as Grote says, that in the case of most philosophers we expect to find a preconceived system and a scheme of conclusions to which everything is made subservient. But Plato's search or debate has a greater importance in his eyes than the conclusion. He never hesitates to set forth what can be said against a given conclusion, even though not prepared to establish anything in its place.[2]

The discussion of Socrates and Parmenides forms one of the most remarkable passages in Plato. Few writers have ever been able to anticipate "the criticism of the morrow" on their favourite notions. But Plato may here be said to anticipate the judgment, not only of the morrow but of all after-ages, on the Platonic ideas.[3] Probably no one but Plato ever ventured on such an experiment —the experiment of showing that his own fundamental principle was practically untenable.[4] He deliberately sets forth everything that could possibly be said against his own conclusions, fully recognizing that reasoned truth never rests upon any better title than the superiority of the case made out on its behalf over the case that may be made out against it.

§ 6. How far can we follow Plato's Method?

Plato lived in an age when little was known of Science, and still less of the method of investigating it. He thus had to devise methods for himself, and so it came about that he underestimated the value of observation and experiment, though, temperamentally, perhaps, he was averse from the laborious work of accumulating details. When in the *Timæus* he constructed the Universe, he did not begin with facts and then construct a hypothesis; he began with a hypothesis; his Universe was the outcome of a pure effort of thought. Such a method could not be expected to produce anything but an absurd result.

Socrates is represented in *The Clouds*[5] as hoisted up in a basket gazing at the sky, and, no doubt, Plato is thinking of this when he speaks of the absurdity of supposing that star-gazing will reveal the

[1] Jowett's Introduction to *Parmenides*. [2] Grote's *Plato*. [3] Jowett, *ib.*
[4] See Maurice, *Moral and Metaph. Phil*, vol. i, p. 155.
[5] Aristophanes, *Clouds*, 171 *et seq.*

laws of the stars. Now suppose an untrained observer watches the course of the sun across the sky. What is the difference between the result of his observation and, for instance, that of Kepler? Assuredly the simple observation that the sun occupies different places in the sky at different times of the day is a true observation. But Kepler's interpretation of the observation is very different from that of the untrained observer, for he puts his observation with a large number of others previously made. The untrained observer sees the sun sweep daily across the sky from East to West, and constructs the hypothesis that the sun goes round the earth once in twenty-four hours. His hypothesis covers the facts, and is absolutely correct as the facts go. But Kepler's multitude of additional facts causes him to construct a totally different hypothesis, and the untrained observer's hypothesis is seen to be wrong. No one imagines that Socrates in his basket will ever get at the truths of astronomy by simple looking; but a certain type of metaphysician[1] denies the necessity of looking at all. True, the mind interprets; but the facts must first be gathered to be interpreted. The interpretation is then checked by new facts, and so the simpler generalization is gradually included in a generalization of a higher order.

But although Plato as a scientific investigator is not to be imitated, there are certain aspects of his method of investigation that no teacher who desires to work on scientific lines can afford to ignore.

In the first place, he insists, as we have already seen, upon the fundamental importance of understanding the exact significance of all the general and abstract terms we use. Such terms as animal and plant, wisdom and justice, suggest different notions to different minds, and the notions are almost always vague and indefinite. The important thing is to examine such terms, and to get clear and definite notions concerning them.

In the second place, Plato urges attention to the so-called "categories".—How we all assume that a child knows exactly what we mean by such a term as "like". But do *we* know precisely what it signifies? One of the axioms of Formal Logic is, "whatever is true of a thing is true of its like"; but what does

[1] We all know, for example, the young man who bubbles over with his first metaphysical enthusiasm, talking about analysis and synthesis to his father and mother and the neighbours, and hardly even sparing the dog. He despises facts and constructs his phantom systems out of nothing. Cf. Jowett's Introduction to *Philebus*; and Nettleship's *Lectures on the Republic*, pp. 270–6.

"like" mean? Are we supposed to refer to things which *appear* alike, or which really *are* alike?

Thirdly, Plato constantly warns us against seeing things from only one point of view, to be on our guard against superficial impressions, and never to accept an opinion merely because it happens to be held by many people in common. The evidence on which such opinions are based must always be carefully scrutinized.

Finally, Plato tells us never to fear being charged with inconsistency. He was a searcher after truth. He knew that his work was imperfect, and he never feared to make corrections when corrections were required. Additional experience often rendered necessary some modification of ideas already formulated, and Plato never hesitated to make the change. We English people are much too prone to "stick to our opinions", and to ignore inconvenient facts. Let Plato teach us the necessary lesson.[1]

CHAPTER VII

Aristotle

(About 384–322 B.C.)

§ 1. Aristotle's Wide Knowledge

Aristotle's father was a physician, and claimed descent from the *gens* of the Asclepiads or supposed descendants of Æsculapius; and no doubt the scientific tendencies of Aristotle's mind are in some measure to be attributed to early environment and family tradition. Amongst the Asclepiads there seems to have been a certain amount of training in observation and in dissection, imparted traditionally from father to son, thus serving as preparation for medical practice. Probably in this way Aristotle acquired that liking for physiological study which so many of his works indicate, but it is doubtful if he ever dissected the human subject, as Greek prejudices would hardly have tolerated such a course.

[1] Besides the *Republic*, *Timæus*, *Theætetus*, and *Parmenides*, the reader will find much to interest him in *Protagoras* and *Phædrus*.

The teacher should beware of introducing the Socratic method in the classroom. The great object of Socrates was to get his victims into a corner. The reasoning was always his; their business was merely to assent to the propositions which he formulated. His was the active mind; theirs were passive.

Aristotle was a pupil of Plato's, but probably owing to his marked opposition to many of Plato's views, he was not appointed head of the school when his master died.[1]

In the history of European thought, down to the period of the revival of letters, the name of Aristotle was supreme. Aristotle not only treated of almost every subject which came within the range of ancient knowledge, but also initiated many new branches of inquiry dependent on observation and induction. He not only represented in himself the culmination of Greek speculative philosophy, but was also, as far as possible, the forerunner of modern Science.[2]

It is characteristic of Aristotle that before laying down his own views on any subject, he examines with almost minute care the views of his predecessors, and it not infrequently happens that his own opinions seem rather brought out in his criticisms than dogmatically affirmed.[3] But the first thing that must strike anyone who examines his writings is the unparalleled extent of his knowledge. In all branches of Science then cultivated he was proficient. He wrote on Politics, giving the outline of no less than two hundred and fifty Constitutions. His treatise on Metaphysics would alone have made him famous. His Ethics, Rhetoric, and Logic are still held by many to be authoritative and unsurpassed.[4]

§ 2. His Rhetoric

Perhaps no work of Aristotle's is better known than his *Rhetoric*. Plato had regarded Rhetoric as the mere embodiment of the tricks of procedure used by the Sophists, and he refused to countenance the study of it. But Aristotle, "who often exhibits less moral earnestness but greater intellectual breadth than Plato", considered it necessary to cultivate all intellectual fields, this included. He had abundance of materials available, illustrating in the greatest variety of forms how speakers *had* been able to move their audiences, and from this mass of materials he decided to generalize. "Let us deduce rules," he said, "by applying which a speaker shall always

[1] See Galen, *De Anatom. Admin.*, ii, 1; and for early life see Grote's *Aristotle* and Maurice's *Moral and Metaph. Phil.*; also article "Aristotle" in *Ency. Brit.*

[2] Cf. *Ency. Brit.*, "Aristotle". [3] Cf. Lewes, *Biog. Hist. of Phil.*, p. 237.

[4] Cf. Lewes, *Aristotle*, p. 20. The catalogue of *Diog. Laer.* contains a list of 146 works, hardly one of which seems to correspond with any of the 40 works which are now ascribed to Aristotle and with which we are acquainted. But only about one-half of the latter number appear to be genuine works of Aristotle. There is no doubt that a very large number of his works are lost.

be able to persuade the reason or to move the feelings, and when we have got our rules we will construct a true art." There is no doubt that "Aristotle's principles of Rhetoric are the results of extensive original induction".[1] It was, clearly, Aristotle's intention to systematize the whole subject on a scientific plan.

§ 3. His Logical Treatises

Aristotle's Logical treatises were placed by his earliest known editor[2] at the commencement of the collected works, it being thought that these treatises, taken collectively, were not so much a part of Philosophy as an *Organon*[3] or instrument, the use of which must be acquired by the reader before he became competent to grasp or comprehend Philosophy; they formed an exposition of method rather than of doctrine.

Aristotle tells us that the theory of the syllogism was his own work altogether and from the beginning; that no one had ever attempted it before, that he therefore found no basis to work upon; and was thus obliged to elaborate, by long and laborious preparation, his own theory from the very rudiments.[4]

It was undoubtedly Aristotle who first told the world how, in deduction, the mind proceeds from some universal proposition. A process of deductive reasoning having been once thus recognized, it was obviously an advantage that the laws should be clearly ascertained, if only in order that any flaw in the process as prac-

[1] Edward Coppleston, *Reply* (Oxford, 1810). Aristotle's *Rhetoric* is worth reading for its own sake. In it are catalogued all the ordinary tricks of the platform speaker, which pass under the name of "Rhetorical devices". Aristotle openly advocates dishonest methods. See, for example, Bishop Welldon's translation, pp. 65, 68, 286. See also Archbishop Whately's Introduction to his own *Rhetoric*, and his excuse there given for writing such a volume.

[2] Andronicus.

[3] The *Organon* includes six different treatises: (1) "The Categories"; (2) "On Interpretation"; (3) "The Prior Analytics"; (4) "The Posterior Analytics"; (5) "The Topics"; (6) "On Sophistical Refutations". The first two deal with Propositions; the "Prior Analytics" and "Posterior Analytics" with the Syllogism and with Demonstration; the "Topics" give rules of debate, it being supposed that the object of the debates is not to prove truth or disprove falsehood, but to secure victory. The sixth treatise deals with Fallacies.

The second treatise ("On Interpretation") is the source of much of the matter of the elementary Logic of modern times, and the substance of the "Prior Analytics" has become the common property of all modern books on deductive Logic. Hardly anything has ever been added to or detracted from what Aristotle wrote about the Syllogism. "Both Kant and Hegel acknowledge that from the time of Aristotle to their own age, Logic had made no progress. The fourth figure was added to the Syllogism, uselessly; and Sir William Hamilton introduced his quantification of the predicate: *voilà tout.*"—See Grote's *Aristotle*, iii, iv, pp. 87, 141, &c.; Hamilton's *Logic*, vol. iii, pp. 87–91; also *Ency. Brit.* ("Aristotle"), and Stahr.

[4] Cf. with the *Rhetoric*. See *Sophis. Refut.*, pp. 183–4. Sir W. Hamilton points out that the Principles of Contradiction and Excluded Middle can be traced back to Plato (*Logic*, iii, pp. 87–91).

tised might be instantly detected. But no one would have repudiated more strongly than Aristotle himself that the formula of the syllogism can be used to test or explain anything beyond the process of reasoning from certain premisses possessed or assumed, and he is never tired of telling us that the only means of obtaining premisses is by experience and observation of facts. "When the facts of each branch of science or art are brought together, it will be the province of the logician to set out the demonstration in a manner clear and fit for use".[1]

Now those who attempt to keep Logic purely formal explain that when they speak of a piece of reasoning as being "formally valid", they mean that its validity is determined solely by its *form*, and is in no way dependent upon the particular subject-matter to which it relates. They regard the *process* of reasoning as something distinct from the *subject-matter* about which it is employed, and the errors of reasoning which they contemplate are those only which occur in the process so conceived.[2] But the mere *process* is a comparatively simple thing; it is the determination of the precise significance of the *subject-matter*,—the exact meaning of all our terms and of the statements containing them, the precise correspondence of what we say with what we mean, and the unquestioned truth of the assertions contained in our propositions: these are the real difficulties. These difficulties once overcome, Aristotle's rules for the subsequent process of reasoning are almost as perfect as they can be made. But these rules form little more than an elaborate piece of mechanism, so much so in fact that a modern logician[3] has actually constructed a logical machine by means of which it may be shown conclusively how entirely mechanical the *process* of deductive reasoning really is. But the value of formal logic is a subject which must be left for consideration in a future chapter.[4]

[1] *Analy. Prior.*, i, 30-3; and cf. Maurice, *Mor. and Met. Phil.*, vol. i, p. 189

[2] Sidgwick, *Use of Words in Reasoning*, p. 9.

[3] Jevons. See his *Principles of Science*.

[4] Aristotle regarded the Sophists as mere charlatans, and he collected, classified, and exposed their fallacies. So exhaustive is he that the human mind has hardly invented any fallacious argument since, which may not be brought under some head of the *Sophistical Refutations*. The reader should go through this treatise (which is not a large one) and then through the corresponding section in some modern textbook on Logic. Here is a sample from Aristotle's collection of fallacies, taken at random: "If the knowledge of a thing is good, it is a good thing to learn; the knowledge of evil is good, therefore evil is a good thing to learn; but evil is evil and a thing to learn; therefore it is an evil thing to learn".

§ 4. "Fact" and "Theory". Aristotle's Notion of Induction

The part of Logic which most closely concerns scientific investigation is Induction, and we must now consider Aristotle's views on Induction and on Scientific Method generally.

Aristotle taught that the very first essential of Science is to collect "facts": we must "first classify them, bring particular facts under general heads, and co-ordinate them into theories".

But what are "Facts"? And are we justified in drawing a sharp distinction between a Fact and an Idea, or between Fact and Theory? The distinction seems to fade away on examination. Facts are commonly understood to relate exclusively to the objective world,—to phenomena existing externally; Ideas, on the contrary, to consciousness,—to conceptions we form of external things. But we cannot consistently maintain such distinctions. So far from any fact being the unadulterated image of its object, the conditions of our consciousness are necessarily mingled with it. An analysis shows in the simplest fact an inextricable blending of *inference* with sensation. A fact has been defined as a bundle of inferences tied together by one or more sensations. Take a case so simple as that of an apple on the table.[1] All that is here directly certified by consciousness is the sensation of a coloured surface; with this are linked certain ideas of roundness, sweetness, and fragrance, which were once sensations and are now recalled by this of colour; and the whole group of actual and inferred sensations clusters into the fact which is expressed in "there is an apple". Yet any one of these inferences may be erroneous. The coloured object may be an imitation apple in wood or stone; the inference of roundness and solidity would then be correct, those of sweetness and fragrance erroneous: the statement of fact would be false.[2]

Clearly, then, the common distinction between Fact and Theory[3] is misleading. For some purposes, perhaps, it would suffice to say that Fact is the equivalent of Description of the order of phenomena, and Theory the explanation of that order. But on examination we see that a correct explanation is only a fuller description. As I sit in my chair I hear a noise; this is a "fact". But I proceed at once to draw a number of inferences,—to form a "theory": (1) that the

[1] Cf. ch. iii, § 2.

[2] Cf. Lewes, *Aristotle*, p. 72.

[3] In this chapter the term "theory" is used in its looser and more popular sense. See ch. xxi, on Hypotheses.

noise came from the next room; (2) that a picture has fallen down; (3) that a servant has dropped the picture. On proceeding to the next room I find that my inferences—my "theory"—are correct. But had I been in the room and seen the picture fall, the careless action of the servant would have become part of the "fact", part of the description; the completed fact contains the former theory. Of course my suggested theory or explanation might have proved incorrect; the falling of the picture might have been due, for instance, to the snapping of the supporting wire. Some day, Sir J. J. Thomson's interesting theory of the constitution of the atom may, conceivably, become a matter of actual observation, in which case what is now "theory" would become "fact".

We seem, then, to have a succession of links in the chain. With some of these we are acquainted by the operation of our "senses", with others only by the reason; and the boundary line between Theory and Fact is sometimes hardly distinguishable.[1]

Science having collected and classified its facts, has to find a hypothesis to bind the facts together. But until the hypothesis is verified it is only a guess, and may turn out to be an absurd error instead of a great truth. Deductions drawn from unverified hypotheses are necessarily always open to doubt. The great danger of accepting such deductions was entirely overlooked by Aristotle, whose blunders, in consequence, are often grotesque.[2]

Aristotle was intent on seeking *scientific explanations* of phenomena, though he lived at a time when such explanations were novelties; and to achieve this object he decided that the indispensable thing to do first was to collect facts, and to arrange them and classify them. From classification he proceeded, by *induction*, to generalizations, these being indispensable for furnishing the premisses necessary for deduction. In theory, then, he was wholly right.

But in practice he was wrong. He was wrong because he quite misunderstood the nature of the process of Induction. In the first place, he neglected to suspect phenomena, or to suppose that they need sifting and probing if the facts they really denote are to be known;[3] and in the next place he always assumed as *given*,[4] the ideas which entered into his propositions, whereas the most important feature in induction is the *introduction* of a new idea. That

[1] Cf. Lewes, *Aristotle*, pp. 72–6. [2] *ib.* pp. 114–5.
[3] Cf. Maurice, *Moral and Metaph. Phil.*, vol. i, p. 191.
[4] Aristotle's Induction is that of "Simple Enumeration". See ch. xv.

peculiar sagacity in some men which seizes upon the conceptions by which the facts may be bound together seems to be hardly recognized by Aristotle.[1] In short, Aristotle did little towards elucidating the actual methods by which the mind legitimately arrives at general facts or laws of nature.[2]

It may seem somewhat remarkable that, while he so frequently proclaims the necessity of careful induction, he did not acquire a completer mastery of the principles underlying the actual process, especially as he bestowed such elaborate care upon the analysis of the process of deduction and upon the rules for its use. It is true that he attempted to resolve induction into a peculiar variety of the syllogism, but the scheme is hopelessly unpractical. He really made one half of Logic to look like the whole, and this disproportionate treatment of the subject has been adopted and perpetuated by writers on formal logic almost to the present day. Many of our most important works on the subject treat Induction as if it were of no real consequence whatever.

We must, however, remember that, in Aristotle's day, little was known of Science, and far too little experience had then been accumulated from which effective generalizations could be made. For, after all, when a new hypothesis is conceived, the conception depends very largely upon the stores of previous experience already in the mind. Further, Aristotle was of a naturally impatient nature; no doubt, too, he was often unable to garner all the facts he felt to be necessary. At all events he jumped to conclusions. Eager, as most men are, to solve the problems which present themselves, he solved them *a priori*,—he applied his syllogism before he had ascertained the certainty of his premisses. Instead of fusing together accumulated facts by means of verifiable hypotheses patiently thought out, he satisfied himself with vague and hastily-formed generalizations; and he constantly allowed himself to be drawn away from considerations of objective fact to foolish speculations of a metaphysical character.[3] But at that time the verbal disputations of the Sophists had infected all learning, and it is probably due to this rather than to any inherent intellectual shortcomings in himself, that Aristotle was led too readily to accept vague and loose notions drawn from general and superficial observation, instead of seeking carefully, in well-arranged and thoroughly considered instances, for the true laws of nature.[4]

[1] Cf. Whewell, *Phil. of Disc.*, p. 20.
[2] Cf. *Ency. Brit.*, "Aristotle".
[3] Cf. Lewes, *Biog. Hist. of Phil.*, p. 240.
[4] Cf. Herschel, *Philosophy*, p. 109.

Yet Aristotle was so far consistent with his own doctrine of the derivation of knowledge from experience, that he made in almost every province of human thought a vast collection of special facts, and these collections are almost unrivalled even to the present day. In Political Economy, for instance, where almost everything depends upon observation and extended experience, it would be difficult to find in later times any writer by whom Aristotle has been surpassed or equalled.[1] And in his *Natural History* we have not only an immense and varied collection of facts and observations, but a sagacity and acuteness in classification which it is impossible not to admire. On the other hand, in those departments of knowledge when to the facts we must, in order to obtain truth, add the right inductive Idea, we find little of value in Aristotle's works.[2] The superlative excellence of the one side of his work forms an extraordinary contrast to the practical worthlessness of the other.

§ 5. Aristotle's Science.—(*a*) His Works

Aristotle's treatises on Science were numerous, and included a large number of researches into Physics and Biology. But it may be said at once that, from a modern point of view, most of the actual results are so meagre as to be almost unworthy of notice. Yet it seems a little unjust to treat him with such disparagement as Lewes did,—to go so far as to say that he was utterly false in method and puerile in his views of nature. Comparatively little could, of course, be expected from an investigator of 2000 years ago, in respect of experimental science, for, no matter how excellent his method, advance could be made but gradually and from one vantage-point to another. Aristotle's results would probably have been very different, could he have had at his disposal any modern scientific instrument. But Physical Science was then in its infancy, and he could start only where his predecessors left off.[3]

The principal Physical treatises of Aristotle are the eight books of Physical Lectures, the four books of the Heavens, the two books of Production and Destruction, the Meteorologics, and the Mechanical Problems. There are also numerous treatises on different branches of Natural History.[4]

[1] Cf. Poste's Introduction to *Post. Analy.* [2] Cf. Whewell, *Phil. of Disc.*, pp. 21-2.
[3] Cf. *Ency. Brit.*, "Aristotle". [4] Cf. Whewell, *Hist. of Induc. Sci.*, vol. I, p. 32.

§ 5. (*b*) Some General Notions

Aristotle shows a marked tendency to take his facts and generalizations as they are implied in the structure of language. He decides, for instance, that motion is impossible *in vacuo* by such arguments as this: "In a void there could be no difference of up and down; for, as in nothing there are no differences, so there are none in a privation or negation; but a void is merely a privation or negation of matter; therefore in a void bodies could not move up and down, which it is in their nature to do".[1] Clearly, facts are here entirely subordinated to mere words, and the reasoning is absurd.[2]

The widely accepted ancient doctrine of the Four Elements appears to have been founded on the opposition of the adjectives *hot* and *cold*, *wet* and *dry*. Aristotle puts the matter in a more systematic form than his predecessors: "We seek", he says, "the principles of sensible things, that is, of tangible bodies. We must take, therefore, not all the contrarieties of quality, but those only which have reference to the touch. Thus black and white, sweet and bitter, do not differ as tangible qualities, and therefore must be rejected from our consideration. The contrarieties of quality which refer to the touch are these: hot, cold; dry, wet.[3]

"Now in four things there are six combinations of two; but the combinations of two opposites, as hot and cold, must be rejected. We have, therefore, four elementary combinations, which agree with the four apparently elementary bodies. *Fire* is hot and dry; *air* is hot and wet (for steam is air); *water* is cold and wet; *earth* is cold and dry."[4] This disposition to assume that some common elementary quality must exist in the cases in which we habitually apply a common adjective, survived the Aristotelian philosophy for many centuries. As Whewell points out, even Bacon fell into the error.[5] Thus were great minds misled by mere words.

§ 5. (*c*) His Theory of Projectiles

Having argued that motion *in vacuo* is impossible, Aristotle proceeded to maintain that projectiles continue moving after the

[1] Aristotle, *Physic. Ausc.*, iv, 7, 215. [2] Cf. Whewell, *ib.* p. 34.

[3] Aristotle also includes heavy, light; hard, soft; unctuous, meagre; rough, smooth; dense, rare. But he rejects these: heavy and light because they are not active and passive qualities; the others because they are combinations of the four qualities accepted, which therefore he infers to be the four elementary qualities.

[4] Aristotle, *De Gen. et Corrup.*, ii, 2; cf. Whewell, i, p. 36. [5] Whewell, *ib.* p. 37

original motor ceases to be in contact with them, "either, as some say, by reaction, or by the motion of the moved air, which is more rapid than that of the natural tendency of the body to its proper place".

"*In vacuo*, on the contrary, there will be nothing of the kind; no body can have motion there unless it be carried and supported as in a chariot." How the chariot is to be moved *in vacuo*, Aristotle does not explain. Moreover, he adds, "no one can say why *in vacuo* a body once set in motion should ever stop; since why rather here than there? Consequently it must either remain in necessary rest, or,—if in motion,—in endless motion, unless some stronger interferes."

Aristotle had by no means overlooked the fact of the *resistance* of the air, since he compares it with the resistance of water. Yet the air is made to keep up rather than destroy the motion of a projectile. He had also, as we see, got a glimpse of inertia, at least as regards bodies *in vacuo*. But it never occurred to him to connect the two ideas and make inertia keep up the continuity of motion, and resistance of the air destroy the motion. He was forced to seek for some *continuous external motor* for continuous motion; "the pulses of the moved air" was the first cause which presented itself, and was immediately accepted. That such a hypothesis could have been seriously put forward seems almost incredible nowadays.[1]

§ 5. (*d*) His Account of the Rainbow

In Aristotle's account of the Rainbow, not only is his explanation valueless, but even his observation of facts, so common and so palpable, is inexact. He says: "The Rainbow is never more than a semicircle. And at sunset and sunrise, the circle is least, but the arch is greatest; when the sun is high, the circle is larger, but the arch is less." This as regards the circle is, of course, wrong, and it seems almost amazing that Aristotle should have failed to observe the constancy of the angular diameter[2] of the circle of which the arch of the Rainbow forms a part. "After the autumnal equinox," he adds, "it appears at every hour of the day; but in the summer season, it does not appear about noon." Whewell thinks it is curious Aristotle did not see the reason for this, but the fact that Aristotle failed to observe the constancy of the angular diameter of the Rainbow sufficiently explains why the reason, now

[1] See Aristotle, *Phys. Ausc.*, iv, 7, and viii, 10; and cf. Lewes, *Aristotle*, p. 133.
[2] Approximately 82°.

so familiar to us, did not occur to him. Aristotle's further remarks concerning the colours show a certain amount of careful observation: "Two rainbows at most appear, and, of these, each has three colours; but those in the outer bow are duller, aud their order opposite to those in the inner. For in the inner bow the first and largest arch is red; but, in the outer bow, the smallest arch is red, the nearest to the inner, and the others in order. The colours are red, green, and purple, such as painters cannot imitate."

In Aristotle's attempt to "explain" these phenomena, there is much loose speculation. "It is produced", he says, "by Reflection, from a cloud opposite to the sun, when the cloud forms into drops." And as a reason for the red colour he says that "a bright object seen through darkness appears red, as the flame through the smoke of a fire of green wood". This notion hardly deserves notice, though it was taken up again in our own times by the famous German poet, Goethe.[1] But absurd as the "explanation" now seems, we must remember that even if Aristotle's observations had been more complete, his facts would still have been too few to suggest a hypothesis very closely in accordance with the actual explanation as we now know it. But Aristotle hastily jumped to conclusions; his hypothesis was totally unwarranted from the facts at his disposal; and the result shows that the methods he practised were entirely at variance with the methods he advocated.[2]

§5. (*e*) An Example of Aristotle's Method of Reasoning

Aristotle's style of reasoning is unsparingly exposed by Galileo. We append a typical example. Aristotle's object is to prove "the immutability and incorruptibility of the heavens":—

1. Mutation is either generation or corruption.
2. Generation and corruption only happen between contraries.
3. The motions of contraries are contrary.
4. The celestial motions are circular.
5. Circular motions have no contraries.

   a. Because there can be but three simple motions.
      1. To a centre. 2. Round a centre. 3. From a centre.

   β. Of three things, one only can be contrary to one.

[1] Whose rather astonishing claims to be an authority on Optics, readers of his life by Lewes will remember.

[2] Cf. Aristotle, *Meteorolog.*, III, ii-iv; and Whewell, vol. i, pp. 346-7.

γ. But a motion to a centre is manifestly the contrary to a motion from a centre.

δ. Therefore a motion *round* a centre (i.e. a circular motion) remains without a contrary.

6. Therefore celestial motions have no contraries;
7. Therefore among celestial *things* there are no contraries;
8. Therefore the heavens are eternal, immutable, and incorruptible.

It is evident that this string of nonsense is the result partly of absurd hypotheses and partly of the use of the vague and ambiguous terms, generation, corruption, contrariety, &c., on which the changes are rung.[1]

§ 6. Aristotle's Blunders and Mistaken Notions

Aristotle made many blunders, some of them quite inexcusable. He describes, for instance, the human kidney as lobed like that of an ox; he speaks of the heart as having only three chambers; he says that the brain is bloodless, and that it does not extend to the back part of the skull. Such mistakes as these are almost innumerable.

His method of diagnosing scarlet fever is both curious and interesting: "If a woman suffering from scarlet fever looks at herself in a mirror, the surface of the mirror will become suffused with a blood-red mist, and this mist, if the mirror be quite new, cannot be rubbed off without difficulty". This was doubtless one of the old women's tales current in his day. It is almost inconceivable that he did not think of testing the truth of the statement. Instead of this he proceeded to "explain" it, and did so in various ways.[2]

We may quote one or two of his remarkable "problems":—

"What is the reason that lime is set on fire, and on a greater heat, by casting water on it?"—"Lime is hot of nature, and therefore when water is cast on it, it flies from the cold, and, by uniting of its force, gathers a greater heat and strength, and so is set on fire. And that is also the reason that a candle burns faster in the winter than in the summer; for then by reason of the encom-

[1] See Galileo, *Systema Cosmicum*, Dial. I, p. 30. (Herschel, *Phil.*, p. 110.)
[2] Aristotle, *De Insomniis*, ii, 459; cf. Lewes, *Aristotle*, p. 172.

passing cold, the heat unites itself and gathers the closer to the tallow or wax, and so consumes it the faster."

"What is the reason that, if you cover an egg over with salt, and let it lie in it for a few days, all the meat within it is consumed?"—"The great dryness of the salt consumes the substance of the egg; but in sand, some say, they may be kept long, as the mariners practise."

And here are two specimens of the hundreds that might be mentioned of his quaint ideas:

1. "The earth is composed of the noblest matter, which has three dimensions, for three is the most perfect number: Of it we say first, beginning, middle, end."

2. "A man bends when he rises, because a right angle is connected with equality and rest."[1]

§ 7. Aristotle's Method: Summary

The reader ought now to be able to form a fair conclusion as to the value of Aristotle's method. Bacon sums up the method thus: "He had made up his mind beforehand. He did not consult experience in order to make right propositions and axioms, but when he had settled his system to his will, he twisted experience round, and made her bend to his system."[2]

We must, however, give Aristotle full credit for the method he *taught*. Over and over again he urges us to accumulate facts first; over and over again he insists upon the necessity for careful observation and generalization as alone capable of furnishing correct ideas. Speaking, for example, of the parthenogenesis of bees, he says, "There are not facts enough to warrant a conclusion, and more dependence must be placed on facts than on reasonings, which must agree with fact."[3] Again, "Let us first understand the facts, and then we may speak for their causes".[4] Almost any number of such warnings might be quoted from his writings. No one was ever more keen to make "facts" the basis of his theories. But the art of experimenting, and the exact quantitative record of observations, had not been developed, and Aristotle was often quite destitute of the appropriate facts for a particular enquiry, and was sometimes even deceived in the "facts" on which he relied. His

[1] See Nichol's *Bacon*, p. 28. [2] *Nov. Organ.*, Aphor. 63; cf. Whewell, vol. i, p. 345.
[3] Aristotle, *De Gen. Anim.*, iii, 760; cf. Lewes, *Aristotle*, p. 111.
[4] Aristotle, *De Part.*, i, 163*a*; and cf. Lewes, *ib.*

training as a dialectician was a disadvantage to him as an investigator, for he was thus led to depend too much on the evidence of language, in forming his theories of nature.

As Lewes says, Aristotle was, in spite of himself, practically a metaphysician, assuming without misgiving the validity of all principles that were clear and logically consistent, no matter if they were merely verbal propositions, wholly without correspondence in fact. He argued from these principles, and only scrutinized the *logical dependence* of his deductions, instead of scrutinizing the principles themselves, and verifying his conclusions. Thus, from the assumption that the circle is the most perfect form, he deduced the conclusion that the motions of the planets must be circular. From the assumption that the centre is the "noblest place", he deduced the conclusion that the heart, being central, must be the seat of the noblest faculty, the soul.[1]

Lewes thus sums up Aristotle: "It is difficult to speak of Aristotle without exaggeration; he is felt to be so mighty and is known to be so wrong. History, surveying the whole scope of his pretensions, gazes on him with wonder. Science, challenging these separate pretensions, and testing their results, regards them with indifference. His intellect was piercing and comprehensive; his attainments surpassed those of every known philosopher; his influence has been exceeded only by the great founders of Religions. Nevertheless, if we now estimate the product of his labours in the discovery of positive truths, it appears insignificant, when not erroneous. None of the great germinal discoveries in Science are due to him."[2]

§ 8. Plato and Aristotle. Their Methods Compared

It has often been stated that Aristotle was not only ungrateful but actually antagonistic to Plato. But this is going too far. That in many things Aristotle differed fundamentally from Plato, there is, of course, no doubt; that his opposition to Plato was often resolute, there is equally no doubt. But, as Lewes remarks, it is not necessary to construe opposition as an offence. While it is true that Aristotle's criticisms and allusions to Plato are not always remarkable for judicial calmness, they never show any approach to irreverence, and certainly not contempt. Aristotle undoubtedly looked upon Plato as the very greatest of thinkers.[3]

[1] Lewes, *Aristotle*, p. 120. [2] Cf. Lewes, *ib.* p. 1. [3] *ib.* p. 11.

Plato's rich and varied contributions to Logic, Psychology, Metaphysics, Ethics, and Politics were so much scattered up and down in his works, and often so subtly and slightly indicated, that they required a process of codification. Aristotle, with the greatest gifts for the analysis and systematization of Philosophy that have ever been known, applied himself to the required task. He treated the Platonic dialogues as quarries out of which he got the materials wherewith to build up in consolidated form all the departments of thought and science, so far as they could at that time be conceived. It is true that he did the work rather ungraciously, seeming to dwell by preference on the difference of view between himself and Plato, but he probably did this unconsciously, apparently hardly perceiving how much the substance of his own thought, in all his non-physical researches, was derived from Plato. The attitude and aims of the two writers were, of course, different. Plato was a Dialectician; Aristotle was a man of Science.[1] Plato stood apart from induction and systematization; Aristotle's aim, almost from first to last, was to be scientific. Plato's dialogues are masterpieces of literary art; mere form, Aristotle entirely disregarded.[2] Plato held that the deceptions of sense justified scepticism of all sense knowledge; Aristotle, more correctly, taught that error did not arise from the senses being false media, but from the wrong interpretations we put on their testimony. Both agreed that Science is mainly concerned with "universals", but it was Aristotle alone who insisted that these could only be reached through experience.[3]

Until the time of Hegel, the general explanation of the fundamental difference between Plato and Aristotle was that Plato was an Idealist, Aristotle a Materialist; the one a Rationalist, the other an Empiricist; one trusting solely to Reason, the other solely to Experience. This explanation Hegel crushed by showing that although Aristotle laid much more stress upon Experience than did Plato, yet he also expressly taught that Reason was absolutely indispensable to form Science. It was Plato's Ideal theory to which Aristotle was so strongly opposed. Aristotle did not deny to Ideas a *subjective* existence; on the contrary, he made them the materials of Science; but he was completely opposed to their *objective* existence, and called them empty and poetical metaphors. He said that on the supposition of Ideas being Existences and models, there would be several models for the same thing, since the same thing

[1] Cf Sir Alex. Grant's *Ethics of Aristotle*, vol. i, p. 182.
[2] Cf. *Ency. Brit.*
[3] Cf. Davidson's *Aristotle*, p. 162; and *Ency. Brit.*

may be classed under several heads.[1] He saw clearly that Ideas are nothing but the productions of the Reason.

Mr. J. A. Stewart's sympathies are, as we should expect, wholly with Plato; but when he speaks of Plato as a "great man of science and a connoisseur of scientific method",[2] it seems impossible to agree with him. Lewes calls Plato's conception of method "disastrous", and regards him as one of the worst of investigators among men of great eminence.[3] Aristotle's method, though imperfect, was not utterly wrong, but wrong only in certain important particulars; in general direction it was right, for he did insist upon the preliminary accumulation of necessary facts. But his royal confidence in formal reasoning prevented due circumspection. He relied far too much on logical deduction, and accepted evidence without cross-examination. Aristotle's maxim, that "to know truly is to know the causes of things", is a doubtful guide in scientific research. We may *aspire* to know at last *why* things are, but we must be content for a long time with knowing *how* they are.

On the whole we must admit that while Aristotle had the truer notions of scientific method, Plato had the truer views of the nature of Science. Although Plato's notion of a real intelligible world, of which the visible world was a fleeting and changeable shadow, was absurdly extravagant, yet it led him to seek to determine the "forms of Intelligible Things", which are really the laws of visible phenomena. On the other hand, Aristotle was led to pass lightly over such laws, because they did not at once reveal the causes which produced the phenomena.[4]

To some extent we may regard the methods of Plato and Aristotle as mutually complementary. Plato disregards facts, but he does try to get to the heart of things. Aristotle insists on the facts, but then he constantly makes guesses which he does not trouble to verify and which are never substantiated. We can learn much from both philosophers, but we have still far to travel before we get any real insight into true scientific method.[5]

[1] Lewes, *Biog. Hist. Phil.*, p 238. [2] Stewart, *Plato's Doctrine of Ideas.* [3] Lewes, *ib.*
[4] Cf. Whewell's *Phil. of Disc.*, p. 29.
[5] For Aristotle's views on Education, the reader may consult Wilkins's *Nat Educ. in Greece*, Davidson's *Aristotle*, and Burnet's *Aristotle on Education* It will be seen that the problem as to whether the end of education is culture or whether it is to fit us for the business of life, is a problem over which the ancients were divided just as much as are the moderns.

CHAPTER VIII

Scholasticism

§ 1. The Period of Scholasticism

The Athenian Schools were closed by order of the Emperor Justinian in the year 529 A.D., a date which may be regarded as marking the termination of the period of Ancient Philosophy. After centuries of intellectual darkness, during which the settlement of the new races and their conversion to Christianity proceeded, and the foundations of the modern European order were being laid, the first symptoms of renewed intellectual activity appeared at about the same time as the consolidation of the Empire of the West in the hands of Charlemagne. That great monarch opened schools for the prosecution of philosophical studies, and from these Schools (*scholæ*) Scholasticism derives its name. For the most part the schools appear to have been established in connection with the abbeys and the monasteries; and as in those days the clergy were almost the only persons who had leisure or inclination for the studies provided, the schools were really ecclesiastical institutions from the beginning.[1]

Scholasticism, in the widest sense, extended from the eighth to the fifteenth centuries, but its fortunes during that period were various. It became really active in the eleventh and twelfth centuries, and reached its zenith in the thirteenth. As an active force it began to decay rapidly after about 1350.[2]

§ 2. Some Characteristics of Scholasticism

The characteristic of early Scholasticism was the absolute subordination of Philosophy to Theology. To quote Erigena: "There

[1] Cf Lewes, *Biog. Hist. Phil.*, p. 345; and *Ency. Brit.*, vol. xxi, p. 417.

[2] Cousin divides Scholasticism into three epochs. The first of these extends over the ninth and tenth centuries and includes the one great name of Erigena, who lived in the earlier half of the ninth century. The tenth century was a kind of intellectual interregnum. The second period covers the eleventh and twelfth centuries and includes the names of Roscellinus, Anselm, William of Champeaux, and Abelard. The third period extends over the thirteenth and fourteenth centuries; in the thirteenth century the names of Albertus Magnus, Thomas Aquinas, and Duns Scotus represent the culmination of scholastic thought; while William of Occam (died 1347) may be regarded as the representative of the last stage of Scholasticism. (Cf. *Ency. Brit.*, p. 417, &c.; and Lewes, p. 346. For Cousin's general views, see *Victor Cousin's Philosophy*, by Jules Simon.)

are not two studies, one of philosophy and the other of religion; true philosophy is true religion, and true religion is true philosophy".

It has been said[1] that there was no such thing as Philosophy in the Middle Ages; there were only Logic and Theology. But, after all, logical discussion commonly leads up to metaphysical problems, and this was pre-eminently the case with the Logic of the schoolmen. Yet the saying draws attention in a forcible way to the two great influences which shaped mediæval thought,—on the one side the traditions of Ancient Logic, on the other side the system of Christian Theology; and it is the attitude of the schoolmen towards these two influences that yields the general characteristics of the period. Their attitude throughout is that of interpreters rather than of those conducting an independent investigation. And though they are at the same time the acutest of critics, and offer the most ingenious developments of the original thesis, they never step outside the circle of the system they have inherited. They contemplate nature not at first hand but through the medium of Aristotelian logical formulæ. Their problems and solutions alike spring from Aristotle's dicta, for they felt the need of reconciling these with one another and with the conclusions of Theology.[2]

In the Middle Ages, Reason had not the free play which characterized its activity in Greece, and in the Philosophy of modern times: it was subject to authority. Its conclusions were predetermined, and the initiative of the individual thinker was therefore confined, in the treatment of his thesis, to the consideration of little more than mere formal details. From the side of the Church this characteristic of the period is expressed in the saying that "Reason has its proper station as the handmaid of Faith" (*ancilla fidei*). Intellectual activity was confined to methodizing and demonstrating the truths of dogmas founded by the Church. No mediæval philosopher thought of questioning the truth of a religious dogma, even when he found it philosophically false and indemonstrable.[3]

§ 3. Aristotle Followed, not Plato.

Surprise has sometimes been felt that Aristotle rather than Plato should have swayed the minds of the Middle Ages. But it has to be borne in mind that Plato was then almost unknown,[4] and even had

[1] By Prantl. (See *Ency. Brit.*)

[2] Cf. *Ency. Brit.*, vol. xxi.

[3] Cf. Veitch, *Descartes*, p. xvi.

[4] It was not until about the end of the twelfth century that even the whole of the *Organon* of Aristotle was available. The introduction of Aristotle's works at this time was due to the Arabians.

he been known he would almost inevitably have been rejected. No doubt, the Philosophy of Plato was, at bottom, more in accordance with the doctrine of the Church, but the form was so original, so independent, and so provocative of liberty of thought, that it would have been adjudged inadmissible. On the other hand, the Philosophy of Aristotle had perfected the only thing about which men then dared to occupy themselves, namely, *form*. Scholastic Philosophy, such as it was, was a *form* of Theology; and that which tended to perfect the form perfected Theology.[1]

Aristotle was the great authority on all matters of reasoning, as the Bible was the great authority on all matters of Faith. For a long time Reason and Faith marched side by side, and "the constant effort of Scholasticism to be at once philosophy and theology" seemed at last satisfactorily realized.[2] But as time went on, doctrine after doctrine was withdrawn from the possibility of rational proof, and relegated to the sphere of faith; and so at last it came about that Scholasticism failed in its great task of rationalizing the doctrines of the Church. Logic and Theology refused to be reconciled. "The Aristotelian form refused to fit a matter for which it was never intended; the matter of Christian theology refused to be forced into an alien form." The ultimate result was that, although at the outset of Scholasticism Philosophy was absolutely subordinated to Theology, and although later on the two agreed to walk hand in hand, Philosophy began to feel an imperative need for independence. Complete and final separation was then only a question of time.[3]

It must always be borne in mind that Scholasticism had formed a body of thought remarkable for its order and symmetry, well-knit and squared, solid and massive, and capable for centuries of defending itself from all attacks. But it was formed for conservation and defence, not for progress, and the day was therefore bound to come when new forces, making for progress, would inevitably prevail against it.[4] Yet it would be unjust to look upon Scholasticism as philosophically barren, and to speak as if Reason,[5] after hibernating for a thousand years, woke up again at the Renaissance. In spite of their initial acceptance of authority the scholastics cannot be regarded as the antagonists of Reason; on the contrary, they fought many battles on its behalf. It has often been well pointed out that

[1] Cf. Lewes, *Biog. Hist. Phil.*, p. 347 [2] Cf. Milman, *Latin Christianity*, ix, 101.
[3] Cf. Lewes, *ib.* pp. 346-8; and *Ency. Brit.*, p. 418.
[4] Cf. Veitch, *Descartes*, p. xviii.
[5] The exercise of "reason" must of course be distinguished from the reasoning of formal logic.

the attempt to establish *by argument* the authority of Faith is, in reality, the unconscious establishment of the authority of Reason; and Reason, if admitted at all, must ultimately be admitted altogether. On the other hand, the successive results of Scholasticism are not the free products of speculation; they are modified versions of Aristotle. Each new result is, however, a fresh recognition of the rights of Reason, and Scholasticism as a whole may be justly regarded as the history of the growth and gradual emancipation of Reason, an emancipation which was completed in the movements of the Renaissance and the Reformation.[1]

§ 4. The Last Phase of Scholasticism

With the fifteenth century an epoch commences which may be regarded as one of transition from Scholasticism to Modern Philosophy. Scholasticism was now beginning to be identified with obscurantism. The taking of Constantinople and the revival of ancient letters hastened materially the development of the human mind; the works of Plato became known and were enthusiastically studied. The different nations and languages of modern Europe began to assert their individuality; the sudden contact with new men, new lands, and new faiths, awakened the slumbering intelligence of Europeans and roused their curiosity; and men's interests ceased to be predominantly ecclesiastical. Scholasticism, therefore, which was in its essence ecclesiastical, began to die of inanition. The sixteenth century brought Luther and the Reformation, the result of which was to place the Bible in the hands of the people, just as the revival of letters had placed the writings of the Greek philosophers in the hands of the students. Authority, already feeble, was quickly thrown to the ground, and Philosophy transferred its allegiance from the Church to antiquity.[2] Erasmus, in his *Praise of Folly*, pours out his exultation over the old world of ignorance and bigotry, now beginning to vanish away before light and knowledge. Folly in cap and bells mounts a pulpit and pelts with her satire the absurdities of the world around her, the superstition of the monk, the pedantry of the grammarians, and the dogmatism of the doctors of the schools.[3]

Science also began to make advances. Galileo in 1609 invented

[1] Cf. *Ency. Brit.*, "Scholasticism".

[2] Cf. Lewes, *op. cit.*; and *Ency. Brit.*, *ib.* p. 430.

[3] Cf. Green's History, "The New Learning"; and Erasmus, *Praise of Folly* (the interesting edition with Holbein's drawings can sometimes be obtained for a few shillings).

the telescope, which enabled him to discover the satellites of Jupiter. Kepler was engaged in those discoveries which have immortalized him. Gilbert published his speculations on the magnet. Algebra was developed,—in fact, Mathematics generally was sedulously cultivated, and had already been applied to Astronomy, Mechanics, and Physics, thus effectually ruining the authority of both Aristotle and the Schoolmen. Elements were at work which made the age ripe for the appearance of Bacon and Descartes. Had Bacon or Descartes appeared earlier, "their influence would have been comparatively trifling; but the age was ready for them, the age wanted them, and the age adopted them. The special want of the age was a *method*, and these men furnished it."[1]

Bacon and Descartes threw off the trammels of their time and opened a new era. Both are often called philosophers, but it seems preferable to regard Bacon primarily as a man of science and Descartes as a metaphysician. Bacon is sometimes called the Father of Experimental Philosophy, just as Descartes is called the Father of Metaphysical Philosophy. The titles are apt, for Bacon's philosophical instrument is Induction; that of Descartes, Deduction.

But although these two famous men did separate themselves from the reigning dogmas of the day and did open new paths of inquiry, in which they travelled far beyond their contemporaries, we must not suppose them unindebted to their contemporaries. They were the creatures no less than the creators of their epoch. They founded new schools, but they founded them on the ruins and out of the materials around them.[2]

[1] Cf. Lewes, *op. cit* p. 349.

[2] Cf. Lewes, p. 340. "It was at this period that Physics and Metaphysics first stood up openly against each other; consequently it is now that the ambiguous nature of the term Philosophy becomes most apparent. When Physics was jumbled with Metaphysics there was no impropriety in designating all men's speculations by the name of Philosophy. But when the separation took place, men were anxious to indicate that separation even in their language. And so it is that whenever a History of Philosophy is spoken of, a History of Metaphysics is almost invariably meant. (Lewes, *Biog. Hist. Phil.*, p. 344.)

Perhaps the most intelligible and satisfactory idea of the method, objects, and results of Scholasticism is to be gained from the analysis of Abélard's works, which fills a volume and a quarter of Rémusat's *Abélard*, Paris, 1845. See also Victor Cousin, *Hist. de la Phil.*, ii.

CHAPTER IX

Bacon

(1561–1626)

§ 1. Bacon's Independence of Mind

We have seen that one of the distinguishing characteristics of Scholasticism had been the attempt to acquire a knowledge of Natural Philosophy by mere thinking and arguing, without coming into contact with the contradictions, or corrections, or verifications of experience; but men were now beginning to see the necessity for pursuing their inquiries into nature by careful observation, and there were already successful workers in Italy, in Germany, and in England. It was, however, Bacon who first systematized the new method, and proclaimed it from the housetops as the key to the secret of interpreting nature.[1]

It is scarcely likely that anyone will, nowadays, accept Macaulay's estimate of Bacon as a man; but should there still be any lurking suspicion that there is some discordance between the character and the intellect of Bacon, it would be well to bear in mind, not only that character and intellect are never necessarily related, but also that there is a complete explanation of Bacon's general acceptance of conventional standards. This explanation is to be found in Bacon's absolute interest in knowledge and his want of interest in man. In this respect Bacon and Shakespeare are as the poles asunder. To Bacon, nature was the supremely absorbing fact, just as man was to Shakespeare. And it is this that gives us the key to Bacon's life.

Of Bacon's independence as a thinker we have evidence as far back as his undergraduate days, when he spoke of the "unfruitfulness" of Aristotle. And in his paper "On Controversies of the Church", which he wrote at the age of twenty-eight, his attitude is entirely that of a disinterested observer. He had been brought up in a Puritan household of the straitest sect, and he got to see the inside of Puritanism, its best as well as its worst side; he saw its learning, its labour, and its hatred of wrong, and he saw its aggressiveness, its intolerance, and the personal ambition of its leaders. But in the paper just mentioned, it is easily seen that

[1] Cf. Nichol's *Bacon*, vol. ii, pp. 10–11.

Bacon had ceased to feel as a Puritan,—he was too tolerant and too neutral.[1] His attitude was that of an impartial judge. He had already become imbued with the true scientific spirit.

Bacon not only refused to accept the authority of ecclesiasticism, but he also treated with disdain the results of the labours of the Greek philosophers. He was quite ready to admit that "things are not what they seem"; but, apart from the inscrutable truths of religion, he had no faith in anything that was not physical. With metaphysical modes of thought he had no sympathy at all.[2] "When it comes to the questions which have attracted the keenest thinkers," says Dean Church, "the question, what it is that thinks and wills, what is the origin and guarantee of the faculties by which men know anything at all and form rational and true conceptions about nature and themselves, whence it is that reason draws its powers and materials and rules, what is the meaning of words which all use but few can explain—Time and Space and Being and Cause—Bacon is content with a loose and superficial treatment of them. Bacon was certainly not a metaphysician, nor an exact and lucid reasoner. The subtlety, the intuition, the penetration, the severe precision, and even the force of imagination which make a man a great thinker on any abstract subject were not his."[3] But this criticism is unreasonable and unjust, though it is part of the price Bacon has still to pay because his attitude towards Metaphysics was that of a disinterested scepticism. Bacon knew full well that the acutest intellects of 2000 years had tried to solve the problems of Metaphysics, and had tried in vain. Why, then, should he waste time in trying to solve the insolvable? As Fowler says, a deep sense of the unprofitable character of metaphysical speculations has been a characteristic not only of the Baconian Philosophy in particular but of British Philosophy in general, which, with a healthy instinct, has usually either avoided them altogether, or discussed them solely with the view of showing that they lie outside the limits of human knowledge.[4] Bacon assumed the ordinary distinction of mind and matter,—a universe of objects to be known, and a thinking subject capable of attaining to a knowledge of them; and he set himself the task of solving those problems which he felt really were within the range of human powers.

[1] Cf. Church's *Bacon*, pp. 1-14.
[2] Cf. Nichol, vol. ii, pp. 98-101.
[3] Church, pp. 204-5.
[4] Fowler, *Nov. Org.*, p. 15.

§ 2. Bacon's Method: General Notions

Hitherto, the mode of demonstration had been by the syllogism, but "the syllogism is, in many respects, an incompetent weapon, since it is compelled to accept its first principles on trust". Bacon was convinced that a radical change of method by which Science was pursued was indispensable, and the boldness and definiteness of his views of the change that was requisite are truly remarkable.[1] A cardinal principle of Bacon's method was the necessity not only of proceeding from experience but of proceeding cautiously and gradually, and this is the essential difference from the mediæval and ancient methods. The ancient method certainly did begin with facts of observation, but rushed at once, with no gradations, to the most general principles; and the same course had been followed by all those speculative reformers who had talked so loudly of the necessity of beginning Philosophy from experience. Whenever any of these men had attempted to frame a physical doctrine, they had caught up a few facts of observation and had erected a universal theory upon the suggestions which these offered. They anticipated instead of interpreting nature.[2]

Bacon called men, as with the voice of a herald,[3] to lay themselves alongside of Nature, to study her ways, and imitate her processes. To use his homely simile, he rang the bell which called the other wits together. He insisted on the importance of experiment, as well as on observation. He insisted on the necessity of collecting facts. He urged that authority must be disregarded. The office of Reason, he said, ought not to be limited to an examination of the conclusions and their dependence on the premisses; we must insist on examining the premisses themselves. And he insisted on the importance of a gradual ascent from propositions of a lower to those of a higher degree of generality.[4]

Bacon also urged the subordination of scientific enquiries to practical aims, to the increase of man's comforts, and to the general convenience of life. This attitude has been severely criticized; but when we recollect the frivolous character of many of the questions which men of the most brilliant abilities were then in the habit of disputing, and the profound misery in which the mass of man-

[1] Cf. Whewell, *Phil. of Disc.*, p. 370.
[2] Cf. *Nov. Org.*, Aph. 20, 22; and Whewell, *Phil. of Disc.*, p. 133.
[3] Cf. *De Aug.*, iv, 2.
[4] Cf. Fowler, *Nov. Org.*, pp. 85, 126.

kind, then even more than now, were sunk, we can hardly feel surprise or regret that a great statesman and great philosopher should have suggested the application of man's intellectual gifts to the improvement of his material condition.[1]

§ 3. His Philosophical Works

It is very difficult to give an account of Bacon's philosophical writings at once clear and sufficient. This arises, first, from the fact that his work is very incomplete. Of the three main contributions to his scheme, the *De Augmentis* (practically an expansion of the *Advancement of Learning*) alone is finished. The *Novum Organum* is only a fragment; and the *Sylva Sylvarum* is a mass of disjointed though interesting observations.[2] The second obstacle to a satisfactory analysis is Bacon's habit of repeating himself. It seems preferable, therefore, instead of considering the separate works, to consider Bacon's *method* as developed in the works as a whole.

§ 4. The Four Classes of "Idols"

Preliminary to the method itself are the discussions of the First Book of the *Novum Organum*, and the most important of these concern our common intellectual vices and tricks of self-deception, the *Idols*, as Bacon, in his figurative language, calls them. The word *Idolon* or *Idolum* is manifestly borrowed from Plato. It is used twice by Bacon in connection with the Platonic Ideas. The εἴδωλον of Plato is the fleeting transient image of the real thing.[3] "Idola" is usually translated *Idols*, but it would be more correct to speak of "phantoms of the mind", "false notions", or "false appearances". Bacon never refers to the common meaning of the word, namely, the image of a false god. Idols are with him *placita quaedam inania*. The doctrine of Idols stands, he says, in the same relation to the interpretation of Nature as the doctrine of fallacies to ordinary Logic.[4]

[1] Fowler, *Nov. Org*, p. 127. See also Whewell, *Phil*, pp. 142-3.

[2] There is also a volume of posthumously published discourses and discussions, for the most part forecasts of the *Organum*. Its precise place in the author's scheme is often hard to determine.

[3] Bacon evidently refers to the passage in the *Republic*, vii, 516 A. Cf. ch. vi, § 5. See also *Nov Org.*, 23, 124; and *Ency. Brit*, "Bacon".

[4] Cf. Ellis, Pref. to *Nov. Org.*, p. 223; Hallam, *Hist. of Europe*, iii, 194-6; Lewes, *Biog. Hist. Phil.*, p. 364; Nichol, *Bacon*, ii, p. 153.

Bacon divides the Idols into four classes:—

1. *Idola Tribus*, i.e. Idols of the Tribe.
2. *Idola Specus*, i.e. Idols of the Den.
3. *Idola Fori*, i.e. Idols of the Market Place.
4. *Idola Theatri*, i.e. Idols of the Theatre.

"The *Idola Tribus* are inherent in human nature and the very tribe or race of man." "The mind is not like a plane mirror, which reflects the images of things exactly as they are; it is like the mirror of an uneven surface, which combines its own figure with the figures of the objects it represents."[1]

Among the Idols of this class is the propensity which there is in all men to find a greater degree of order, simplicity, and regularity than is actually indicated by observation, and to be diverted from the truth by an unconscious love of uniformity. Thus, as soon as men perceived the orbits of the planets to return into themselves, they immediately supposed them to be perfect circles, and the motion in those circles to be uniform; and to these hypotheses the astronomers and mathematicians of antiquity laboured to reconcile their observations. Then, again, most men are warped by the strength of first impressions, and, having adopted opinions, hold them tenaciously; or they look only to affirmatives and not to negatives. "It was a good answer made by one who, on being shown in a temple the votive tablets suspended by such as had escaped from shipwreck, and being pressed as to whether he would now recognize the powers of the gods, asked, 'But where are the portraits of those who have perished in spite of their vows?'"[2]

The *Idola Specus* are those which spring from the peculiar character of the individual. Besides the causes of error common to all mankind, each individual has his own dark cavern or den, into which the light is imperfectly admitted, and in the obscurity of which a tutelary idol lurks, at whose shrine the truth is often sacrificed.

These Idols of the Den take their rise in peculiarities of mental or bodily structure, in education, habit, or accident. Among them are professional zeal, the narrow devotion of men to certain studies, either because they have bestowed much thought on them, or, as it were, have lived all their lives in the midst of them. Some love the old, others the new. "Profound understandings are dis-

[1] *Nov. Org.*, i, 41.

[2] Cf. *Nov. Org.*, i, Aph. 46-52; Nichol, *Bacon*, ii, p. 154; Lewes, *op. cit.* p. 365; and *Ency. Brit.*, p. 212.

posed to attend carefully, to proceed slowly, and to examine the most minute differences; while those that are abnormally active are ready to lay hold of the slightest resemblances." Each of these easily runs into excess, the one by catching continually at distinctions, the other at resemblances. "Let every student of nature take this as a rule, that whatever the mind seizes and dwells upon with particular satisfaction is to be held in suspicion."[1]

The *Idola Fori* are those which arise out of intercourse with society, and those also which arise from language. Bacon calls these delusions of the "Market Place" on account of the consort of men there. Men believe that their thoughts govern their words, but it often happens that "words, like the arrows from a Tartar bow, are shot back and react upon the mind". "Words being commonly framed and applied according to the capacity of the vulgar, follow those lines of division which are most obvious to the vulgar understanding."[2]

The *Idola Theatri* are the false notions which have arisen from the dogmas of different philosophers. They are called Idols of the "Theatre" because all the received systems are "but so many stage-plays representing worlds of their own creation after an unreal and scenic fashion". They do not enter the mind imperceptibly like the other three; a man must labour to acquire them. Examples of false systems are "those which, from a few and random experiments, leap at once to general conclusions"; or "those which corrupt philosophy by poetical and theological notions"; or those like Aristotle's, "which substitute formulæ for the investigation of nature".[3]

It is evident that the Idols—"the spectres of the mind"—may either act together or separately in the same person and in reference to the same thing. If I say, "the sun moves round the earth", because my eyes tell me so, it is an Idol of the Tribe; if because common language says so, it is an Idol of the Market Place; if because Ptolemy says so, it is an Idol of the Theatre; if because that view agrees with other theories of my own, as was the case with Bacon himself, it is an Idol of the Den.[4]

[1] *Nov. Org.*, Aph. 42, 54–8; Lewes, p. 365; Nichol, ii, p. 155; *Ency. Brit.*, p. 212.
[2] *Nov. Org.*, Aph. 43, 54, 60; Lewes, p. 365; Nichol, ii, p. 156; Spedding, p. 224; Fowler, p. 229.
[3] *Nov. Org.*, Aph. 44, 61, &c.; Lewes, pp. 365–6; Nichol, ii, p. 157. Cf. also Bacon, "*Redargutio*" and "*Cogitata et Visa*".
[4] Cf. Nichol, ii, pp. 157–8.

§ 5. Bacon's Method.—(*a*) Collection of Facts

These preliminary discussions completed, Bacon proceeds in the second book of the *Organum* to describe and exemplify the nature of Induction.

The indispensable preliminary to Induction is the observation and collection of facts. "Man the servant and interpreter of Nature, can do and understand so much, and only so much, as he has observed in fact or in thought of the course of nature; beyond this he neither knows anything nor can do anything."[1] "Our first object must therefore be to prepare a 'history' of all the phenomena to be explained." The history is to include both observations and experiments; "it ought to be composed with great care; the facts accurately related and distinctly arranged; their authenticity diligently examined; those that rest on doubtful evidence, though not rejected, yet noted as uncertain, with the grounds of the judgment so formed. The last is very necessary, for facts often appear incredible only because we are ill-informed, and cease to appear marvellous when our knowledge is further extended."[2]

Bacon dwells upon the importance of this part of his scheme,[3] declaring that, without such a register or "history" of the facts of nature, nothing can be done, even "if all the wits of all the ages shall meet in a world university". "Let such a history be once provided, and the investigation of nature and of all sciences will be the work of a few years." But Bacon completely underestimated the magnitude of such a task. No one acquainted with the history of Natural Philosophy would now think it possible to form a collection of all the "facts" which are to be the materials of any "science", antecedently to the formation of the "science" itself. "Fact" and "theory" cannot be separated in this way.[4] Yet Bacon thought it possible so to sever observation from theory that the process of collecting facts, and that of deriving consequences from them, might be carried on independently and by different persons. His opinion seemed to be that the connection between fact and theory was merely an external one; and that the facts, being comparatively few, might be observed and recorded within a moderate length of time by persons of ordinary intelligence. Now it is true that when the laws of nature have been caught sight of, much may be done by ordinary observers

[1] *Nov. Org.*, Aph. 1.
[2] Cf. Lewes, *op. cit.* p. 366.
[3] See his Essay, the *Parasceve.*
[4] Cf. Nichol, ii, p. 163; and Kuno Fischer *Bacon*, pp. 96–7.

in verifying and exactly determining them; but, as Whewell points out, when a real discovery is to be made, this separation of the observer and the theorist is not possible, the questioning temper and the busy suggestive mind being needed at every step to direct the operating hand or the observing eye.[1] Mere observers cannot supersede the discoverer who is to introduce into the facts a new principle of order, though it is of course true that persons of moderate powers may, when properly trained, make observations which may be used by greater discoverers than themselves.

§ 5. (*b*) Discovery of "Forms"

Bacon's next step is to discover, by a comparison of the different facts, the "Form" of the phenomena under investigation. But he seems to find great difficulty in giving an adequate and exact definition of what he means by a Form, though, as a general description, the following passage is pretty clear. "The Form of any nature is such that, given the Form, the nature infallibly follows. Again, the Form is such that, if it be taken away, the nature infallibly vanishes. Lastly, the true Form is such that it deduces the given nature from some essence inherent in many natures."[2] From this it would appear that since by a *nature* is meant some sensible quality, superinduced upon, or possessed by, a body, so by a Form we are to understand the *cause* of that nature, which cause is itself a manifestation of some quality inherent in a greater number of objects.

Lewes explains the Forms thus: "The Form of any quality in a body is something convertible with that quality; that is, where it exists, the quality exists. Thus, if transparency in bodies be the thing inquired after, the *Form* of it is something found wherever there is transparency. Thus Form differs from *Cause* in this only: we call it *Form* or Essence when the effect is a permanent quality; we call it *Cause* when the effect is a change or event."[3]

This is not very illuminating, and it is really difficult to understand precisely what Bacon intended his Forms to signify. It certainly does not mean the outward shape, which is a mere matter of sight and touch. Nor can it be the Platonic *Idea*, or any abstraction separable from concrete realities. Nor is it a Law of nature, as now

[1] See Bacon, *Phenomena Universi*; and cf. Ellis, p. 35. See also Whewell's criticism of Spedding's views in *Phil. of Disc.*, pp. 154–5.

[2] *Nov. Org.*, ii, 4. Cf. *Ency. Brit.*, p. 213.

[3] Lewes, *op. cit.* p. 366. Cf. Bacon, *Valerius Terminus*, ch. xi.

understood.[1] Fowler says that at one time he thought Bacon attached to the term two entirely distinct meanings, which may be represented roughly by *cause* and *essence*, but later he concluded that it had various shades of meaning in different places, all of these admitting of derivation from a single conception.[2] Ellis was disposed to believe that the doctrine of Forms is in some sort an extraneous part of Bacon's system. Certainly the second part of the *Novum Organum* is rendered more or less vague and obscure by the employment of the term, instead of the more precise expressions, such as Law, Cause, Conditions, &c., by which it is now replaced.[3]

One thing is certain, and that is that Bacon not only had a firm grasp of particular physical properties, but was able to form a clear conception of *generalized* physical properties; and we may perhaps look upon his Forms as physical properties of a highly generalized character. "Though Bacon uses the word *cause*, and even identifies Form with cause, it is evident that, to him, effects were manifestations and not consequents. Notions of cause as dynamical were foreign to him; in his view, nature had a purely statical aspect."[4]

§ 5. (*c*) The "True Difference"

The next point to be considered is the particular method, employed by Bacon, of comparing the collected facts of any given phenomena and of discovering the Form or Cause concealed among them. We are told to ascend from the experience of facts to the experience of causes:[5] and so we come to Bacon's theory of Induction.

In our examination of the collected data, we must, by some means or other, discover and set aside whatever is non-essential and contingent. Clearly, if this be done, the residue will consist of what is really essential to the phenomena under investigation. To this residue of data thus left over and constituting the essential conditions of the phenomena, Bacon applies the term "true difference",[6] which he further designates as "the fountain of things",—the Form of the phenomena. If, then, we are to arrive at the

[1] Cf. Nichol, ii, p. 184; and Maurice, *Moral and Metaph. Phil.*, ii, p. 223.
[2] Fowler, p. 53. [3] *ib.* p. 131.
[4] Cf. *Ency. Brit.*, p. 213. For some account of Bacon's "simple natures", cf. *Nov. Org.*, ii, 5; Ellis, p. 16, and the illustration concerning the natures of gold in the *Sylva Sylvarum*; also *De Aug.*, iii.
[5] Cf., for instance, "*Recte ponitur: vere scire esse per causas scire*", &c.
[6] '*Differentia vera.*"

Natural Law underlying the phenomena—to discover the Form of the phenomena—the problem to be solved is *the discovery and elimination of the non-essential.*

Bacon was of opinion that the discovery of the Form or Cause thus concealed among the facts presented to sense, could be made with absolute certainty and with mechanical ease. The form of induction hitherto used by logicians he regarded as useless, since it was a mere catalogue of a few known facts, made no use of exclusions or rejections, and so was always liable to be overthrown by a negative instance. Now this last point Bacon looked upon as of fundamental importance, and therefore directed special attentior to it in his own Method.[1]

§ 5. (*d*) The Tables of Investigation

In order that the necessary preliminary classification of facts might be systematic and complete, Bacon suggested three "Tables of Investigation".

1. *The Table of Affirmatives.*—This is to contain a collection of all the known instances that agree in having the same quality. If, for example, the subject to be enquired into is heat, we should include in our Table the sun, lightning, flame, burning-glasses, the blood of mammals, hot-iron, &c.[2] We are advised, in forming the Table, to collect instances from all quarters, and from varied and dissimilar objects.[3] But any conclusion arrived at from an inspection of this Table will be a guess; for Bacon curiously remarks, only God and the angels can tell the cause from the contemplation of the affirmatives.[4] We are bound to make use of—

2. *The Table of Negatives,*—"a collection of examples of bodies otherwise similar (else the list would be endless), which do not agree in the same nature". Thus, the negative Table of Heat would contain such instances as the moon's rays, blood of fish, dead animals, &c.

The stress Bacon lays on negative instances is one of the earliest applications to Philosophy of the principle "*Audiatur et altera pars*". He constantly urges that the cardinal defect of the old induction was the neglect of this; that "our conclusions can never be legitimate or secure till they have passed through the sieve of this table and have no more to apprehend from an unforeseen exception".[5]

[1] Cf. *Nov. Org.*, i, 69. [2] See Bacon's investigation into "Heat", *Nov. Org.*, ii.
[3] Cf. Mill's "First Canon of Induction" (see ch. xvii). [4] Nichol, vol. ii, p. 165.
[5] Cf. Nichol, ii, p. 166; and Mill's "Joint Method" (see ch. xvii).

3. *The Table of Comparison*,—i.e. "a collection of instances where the phenomenon sought to be explained is present in various degrees". Thus "heat is unequal in various kinds of flame, rising in degree from that of burning spirits of wine to that of a blast-furnace; it varies in the same animals under different circumstances"; and so on.[1]

This form of Table often throws suggestive light on the relation of antecedents and consequents, but its efficacy largely depends on the skilful use of experiment, in which Bacon, while recognizing its importance, was in practice unskilled.

§ 5. (*e*) The Process of Exclusion

After the formation of these Tables we are to apply what is perhaps the most valuable part of Bacon's method, and that which the author regarded as the corner-stone of his system,—*the process of exclusion or rejection.*[2] Suppose, for instance, we check the first Table by means of the second, we may be able to correct, and perhaps have to reject, a generalization already provisionally made from the first Table alone. Thus when it appears that the blood of terrestrial animals is hot and that of fish cold, the hasty conclusion that the blood of all animals is hot is rejected. Then, again, if we are trying to discover the cause of transparency in bodies, we should, from the fact that the diamond is transparent, immediately exclude rarity and fluidity from possible causes, the diamond being a very solid and dense body.

This *elimination of the non-essential* is the special feature wherein Bacon's method differs from that of previous philosophers. It is evident that if the Tables were complete, and our notions of the respective phenomena clear, the process of exclusion would be a mere mechanical *counting out*, and would infallibly lead to the detection of the Cause or Form. But it is evident that these conditions can never be adequately fulfilled. Bacon saw this, and therefore set to work to devise new "helps".[3]

§ 5. (*f*) Other "Helps". The "First Vintage"

There is, naturally, great difference in the value of facts. Some of them show the thing sought for in the highest degree, some in

[1] Cf. Mill's "Method of Concomitant Variations" (see ch. xvii); and see Nichol, ii, p. 166.
[2] See *Nov. Org.*, i, 69, 105; ii, 15, 16, 19 [3] Cf. *Ency. Brit.*, "Bacon", p. 216.

the lowest; some exhibit it "simple and uncombined", in others "it happens confused in a variety of circumstances". Bacon's scheme of "*Prerogative Instances*"[1] was the outcome of his consideration of this comparative value of facts. He enumerates twenty-seven different species, but few of these add much to our knowledge of his Method, though one is well worth mentioning, viz. the *Instantia Crucis*.

When, in any investigation, the understanding is placed *in equilibrio*, as it were, between two or more possible causes, each of which seems to account equally well for the phenomena, nothing remains to be done but to look out for a fact which can be explained by one of these causes and not by the other. Such facts perform the office of a *cross*, erected at the junction of two or more roads, to direct the traveller which road to take. They are therefore called *crucial* instances.

The *Experimentum Crucis* is of such importance in inductive investigation that, in all those branches of Science where it cannot be resorted to, there is often great want of conclusive evidence.[2]

Of the other "helps" enumerated by Bacon we have but scattered hints. And although the rigorous requirements of Science could only be fulfilled by the employment of all these means, yet, in their absence, it was permissible to draw from the various Tables a hypothetical conclusion, the truth of which might be verified by the use of the other processes. Such a hypothesis Bacon quaintly called the "First Vintage".[3]

§ 6. Bacon's Investigation into Heat

Bacon's inductive method, so far as exhibited in the *Organum*, is exemplified by an investigation into the nature of Heat.[4] He throws into a Table of Exclusions everything about Heat which is not present in the Table of Affirmative Instances, or which is present in the Table of Negative Instances, everything which increases when the phenomenon decreases, and vice versa. From the possible causes of Heat he is now able to throw aside Light, Fluidity, and Quiescence, and at last arrives at the hypothetical conclusion that the essential nature of Heat is *motion*. Flame is perpetually in motion; so are hot or boiling fluids. Heat is increased by motion, as in bellows or blasts; all bodies are destroyed or have the

[1] Cf. *Nov. Ory*, ii, 21.
[2] Playfair. See Lewes, pp. 368–9.
[3] "*Vindemiatio*". See *Ency. Brit.*, p. 216.
[4] *Nov. Ory*, ii.

position of their parts altered by Heat; when it escapes, as in death, the body rests. Motion is therefore clearly the genus of heat.—This conclusion is not, of course, very far wrong, though it is defective in detail and obtained by a very imperfect process.[1] But the conclusion is only the "first vintage", beyond which, as a matter of fact, Bacon was never able, in any of his investigations, to advance. He had worked up to Mill's Canon of the Method of Residues, but he failed properly to apply it. Instead of proceeding with further testing experiments, he was hurried on by the very impatience, misled by the same love of uniformity, which in his predecessors he denounced.[2]

§ 7. The Method a Failure in Practice

It is not correct to say that Bacon ignored the deductive side of reasoning altogether, for whenever he saw its value for the purpose of applying the truths already arrived at by induction, he seems to have assigned it an almost co-ordinate rank.[3] But he did reject deductive reasoning in his process for establishing general principles. He scoffed at the old scholastic notion that it was possible to establish by *a priori* methods the first principles of any 'science",[4] and then to deduce by syllogism all the propositions which that science could contain.[5] In this he was, of course, abundantly justified, though sometimes he went rather too far in assigning to deductive reasoning such a strictly subordinate function. "We reject the syllogistic method", he says, "as being too confused and allowing nature to escape out of our hands. In everything relating to the nature of things we make use of induction for both our major and minor propositions."[6] Bacon felt, as we now all feel, that in the Middle Ages an absurd importance had been attached to the

[1] A careful examination of this investigation will show, as Ellis points out, that Bacon's provisional conclusion is not really the result of the method of Exclusion, but rests immediately on the three Tables of Investigation Hence it does not pretend to be the result of formal proof, but only a sort of probable hypothesis, based upon the consideration and comparison of a large number of instances. Whewell's opinion of the investigation is that it is a complete failure. Yet the essential part of the conclusion is that Heat is an expansive motion amongst the minute particles of bodies, so that, in any case, Bacon did divine the true nature of Heat. But, after all, the conclusion was more the result of a lucky guess than of good method, and the investigation which occupies so much of the second book of the *Novum Organum* does not afford us much insight into a method that is really practicable.—Cf. Appendix to Tyndall's *Heat*, and Aph 20 of the *Nov. Org.*; also Whewell's *Phil. of Disc.*, pp. 136–8; Ellis, vol. i, pp. 36–7; Fowler, pp. 36–43.

[2] Cf. Nichol, vol. ii, pp. 168–9.

[3] Cf. Fowler, p. 130; *Nov. Org.*, i, 93, 106, ii, 21; and Rémusat, p. 224

[4] The objection to the term "sciences" has already been mentioned.

[5] Cf. Ellis, p. 38. [6] Cf. Bacon's *Preface*; Devey, p. 12.

syllogism, but apparently he did not quite realize the important part that deductive reasoning still had to play in scientific investigation.

Bacon was not the first to tell men that they must collect knowledge from observation, from experience, but he had no rival in his peculiar office of teaching them *how* knowledge must thus be gathered. With great clearness, he insists on "*a graduated and successive induction*", as opposed to a hasty transit from particular facts to the highest generalizations.[1] As Whewell points out, it is a truly remarkable circumstance to find this recommendation of a continuous advance from observation, by limited steps, through successive gradations of generality, given at a time when speculative men in general had only just begun to perceive that they must begin from experience in some way or other. There is no vagueness in Bacon's assertion of this important truth. He repeats it over and over again, and illustrates it by a great number of emphatic expressions. Thus he speaks of the successive "floors" (*tabulata*) of induction; and he makes use of a further happy simile when he speaks of each "science" as a *pyramid*[2] which has observation and experience for its base, with the lower generalizations gradually converging to the highest generalization at the apex.[3]

Bacon made a great advance on Aristotle, in that, instead of being satisfied with a "simple enumeration" of facts, he insisted on an assemblage and codification of sifted and tested facts; but his method of induction is a long way behind that of Mill, Whewell, Herschel, Faraday, and Darwin, and does not form a suitable instrument for the successful investigation of the laws of nature. Bacon thought his method as certain in its results as a demonstration in Euclid; and so mechanical that, when once understood, all men might employ it.[4] But in practice it is entirely unworkable.

Bacon underestimated the part played by mind in the constitution of knowledge. "Our method of discovering the sciences is one which leaves not much to acumen and strength of wit, but nearly levels all wits and intellects."[5] While it is true that Bacon does not entirely neglect to consider the formation of scientific conceptions,[6] yet he gives us no single hint as to the manner in which induction is to be employed for this particular purpose, and by this circumstance alone our knowledge of his method is rendered im-

[1] *Nov Org.*, i, 19. [2] *Aug. Sci.*, iii, 4, 194; and *Nov. Org.*, i, 104.
[3] See Whewell's *Phil. of Disc.*, pp. 132, 144
[4] *Nov. Org.*, ii, 1, 5; Nichol, ii, pp. 181, 231. [5] *Nov. Org.*, i, 61.
[6] Cf. *Nov. Org.*, i, 15; ii, 19.

perfect and unsatisfactory. And perhaps Bacon never, even in idea, completed his method thus far.

Now, although in the process of scientific discovery the formation of conceptions is the part with respect to which it is practically impossible to lay down general rules, yet its importance in practice is so great, that Bacon's avoidance of the difficulty has reduced the value of his method almost to vanishing point. In fact, not only has the method never produced any appreciable result, but the process by which many scientific truths have been established cannot, as a rule, be so presented as even to appear to be in accordance with it. The process always involves an element to which nothing corresponds in Bacon's Tables of Comparison and Exclusion, namely, the application to the facts of observation of an *idea*,—call it a scientific conception, a hypothesis, a general principle, or what we will,—from the mind of the discoverer. The *finding of an appropriate idea*—the formation of a hypothesis explanatory of the collected facts—is the very essence of the inductive act. If, for instance, we consider Kepler's discovery that Mars moves in an ellipse, we see that the core of the difficulty lay in bringing into connection with the facts of observation *the idea of motion in an ellipse*. The hypothesis once found, it is *verified* by a further appeal to facts, a point we shall have to consider in detail in a future chapter.

Bacon failed because he thought it possible to make the process of induction mechanical; because he did not recognize that Science must progress by the application of ideas to facts; and because nature is practically infinite and not reducible to a mere "alphabet".[1] "Bacon inherited the mental diseases of those he imagined himself to have slain." "In the act of arraigning Aristotle, he is nowhere more Aristotelian than when he speaks of dense and rare, light and heavy, as if they were absolute qualities, instead of terms as relative as up and down, broad and narrow."[2] Bacon had little experimental skill, and his facts were drawn more from books than from nature, and he was therefore able only to suggest, not to realize. Then he set before himself an unattainable goal. He set out to become master of all nature. His audacity in this respect contrasts with the modest aims of more practically successful men of science, such as Leonardo da Vinci, Copernicus, Galileo, and Newton, who owed their triumphs in large measure to self-restraint.[3] As Bacon underrated the vastness and subtlety of nature, so he overrated his own appliances to bring it under his command. Cowley compared him

[1] Cf. Nichol, ii, p. 171. [2] *ib.* p. 195. [3] *ib.* pp. 171, 195, 196, 227.

to Moses on Pisgah surveying the promised land; it was but a distant survey, and Newton was the Joshua who began to take possession of it.[1]

Although Bacon's method excites our admiration historically, it excites no admiration for its present intrinsic value. We have a much more perfect method now, the processes of scientific investigation being far better understood. But we are never in communion with his vast and penetrating intellect without acknowledging his greatness, for much of his teaching is as applicable now as when first written.[2]

As De Morgan says, Bacon was eminently the philosopher of *error prevented*, rather than of *progress facilitated*.[3]

§ 8. Bacon's Errors and Oversights in Science

It is often said that Bacon was very imperfectly acquainted with the Science of his own day. Spedding gives numerous details of his errors and oversights, and some of these are worthy of mention.

Bacon paid great attention to Astronomy, but he appears to have been quite ignorant of the discoveries that had just been made by Kepler's calculations. Though he complained in 1623 of the want of compendious methods for facilitating arithmetical computations, he does not say a word about Napier's logarithms, which had been published only nine years before. He complained that hardly any advance had been made in Geometry beyond Euclid, without taking any notice of what had been done by Archimedes and Apollonius. He saw the importance of determining accurately the specific gravities of different substances, and attempted to form a table of them by a rude process of his own, without knowing of the more scientific though still imperfect methods previously employed by Archimedes and others. He speaks of the "principle" of Archimedes in a manner which implies that he did not understand the nature of the problem to be solved. In reviewing the progress of Mechanics, he makes no mention either of Archimedes, or of Stevinus, or of Galileo. He observes that a ball of one pound weight will fall nearly as fast through the air as a ball of two, without alluding to the theory of the acceleration of falling bodies, which had been made known by Galileo more than thirty years before.

[1] Church, p. 179.

[2] Cf. Whewell, *Phil.*, p. 151; and Lewes, p. 377.

[3] *Budget of Paradoxes*, p. 50.

He makes no allusion to the theory of equilibrium. He proposes an inquiry with regard to the lever, though the theory of the lever was as well understood in his own time as it is now. He speaks of the poles of the earth as fixed, in a manner which seems to imply that he was not acquainted with the precession of the equinoxes; and, in another place, of the North pole being above, and the South pole below, as a reason why, in our hemisphere, the north winds predominate over the south.[1]

Then Bacon believed, with qualifications, not only in Natural, but in Judicial, Astrology. He believed that air and water, under certain conditions, were mutually convertible. He makes no mention of the circulation of the blood, which Harvey began to teach in 1616. He believed in the existence of bodies of positive levity, and held that air has no weight. He gave his countenance to many of the most absurd fancies of the time, as that an ape's heart, "applied to the neck or head, helpeth the wit".[2] He speaks of Astronomy as being degraded by Mathematics. He says that "wood and metal are not equally cold".[3] He lays it down that the phosphorescence sometimes seen in the sea is due to its being struck violently by the oar, or agitated by storms.[4]

We thus see how over-confident was Bacon's temper, and how little he did in his own practice to rectify the fallacious methods of which he so eloquently complained.[5] The fact is, he never divested himself of a prejudice in favour of the simplicity of nature, and this disposed him to exaggerate the facility of its analysis.[6]

§ 9. His Rejection of the Copernican Theory

Perhaps the most important and at first sight least excusable of Bacon's scientific errors was his persistent rejection of the Copernican theory.[7] It seems strange that such a great reformer of Science should have steadily refused to admit the greatest reform in scientific conceptions which had been proposed for many generations, and which had already been before the world for eighty years. But it cannot be said that, till the laws of formal astronomy were connected by Newton with the physical laws of matter and motion, the motions of the earth or its relations to the rest of the solar system could in

[1] See Ellis, vol. i, pp. 572, 578, 625, 631, notes, &c.; also Fowler, pp 23-5.
[2] See *Nov. Org.*, ii, 40, 48, 111, &c.
[3] See *De Aug.*, iii, 4; and *Nov. Org*, ii, 13.
[4] *De Aug.*, iv, 3; and *Nov. Org.*, ii, 12
[5] Robertson, pp. 80-2.
[6] Cf. Abbott, p. 408.
[7] i.e. the heliocentric theory.

any way be regarded as placed beyond the range of dispute. The following sentences from De Morgan read us a useful lesson in estimating the scientific judgments of men of past ages. "By investing Copernicus with a system which requires Galileo, Kepler, and Newton to explain it, and their pupils to understand it, the modern astronomer refers the want of immediate acceptance of that system to ignorance, prejudice, and over-adherence to antiquity. No doubt all these things can be traced; but the ignorance was of a kind which belonged equally to the partisans and to the opponents, and which fairly imposed on the propounder of the system the onus of meeting arguments which, in the period we speak of, he did not and could not meet. It must be remembered that, in the sixteenth century, the wit of man could not imagine how, if the earth moved, a stone thrown directly upwards would fall down upon the spot it was thrown from. Easy experiments verify the law of motion which now explains this; but, to be proved by experiment, a law must be conceived and imagined. To be put under discussion it must be proposed. Now the advocates of the earth's motion never, before the time of Galileo, even conceived this law, never proposed it, and of course never proved it."[1]

It is true that Bacon once spoke of "these carmen who drive the earth about",[2] but he was then a young man, and Hume goes too far in saying that Bacon rejected the Copernican system "with positive disdain". No doubt in early life Bacon conceived, like the majority of his scientific contemporaries, a strong prejudice against the theory, and equally no doubt, as he got older, the reasons against the theory appeared to him more and more decisive. But it would be unreasonable to think that Bacon did not weigh the evidence, for and against the theory. To parallel cases of the rejection[3] of a proposed theory there is scarcely any limit. The Cambridge mathematicians, for instance, adhered to the Cartesian system long after the publication of Newton's discoveries; and Leibnitz obstinately declined to accept the Newtonian doctrine of gravitation. Many theories now universally accepted were at first supported only by the slenderest evidence, and assuredly cautious men showed their wisdom by suspending judgment. Consider the many scientific theories floating in the air at the present time. Assuredly some, perhaps most, will have to be rejected, while evidence will

[1] *Companion to British Almanac* for 1855, pp. 21-2.

[2] Cf. *Praise of Knowledge* and *Temporis Partus Masculus*. (See Ellis and Spedding, i, p. 124, and iii, 536; Fowler, p. 34.)

[3] It would perhaps be fairer to say "suspension of judgment".

gradually accumulate to lead to the general adoption and final acceptance of others.[1]

Bacon is sometimes regarded as a dilettante in Science, but he never pretended to have any claims to the distinction of being a great discoverer. His main business was with the *logic* of Science. Yet the wealth of illustration exhibited in the *Novum Organum* and the vast number of subjects reviewed in the *De Augmentis* show a range of knowledge probably quite unequalled by any other man then living. He was not a Specialist, but he threw out many suggestions of rare sagacity. He suggested the necessity of a closer union between formal and physical astronomy.[2] He recognized the possible influence of the moon on spring and neap tides.[3] He conjectured that light requires time for its transmission.[4] He anticipated some of the optical investigations of Newton.[5] His experiment with the hollow globe of lead to determine the question of the compressibility or incompressibility of water, preceded by nearly fifty years the celebrated Florentine experiment of a similar nature. Humboldt complimented Bacon on having considered the direction of the winds in connection with temperature and aqueous phenomena.[6] And he seems even to have had a glimpse of the fallacy of the doctrine of the fixity of species.[7]

It is of course an error to think that Bacon took no interest at all in experimental work. In fact, as Macaulay points out, he was destined to be the martyr of experimental Science. It had occurred to him that snow might be used with advantage for the purpose of preventing animal substances from putrefying; and on a very cold day early in 1626 he alighted from his coach near Highgate to try the experiment. He went into a cottage, bought a fowl, and with his own hands stuffed it with snow. While thus engaged he caught a chill and, after a week's illness, died.[8]

§ 10. Bacon's Critics

Like all great men, Bacon has been subjected to much criticism, and, of his frankly hostile critics, Macaulay is perhaps the most

[1] Cf. Fowler, p. 36.
[2] See *Descr. Glob. Intell.*, v. (Ellis and Spedding, vol iii, 734; Fowler, p. 37.)
[3] *Nov. Org.*, ii, 45-8. [4] *ib.* 46. [5] *ib* 22.
[6] *ib.* 45, 50; and *Hist. Dens. Rar.* (Ellis and Spedding, ii, 299, 300; Fowler, p. 46); Humboldt, *Kosmos*, ii, 322, 379. [7] *Nov. Org.*, i, 66.
[8] Macaulay, *Essay on Bacon.* For an excellent example of Bacon's experimental work, see *Nov. Org.*, ii, 40.

unfair. Macaulay almost leads us away by his ingenuity and plausibility. But his puerile arguments about mince-pies and about Jacobinism, as illustrations of the futility of induction,—"the inductive method has been practised ever since the beginning of the world by every human being",—reveal his total incompetence to examine Bacon's method. That all men in all ages have practised "induction" is true; but Macaulay confounds induction as an "art of life", with the inductive method as a scientific process. The induction as practised by ordinary people is that of *simple enumeration*, and is fundamentally different from that of the scientific investigator. Impartial readers of Macaulay's Essay will be forced to the conclusion that the author's "monstrous absurdity" is to be found in his own argument rather than in the inductive process which he tries to ridicule.[1]

Mill's criticism is searching, but fair. "Some have preferred to assert that all rules of induction are useless, rather than suppose that Bacon's rules are grounded upon an insufficient analysis of the inductive process. Such, however, will be seen to be the fact as soon as it is ascertained that Bacon entirely overlooked plurality of causes. All his rules tacitly imply the assumption, so contrary to all we know of nature, that a phenomenon cannot have more than one cause."[2]

The *Novum Organum* appears to have received, at first, a wider recognition on the Continent than in England. Descartes, writing to Mersenne, said, "You desire to know how best to make experience useful; on this point I have nothing to add to Verulam". The opponent of Descartes, Gassendi, yet praises him for his points of resemblance to Bacon. Puffendorf, the Jurist, declared that it was Bacon "who raised the standard and urged on the march of discovery". As time went on Bacon's work began to receive due recognition in England. Boyle, Hooke, and others admitted Bacon's greatness, but if Newton owed anything to Bacon he does not acknowledge it. Yet Walpole is probably fully justified in saying, "Bacon was the prophet of things that Newton revealed".

The majority of German metaphysicians, repelled by Bacon's scoffing protests against *a priori* views, have criticized him harshly. Spinoza regards his school as that of superficial industrialism; Hegel is equally uncomplimentary; but Leibnitz shows a marked apprecia-

[1] Cf. the *Essay on Bacon*, with Nichol, pp. 2, 3, 24, 25; Lewes, *Biog.*, p. 380. On Macaulay's reference to Bacon's "utility", see J. Grote, *Explor. Phil.*, vol. i, p. 13.

[2] Mill, *Logic*, ii, pp. 127, 373, &c.; Lewes, *op. cit.* p. 388.

tion of Baconian philosophy. Of more recent German thinkers Schopenhauer is one of the most sympathetic.[1]

A few other critics may receive passing mention. Condillac is quite complimentary, and D'Alembert extremely so. Cowley describes how Bacon chased away authority—

> "Nor suffered living men to be misled
> By the vain shadows of the dead."[2]

Hume's praise was of a modified character.[3] Voltaire's was excessive. De Maistre was frankly hostile, and so were Brewster[4] and Liebig.[5]

Concerning Bacon as a man it would be easy to quote from a large number of writers. Spedding is Bacon's greatest defender; Sortain is "piously hostile"; Hepworth Dixon is an admirer and a friend, but too partisan; Dr. Abbott is both ungenerous and unfair. Sir Sidney Lee pronounces Abbott's book as "the best summary of Bacon's life and work", without even making a reference to Spedding's far greater performance. Dean Church comes to the conclusion, though apparently with regret, that "it is vain to fight against the facts of Bacon's life". Mr. S. H. Reynolds says, "For accuracy of detail Bacon had no care whatever, and this may be set down as probably part of his craft".[6] Such a reckless assertion carries its own condemnation. Mr. J. M. Robertson's attitude towards Bacon is that of an impartial judge.[7]

The hostility of some of Bacon's critics is due to their unfavourable opinion of his personal character; of others, because they were under the impression that he was a Materialist; and of still others, because of his "utilitarian" philosophy. But his impartial critics brush aside all such issues, as not being germane to the case before them. In the main their reasoned opinions of Bacon as a man are entirely favourable, and they have the deepest respect for him as a philosopher.

§ 11. Bacon and Aristotle

Two men stand out, says Church, "the masters of those who know", without equals up to their time—the Greek Aristotle and the Englishman Bacon. They agree in the universality and com-

[1] Cf. Nichol, ii, pp. 232–43.
[2] *Ode to the Royal Society.*
[3] *Hist. of Eng.*, Appendix to Reign of James I.
[4] *Life of Newton.*
[5] *Ueber Francis Bacon.*
[6] *Essays*, Clar. Press Edition, Introduction, p. xxxiv.
[7] Cf. Robertson, *Bacon*, pp. 45, 47, 51, &c.

prehensiveness of their conception of human knowledge; and they were absolutely alone in their serious practical ambition to work out this conception. In the separate departments of thought, of investigation, of art, each is left far behind by numbers of men, who in these separate departments have gone far deeper than they, have soared higher, have been more successful in what they attempted. But Aristotle first, and Bacon after him, ventured on the daring enterprise of "taking all knowledge for their province"; and in this way they stood alone. The new world of knowledge has, however, turned out in many ways very different from what Aristotle or Bacon supposed; but their industry, their courage, their genius, in doing what none had done before, makes it equally stupid and idle to impeach their greatness.[1]

Aristotle condemned the methods of his predecessors; Bacon condemned Aristotle's; we condemn Bacon's; and no doubt ours will in time also be condemned. The perfection of method comes with the completion of experience.

"What Bacon says of Plato is pre-eminently true of himself. 'He was a man of sublime genius, who took a view of everything as from a high rock.'[2] Now to the young student I know nothing of so much importance as to be brought into contact with works of real genius, and there must be many men who recollect the transition from dry manuals of Logic to the brilliant pages of Bacon as forming one of the eras in their lives."[3]

Finally, what are the lessons which Bacon communicates?—"The duty of taking nothing upon trust which we can verify for ourselves; of rigidly examining our first principles; of being carefully on our guard against the various delusions arising from the peculiarities of human nature, from our various interests and pursuits, from the force of words, and from the disputes and traditions of the different schools of thought; the duty of forming our conclusions slowly and of constantly checking them by comparison with facts; of avoiding merely subtle and frivolous disputations; of confining our inquiries to questions of which the solution is within our power; and of subordinating all our investigations to the welfare of man and society."[4] And what greater lessons have been taught by any philosopher of any age?

[1] Church, pp. 201-4. [2] *De Aug.*, iii, 4. [3] Fowler, p. 129.

[4] Fowler, p. 129. It is a remarkable thing how one reviewer after another will accept Macaulay's estimate of Bacon's personal character. Two main charges are commonly brought against Bacon, viz., treachery to Essex, and bribery. As regards the first, Bacon's duty as head of the State was far greater than his very limited obligations to Essex. As regards the

CHAPTER X

Descartes

(1596–1650)

§ 1. Descartes Dissatisfied with Existing Philosophic Systems

Réné Descartes Duperron was born in Touraine in 1596. Bacon had then reached his thirty-sixth year.

The remark sometimes made that, for the man of genius, there is no education but what he gives himself, seems to apply with peculiar force to Descartes, who, on leaving the Jesuit College of La Flèche, declared that he had derived no other benefit from his studies[1] than that of a conviction of his utter ignorance, and a profound contempt for the systems of Philosophy then in vogue. The incompetence of philosophers to solve the problems with which they occupied themselves, the fact that no two thinkers could agree upon fundamental points, the extravagance of the conclusions to which some accepted premisses led, "determined him to seek no more to slake his thirst at their fountains".[2]

"As soon as my age permitted me to pass from under the control of my instructors, I entirely abandoned the study of letters and resolved no longer to seek any other science than the knowledge of myself, or of the great book of the world. I spent the remainder of my youth in travelling, in visiting courts and armies, in holding intercourse with men of different dispositions and ranks, in collecting varied experience, and, above all, in making such reflection on the matter of my experience as to secure my improvement. For it

second, there is no doubt that Bacon did accept gifts from suitors, but in not a single case is there any reason to believe that he was corruptly swayed by the gifts, and in taking these he was simply following the judicial practice of his day. Pope's gibe that Bacon was "the meanest of mankind" probably explains, in some measure, Macaulay's prejudice. The dislike shown to Bacon by Sortain and Dr. Abbott probably arose from the fact that Bacon did not champion any particular creed, but there is absolutely no doubt about the sincerity of his religious convictions. Roscoe's opinion that Bacon was "a crafty man" may be measured by his fulsome flattery of Coke, who was one of "the most truculent and unscrupulous of English lawyers". No doubt Bacon was ambitious, and no doubt he was, as Dean Church says, "a pleaser of men". These are admitted faults. But most of the charges usually levelled against Bacon have no foundation in fact, and Spedding has no difficulty at all in tearing Macaulay's case to pieces. The reader should refer to the works of Mr. Spedding and to those of Mr. J. M. Robertson.

[1] In "Mathematics, Physics, Logic, Rhetoric, and the ancient languages".

[2] Cf. Lewes, *Biog. Hist. Phil.*, pp. 391-2.

occurred to me that I should find much more truth in the reasonings of each individual with reference to the affairs in which he is personally interested, and the issue of which must presently punish him if he has judged amiss, than in those conducted by a man of letters in his study, regarding speculative matters that are of no practical moment, and followed by no consequences to himself, further, perhaps, than that they foster his vanity the better the more remote they are from common sense; requiring, as they must in this case, the exercise of greater ingenuity and subtlety to render them plausible."[1]

For many years Descartes thus led a roving unsettled life, now serving in the army, now making a tour; now studying Mathematics in solitude, now conversing with scientific men. At the age of thirty-three he retired into Holland, there in silence and solitude to arrange his thoughts into a consistent whole. When, several years later, the results of his meditations were given to the world, in the shape of his celebrated *Discourse on Method*, the sensation produced was immense. It was evident to all men that an original and powerful thinker had arisen, and although, as might be expected, this originality could not but rouse much opposition, just as originality almost always does, yet Descartes soon gained the day, and his fame became European.[2]

§ 2. He Considers a New Method of Procedure Necessary

We have already seen that Scholasticism had, before this time, quite worked itself out. Even in Italy itself many men[3] were deeply inspired by the spirit of revolt against authority, and were asserting the freedom, individuality, and supremacy of thought. And the far-reaching speculative tendencies of the time were reinforced by the new spirit of inquiry applied to nature by Copernicus, Kepler, Galileo, and Bacon. Bacon's *Novum Organum* appeared in 1620; Descartes' *Method* seventeen years later.[4]

Descartes had none of the outspoken boldness which we are accustomed to associate with great reformers, and one of his biographers goes so far as to say that he was "timid to servility".[5]

[1] *Discourse on Method*, Part I Cf Veitch, *Descartes*, p. 10; and Lewes, *op. cit.* p. 392.

[2] Cf. Lewes, *op cit.* pp. 392-3.

[3] For instance, Bruno, Vanini, and Campanella, whose lives will interest the reader. The attack on Aristotle by Ramus is also interesting.

[4] Cf. Veitch, *Descartes*, xvi-xxi. Descartes had not seen the *Organum* before thinking out his *Method*.

[5] Cf. Lewes, p. 393.

He certainly did not care—perhaps he did not dare—to encounter the powerful opposition of the Church. As far as possible he avoided the appearance of an innovator, although this is exactly what he was in the truest sense of the word. When he attacked an old dogma, it was not by a daring march up to the face of it, but rather by a quiet process of sapping the foundations. He got rid also of traditional principles not so much by direct attack as by substituting for them new proofs and more acceptable grounds of reasoning.

Though Descartes probably read more than some of his admirers supposed, he was not in any strict sense a reader. His wisdom grew mainly out of his own reflections, calmly yet ceaselessly pursued. Of mere learning and scholarship he had no esteem. He was in many ways typical of the self-reliant, somewhat harsh and intolerant, man of science, to whom erudition and all the heritage of the past seem to be mere trifling.[1] The very first sentence of his philosophy contains this celebrated declaration: "Since we begin life as infants, and have contracted various judgments concerning sensible things before we possess the entire use of our reason, we are turned aside from the knowledge of truth by many prejudices; from which it does not appear that we can be any otherwise delivered, than if once in our life we make it our business to doubt of everything in which we discern the smallest suspicion of uncertainty".[2] All authority he threw aside, and boldly attempted to solve by reason alone the problems which hitherto had been solved by faith.[3]

"I had become aware," he said, "even so early as during my college life, that no opinion, however absurd and incredible, can be imagined, which has not been maintained by some one of the philosophers; and afterwards I was led to infer that the ground of our opinions is far more custom and example than any certain knowledge. And I remarked that a plurality of suffrages is no guarantee of truth where it is at all difficult of discovery, as in such cases it is much more likely that it will be found by one than by many. I could, however, select from the crowd no one whose opinions seemed worthy of preference, and thus I found myself constrained, as it were, to use my own Reason in the conduct of my life."[4]

[1] Cf. *Ency. Brit.*, "Descartes", p. 118.
[2] *Prin of Phil*, I (i). Cf. Veitch, p. 193; and Whewell, *Phil. of Disc.*, p. 163.
[3] Cf. Morel, *Phil* vol. i, p. 152; and Lewes, p. 394.
[4] *Discourse on Method*, Part II. Cf. Veitch, pp. 16, 17.

§ 3. His "Organon" is "Doubt"

Descartes considered it necessary to examine the premisses of every conclusion, and to believe nothing but upon the clearest evidence of reason, evidence so convincing that he could not by any effort refuse to assent to it.[1] Yet he could find no criterion of positive certainty in any existing philosophic system. The great question to him, therefore, was, "Is there in knowledge an ultimate basis which I can regard as absolutely true and certain?" "And, supposing this found, can I obtain from it a criterion of truth and certainty?"

In the settlement of these questions, *doubt* was the great instrument which Descartes pressed into his service. He began with an examination, by reflection, of the facts of consciousness. Of what, and how far, can we doubt? We can doubt, Descartes would say, whether it be true, as our senses testify, or seem to testify, that a material world really exists; we can doubt even of mathematical truths, at all events when the evidence is not directly present to our minds. But in the pursuit of a reflective doubt, we are bound at last to reach the boundary line which divides doubt from certainty. This dividing line Descartes found in self-consciousness; for self-consciousness implies self-existence, and about this there can be no doubt at all. We thus have a method which seems to make the least possible assumption. It starts simply from the fact of a conscious questioning; it proceeds to exhaust the sphere of the doubtable; and at last it reaches that truth or principle which is its own guarantee.[2]

Descartes has told us how he found that he could plausibly enough doubt of everything, except of his own existence. He pushed his scepticism to the verge of self-annihilation. There he stopped: there in Self, there in consciousness, he found at last irresistible Certainty.[3] "The reality of the 'Ego' of Descartes is inseparably bound up with the fact of the definite act of consciousness." "The act and the Ego are the two inseparable factors of the same fact or experience."[4]

§ 4. "Cogito ergo sum"

Descartes felt that he might doubt the existence of the external world and treat it as a phantom. But of the existence of his

[1] Cf. Lewes, *op. cit.* p. 395.
[2] Cf. Veitch, pp. xxii, xxiii.
[3] Cf. Lewes, p. 395.
[4] Veitch, p. xxii.

thinking, doubting mind, no sort of doubt was possible. He, the doubter, existed, if nothing else existed. The existence that was revealed to him in his own consciousness was the first absolute certainty. Hence his famous *cogito ergo sum*: I think, therefore I am.[1]

The object of Descartes was *to find a starting-point from which to reason*, to find an irreversible certainty; and he found this in his own consciousness: "Doubt as I may, I cannot doubt of my own existence, because my very doubt reveals to me something which doubts. You may call this an assumption if you will; I point out the fact to you as a fact above and beyond all logic, a fact which logic can neither prove nor disprove, a fact which must always remain an absolute certainty, and as such a fitting basis for philosophy. *Je pense, donc je suis.* — I exist. This is a certainty if there be none other. It is in vain to ask a proof of that which is so irresistibly self-evident."[2]

We are assured of our own existence because the conception of existence is at once involved in the consciousness of self. We can distinguish the two elements but we cannot separate them; whenever we clearly and distinctly conceive the one, we are forced to think of the other along with it. But this gives us a rule for all judgments whatever. Whatever we cannot separate from the clear and distinct conception of anything, necessarily belongs to it in reality; and, on the other hand, whatever we can separate from the clear and distinct conception of anything, does not necessarily belong to it in reality. If, then, we set an object clearly before us, and separate it in thought as far as is possible from all other objects, we shall at once be able to determine what properties and relations are essential, and what are not essential to it. And if we find empirically that any object manifests a property or relation not involved in the clear and distinct conception of it, we can say with certainty that such property or relation does not belong to it except by arbitrary arrangement of things which have no natural affinity or connection.[3]—This is Cartesianism in a nutshell.

[1] Lewes, p. 395. Cf. *Discourse on Method*, Part IV; and Veitch, p. 33; also Maurice, *Mor. and Metaph. Phil.*, vol. ii, p. 299; and Grote, *Expl. Phil.*, ii, pp. 35, 41, 79, 178-9.

[2] Lewes, p. 396; and cf. *Ency. Brit.*, "Descartes", p. 122. In regard to Gassendi's objection, see Lewes, p. 395, and Veitch, p. xxiv. As to Huxley's criticism, see Veitch, pp. xxvi–xxvii.

[3] Cf. Caird, *Cartesianism*, p. 143.

§ 5. Clear and Distinct Ideas

Descartes, then, introduced a new method. It was indeed but another shape of the old formula "Know thyself", but it gave that formula a precise significance. It is of little use to tell a man to know himself by examining the nature of his thoughts: that had been done without success. Or by examining the *process* of his thoughts; that, too, had been accomplished, and the logic of Aristotle was the result.—The formula needed a precise interpretation, and that Descartes supplied. Thus, the vital portion of his system lies in this axiom: *whatever is clearly and distinctly conceived is true.* This axiom he regarded as the foundation of all Science.[1]

Knowledge, then, must be "clear and distinct". Descartes has defined this test in the following words: "I call that *clear* which is present and manifest to the mind giving attention to it, just as we are said clearly to see objects, when, being present to the eye looking on, they stimulate it with sufficient force, and it is disposed to regard them; but the *distinct* is that which is so precise and different from all other objects as to comprehend in itself only what is clear".[2] By *clear* Descartes means with plenty of light upon the idea, so that it is not dim or obscure; by *distinct* he means standing out in bold relief, so that there is no difficulty in distinguishing one idea from other ideas, and no confusing it with them. But we must not, of course, think that the clearness of its perception in any way *constitutes* the truth of the idea. Against the possibility of this error, Locke's view as to the aggregation of our knowledge, which sets plainly before us the dependence of trueness on *fact*, should always be kept in mind.[3]

§ 6. Descartes' Four Rules

Descartes having laid down his fundamental axiom, his next step was to determine the rules for the proper detection of the ideas. "As a multitude of laws often only hampers justice, so that a state is best governed when, with few laws, these are rigidly administered; in like manner, instead of the great number of precepts of which Logic is composed, I believed that the four following would prove

[1] Descartes, *Prin. Phil.*, Part IV; and Cf. Lewes, p. 397.

[2] *Prin. Phil.*, Part I; cf. Veitch, lv. Cf. also Leibnitz on Knowledge in *Meditationes de Cognitione, Veritate et Ideis*; and Veitch, p. lvi. Grote prefers the axiom, "False ideas cannot present themselves clearly and distinctly". See *Explor. Phil.*, ii, p. 206.

[3] Cf. Grote, *Explor. Phil.*, ii, pp. 41, 206–7.

perfectly sufficient for me, provided I took the firm and unwavering resolution never in a single instance to fail in observing them.

"'1. Never to accept anything as true but what is evidently so; to avoid precipitancy and prejudice; and to admit nothing but what so clearly and distinctly presents itself as true that there can be no reason to doubt it.'

"'2. To divide each of the difficulties under examination into as many parts as possible; that each part being more easily conceived, the whole may be more intelligible.' (Analysis.)

"'3. To conduct the examination with order, beginning by that of objects the most simple and therefore the easiest to be known, and ascending little by little to knowledge of the more complex.' (Synthesis.)

"'4. To make enumerations so complete and reviews so general, as to be confident that nothing essential has been omitted.'"[1]

It is true, say the Port Royalists,[2] that there is much difficulty in observing these rules, but it is always advantageous to have them in the mind, and to observe them as much as possible when we try to discover the truth by means of reason, and as far as our mind is capable of knowing it.

The four rules were Descartes' substitute for Logic. He had learned from Geometry to believe that all the subjects of human knowledge stood in a certain sequence, which may be detected if we are watchful never to assume as true what we have not ascertained to be true, and if we are on our guard against all "hasty jumps".[3] Descartes' mathematical studies profoundly influenced his method.

"There is no question more important to solve", says Descartes, "than that of knowing what human knowledge is and how far it extends." "This is a question which ought to be asked at least once in their lives by all who seriously wish to gain wisdom." And he points out "wherein consists all the knowledge we now possess, and what are the degrees of wisdom at which we have arrived. The first degree contains only notions so clear of themselves that they can be acquired without meditation; the second comprehends all that the experience of the senses dictates; the third that which the conversation of other men teaches us; to which may be added as the fourth, the reading, not of all books, but especially of such

[1] *Discourse on Method*, Part II. Cf. Veitch, p. 19; Lewes, p. 397; and Welton, *Logic*, ii, p. 215.

[2] *Port Royal Logic*, pp. 315–6. [3] Cf. Maurice, *Mor. and Metaph. Phil.*, ii, p. 297.

as have been written by persons capable of conveying proper instruction. And it seems to me that all the wisdom we in ordinary possess is acquired only in these four ways." Descartes then proceeds to castigate the philosophers,—men who in all ages have vainly endeavoured to find a fifth road to wisdom, by their search for "first causes and true principles from which might be deduced the reasons of all that can be known by man".[1] And a little further on, he says, "they who have learned the least of all that has been hitherto distinguished by the name of philosophy are the most fitted for the apprehension of truth".[2]

Professor Veitch thinks that the most liberal and probably the fairest interpretation of the criterion of Descartes is, that it is the assertion of *the need of evidence*, whatever be its kind, as the ground of the acceptance of a statement or proposition. As such, it is the expression of the spirit not only of the philosophy of Descartes but also of modern research.[3]

Descartes was fully aware of the general destructive tendency of his method, and he felt that there was a necessity, while he was seeking for new foundations, for the adoption of a provisional morality, that he might not remain irresolute in his actions whilst he was occupied in this search for principles. This provisional morality consisted of a few simple maxims; for instance, obedience to his country's laws, adherence to the faith of his childhood, and so on. And having thus secured himself, as he conceived, against the perils of scepticism, he had less difficulty in waiting for the assurance that he was always looking for. For nothing, he says, was less his desire than to doubt for doubting's sake. Instead of loving that shifting sand, he was impatient of it. He always believed there was a rock and that it could be found.[4]

§ 7. His Opinion of Logic

Descartes set great value upon the proper development of the reasoning powers. "Those to whom the faculty of Reason is predominant," he said, "and who most skilfully dispose their thoughts with a view to render them clear and intelligible, are always the best able to persuade others of the truth of what they lay down."[5] But he had a very poor opinion of Logic. "I found that its syllo-

[1] Cf. *Ency. Brit.*, "Descartes", pp. 122–3.
[2] Cf. Veitch, Preface to *Principles*, pp. 176, 179.
[3] Veitch, p. 59.
[4] Cf. Maurice, *Mor. and Metaph. Phil.*, vol. ii, p. 298.
[5] *Discourse*, i. Cf. Veitch, p. 8.

gisms and the majority of its other precepts are of avail rather in the communication of what we already know, or in speaking without judgment of things of which we are ignorant, than in the investigation of the unknown; and although the science contains indeed a number of correct and very excellent precepts, there are, nevertheless, so many others, and these either injurious or superfluous, mingled with the former, that it is almost quite as difficult to effect a severance of the true from the false as it is to extract a Diana or a Minerva from a rough block of marble."[1] In another place,[2] he says, "The logic of the schools is only a dialectic which teaches the mode of expounding to others what we already know, or even of speaking much, without judgment, of what we do not know".

§ 8. His Mathematics and Physics

Although, in later life, Descartes gave up the study of Mathematics, and was "anxious not to lose any more time in the barren operations of Geometry and Arithmetic",[3] yet he was one of the most eminent mathematicians of his age, and his mathematical training influenced to an enormous extent his views on things generally. His fame as a mathematician rests mainly on the application of Algebra to Geometry. Descartes must be considered not only as the founder of analytic Geometry, but also as the pioneer in the path which led up to the discovery of the Differential Calculus by Newton and Leibnitz.[4] It was Descartes who showed, in the most general manner, that every equation may be represented by a curve or figure in space, and that every bend, point, cusp, or other peculiarity in the curve, indicates some peculiarity in the equation.[5]

Descartes being convinced of the certitude of mathematical reasoning, gradually came to the conclusion that the method of Mathematics was capable of a much more extended application. As consciousness was his *basis* of certitude, so mathematical reasoning should be his *method* of certitude. His demonstrations were thus made to take the form of gradual and successive *deductions* from "clear and distinct ideas" which served as starting-points. The whole system was rigorously deductive and, as might be expected from a mathematician, was based on the smallest possible number

[1] *ib.* ii, p. 18. [2] Preface to *Principia*. Cf. Veitch, p. 183.
[3] Hamilton, *Discussions*, pp. 277-8.
[4] Cf. Mahaffy, *Descartes*, pp. 208-9; and *Ency. Brit.*, "Descartes", p. 121.
[5] Jevons, *Prin. of Science*, p. 632.

of assumptions. He applied this method even to Physics. When, for instance, in his *Principia* he announces his intention of giving a short account of the principal phenomena of the world, he says, "we desire to deduce effects from causes, not causes from effects".[1] Thus, he arrives at his results by pure *a priori* reasoning, in direct opposition to the method of observation and experiment. Descartes was convinced that Geometry furnished the one model of proof.

Descartes proceeds to apply his method to particular cases. In his *Dioptrics* he professes to deduce the laws of reflection and refraction of light from certain arbitrary comparisons, in which the radiation of light is represented by the motion of a ball impinging upon the reflecting or refracting body. It is a curious instance of the caprice of fortune that Kepler, one of the greatest men of science of his day, failed to detect the law of refraction; while Descartes, who professed to be able to despise experiment, actually claimed to have discovered it. There is, however, good reason to believe that Descartes really learned the law of sines from Snell's papers.[2] But whether this be so or not, it is certain that, notwithstanding the profession of independence of experience which his philosophy made, in reality experience constantly guided and instructed him. Descartes' reasonings and explanations were, consciously or unconsciously, often directed by the known facts, which he had observed for himself or learnt from others.[3] He certainly often did seek in facts for the law that lies at the foundation of them. And to this extent, of course, he departed from the method he had been so careful to lay down.

§ 9. His Theory of Vortices

Descartes' famous theory of vortices is perhaps the best-known example of his method.

He begins by banishing the notion of a *vacuum*, not, as his contemporaries said, because Nature has a horror of a vacuum, but because the essence of substance being extension, wherever there is extension there must be substance. The infinite universe must therefore be infinitely full of matter; it is an absolute *plenum*. Consequently, empty space must be a chimera. The substance which thus fills all space must be assumed to be set in motion;[4] the parts

[1] Cf. Mahaffy, *Descartes*, pp. 146, 150; Lewes, pp. 328-9; Whewell, *Phil*., p. 161.
[2] But cf. Mahaffy, p. 209. [3] Cf. Whewell, *Phil.*, p. 162.
[4] I.e. by the Deity. The reader will be interested in Descartes' own exposition in his *Principia Philosophiæ*, where it is illustrated by diagrams.

will then necessarily be ground into a spherical form, and the corners thus rubbed off, like filings or sawdust, form a second and more subtle kind of substance. There is, besides, a third kind of substance, coarser and less fitted for motion. Now we seem to have no alternative but to assume that the motions thus set up are, in form, like whirlpools, or *vortices*. Such circular motion will cause the "first matter" to collect at the centre of each vortex and make luminous bodies, such as the sun and fixed stars; the "second matter" will surround it, and by its centrifugal force constitute light and form the transparent substance of the skies. The third kind of substance is the material of opaque bodies, such as the earth and the planets. An innumerable series of vortices of matter will thus be produced, in which are carried along the grosser bodies situated in them. Our solar system is such a vortex, and the planets are carried round the sun by the motion of this vortex, each planet being at such a distance from the sun as to be in a part of the vortex suitable to its solidity and mobility. The motions are prevented from being exactly circular and regular by various causes. For instance, a vortex may be pressed into an elliptical shape by contiguous vortices.[1]

Now vortices are on the whole plausible suppositions; for planets and satellites bear at first sight much resemblance to objects carried round in whirlpools, an analogy which doubtless suggested the theory. The system is not, then, intrinsically absurd and inconceivable, but it breaks down because it does not give results in accordance with the actual motions of the heavenly bodies. And the very first requisite of a hypothesis is *its agreement with observed facts*.[2] Thus Descartes' method fails, as indeed it fails in practically all his investigations into Science.

§ 10. Why the Method Fails

Although all knowledge of the external world is in reality only to be obtained by observation and induction, the mind conforms to these conditions reluctantly, and is ever ready to rush to general principles and then to employ itself in drawing conclusions from these by deductive reasoning. Men therefore readily overlooked the precarious character of Descartes' fundamental assumptions, in their admiration of the skill with which a varied and complex uni-

[1] See Lewes, pp. 402-3; *Ency. Brit.*, "Descartes", p. 124; Mahaffy, pp. 158-9; and *Principia*, iii, 47.

[2] See Jevons, *Prin. of Sci.*, pp. 511, 517.

verse was evolved out of them. As Mackintosh says,[1] "A system which attempts a task so hard as that of subjecting vast provinces of human knowledge to one or two principles, if it presents some striking instances of conformity to superficial appearances, is sure to delight the framer, and for a time to subdue and captivate the student too entirely for sober reflection and rigorous examination. Consistency passes for truth. When principles in some instances have proved sufficient to give an unexpected explanation of facts, the delighted reader is content to accept as true all other deductions from the principles. Specious premisses being assumed to be true, nothing more can be required than logical inference."

Descartes' system was, at bottom, a glaring example of that error which Bacon had called *Anticipation*,—that illicit generalization which leaps at once from special facts to principles of the widest and remotest kind. Yet Descartes believed that his demonstrations equalled, and even surpassed, the demonstrations of Geometry, such belief being founded on the very nature of certitude as conceived by him.

Now, is Descartes right or wrong in his assumption that consciousness is the ultimate ground of certitude? Can ideas be regarded as the internal copies of external things?

Lewes, in no uncertain terms, declares that Descartes is wrong. "Consciousness is the ultimate ground of certitude *for me*; if I am conscious that I exist I cannot doubt that I exist; if I am conscious of pain, I must be in pain. This is self-evident. But what ground of certitude can consciousness be respecting things which are '*not-me*'? The principle can only extend to things which relate *to me*. I am conscious of all that passes within *myself*, but I am not conscious of what passes in *not-self*: all that I can possibly know of the *not-self* is *its effects on me*. Consciousness is therefore 'cabin'd, cribb'd, confin'd' to *me*, and to what passes within *me*. So far does the principle of certitude extend, and no farther. Any other ideas we may have, any knowledge we may have respecting *not-self*, can only be founded on *inferences*, and directly we leave the ground of consciousness for that of inference, our knowledge becomes questionable. Thus the mathematical certainty which Descartes attributes to these inferences becomes a great uncertainty."[2]

Descartes says that not only do we know things exclusively through the medium of ideas, but also that, in consequence of this,

[1] *Dissertation on Ethical Sciences.* Cf. Whewell, *Phil.*, p. 164.
[2] Lewes, p. 405.

whatever we find in the ideas must necessarily be true of the things. His reason is that, as ideas are caused in us by objects, and as every effect must have as much reality as the cause, so must ideas have the same reality as things. But this is a fallacy. Descartes assumes that the mind is a passive recipient,—a mirror in which things reflect themselves. In truth, however, we are utterly unable to apprehend the real nature of things external to us. "When we are placed in contact with external objects, they operate upon us; their *operations* we know, *themselves* we cannot know." We have no right to infer that the ideas excited are exact *copies* of the exciting causes, or that they apprehend the whole nature of the causes.[1]

§ 11. The Cartesian Method and the Baconian Method

Metaphysicians join hands with Descartes and turn their backs on Bacon because Descartes held the doctrine of the existence of Innate Ideas.[2] But the assumption that we have ideas absolutely independent of all experience is one of the fundamental errors of metaphysical speculations. That we have no such ideas seems now to be clearly established.[3] It may be urged that Descartes hardly seemed to realize the importance of Innate Ideas to his system, but the fact remains that such Ideas form the very basis of the Cartesian doctrine. And Cartesianism would be unacceptable to Science from this fact alone.

Bacon and Descartes lived at a time when reforms were urgently wanted. When Bacon urged attention, and ever-repeated attention, to nature, to fact, to observation, to experiment, he was performing a most useful and greatly needed task; but when he advised less attention to books, and to reflection or self-concentrated thought ("idle cobwebs of the brain"), it is less safe to follow him. For, after all, what was wanted in regard to books and thought was exactly the same as was wanted in regard to observation, not at all *less* attention, but a wiser, better applied, more real attention, which would really help, instead of hindering, observation of nature and fact. Of this wiser and better reflection or thought, Descartes was the apostle, as Bacon was of better and more abundant observation.[4]

Science must employ deduction as well as induction, and Bacon's greatest error was in not sufficiently acknowledging it. Hence, we

[1] Cf. Lewes, p. 406.
[2] Dugald Stewart denies that Descartes held the doctrine, but see Lewes, p. 407.
[3] Cf. Lewes, pp. 407–8, and see the next chapter.
[4] Cf. Grote, *Explor. Phil.*, vol. i, p. 224.

may partly account for the curious fact that Bacon, with his cautious method, made no discoveries, while Descartes, with his rash method, made important discoveries. But of course Descartes' knowledge of Mathematics had much to do with this, for the discoveries were of a kind to which the mathematical method was strictly applicable.

In Bacon's view, the important thing about truth was its closeness to fact; in Descartes' view, clearness and distinctness of thought.[1] Bacon teaches us one great lesson, Descartes another. The lessons are complementary and we need them both.

As we have seen, Bacon's *method* is of little value because it is impracticable. But Descartes' method is not only impracticable but also dangerous, for it teaches us to deduce conclusions from general principles assumed independently of all experience, to deduce effects from imagined causes rather than causes from known effects. The special danger of the Cartesian method is its attractiveness and its plausibility.

Cartesianism took but slight hold in England. Towards the close of the seventeenth century, its only remnants were "an overgrown theory of vortices, which received its deathblow from Newton; and a dubious phraseology about Innate Ideas, which found a witty executioner in Locke".[2]

CHAPTER XI

Locke

(1632–1704)

§ 1. Characteristics of Locke

From Westminster School, which he entered at fourteen, Locke passed on to Christ Church, but the strong attachment of the University to all that was old, and its hardly disguised contempt for anything new, seem to have irritated him. Though now utterly routed elsewhere, Scholasticism was still strongly entrenched at Oxford, and scholastic studies and disputations were maintained with an ostentatious formality entirely unsuited to Locke's inquisi-

[1] Cf. Grote, *Explor. Phil.*, vol. i, pp 225–6.

[2] *Ency. Brit.*, "Descartes", p. 128. The method of Descartes was pushed to its ultimate conclusion by Spinoza, whose works the reader should consult.

tive temper. Like his great predecessor Bacon, he soon imbibed a profound contempt for university studies, and in after-life regretted that so much of his time should have been wasted on such profitless pursuits. So deeply convinced was he of the vicious method of college education that he ran into the other extreme and thought self-education the best.[1]

When considering Locke as a philosopher, it is important to realize that he was no mediæval monk but an English gentleman and a man of the world. He represented all that was best and most accomplished in the English lay mind, and he was exactly the man then wanted to resist the clerical or university mind. But tenacious and effective as this resistance was, it was absolutely untinged with any form of fanaticism.[2]

As a writer, Locke is clear, truthful, direct. "He indulges in no vague formulas, no rhetorical flights, no flattery of prejudices, and no word jugglery."[3] Yet his language is sometimes ambiguous and sometimes even contradictory,[4] and no attempt should therefore be made to gather his opinions from isolated and casual expressions, since these often require to be interpreted on the general analogy of his system. Locke was anxious to make himself intelligible, and to this end he varied his expressions and stated his meaning in a variety of forms. He must be read more than once, or he will almost inevitably be misinterpreted.

The essence of Locke's philosophy is the reasonableness of taking probability as the ultimate guide in all the really important concerns of life. A repugnance to believe blindly what rested on authority, as distinguished from what was seen to be sustained by self-evident reason, or by demonstration, or by good probable evidence, runs through his life. He is a typical man of science in his reverence for facts, in his tendency to turn away from merely verbal reasonings, and in his ready submission to truth.[5] Locke sought anew for himself a solid foundation for human knowledge.[6] Like Bacon, he turned away with aversion from Scholasticism, which seemed to him to encourage the two chief hindrances to the intellectual liberty of the individual,—empty verbalism, and unverified assumption.[7]

[1] Cf. Lewes, *Biog. Hist. Phil.*, pp. 448-9; *Ency. Brit.*, "Locke", p. 752; Alexander, *Locke*, p. 4.
[2] Cf. St. John, *Locke*, vol. i, p. 17; Maurice, *Mor. and Met. Phil.*, vol. ii, p. 440.
[3] Cf. Lewes, p. 454. [4] Cf. Hamilton, *Discussions*, p. 78. [5] Cf. *Ency. Brit.*, p. 755.
[6] Cf. Morel, *Phil.*, vol. i, p. 91. [7] Cf. Frazer, *Locke*, pp. 40-41, 105-6.

§ 2. His Toleration

Locke had found all parties and sects disposed towards persecution. Brought up, like Bacon, in a Puritan household, his first sympathy was, very naturally, with the Puritans, but this was gradually lessened by what he saw of the intolerance of the Presbyterians and the fanaticism of the Independents. He found, he says characteristically, that "what was called general freedom was really general bondage".[1] And in his books he directly encouraged resistance to "masters or teachers who take men off the use of their own judgment, and put them upon believing and taking upon trust without further examination". That every man should be able to see things as things are, and not merely through the eyes of others, was his greatest wish. He pleaded strenuously for toleration; and his *Epistola de Tolerantia* has been called the most original of all his works.

The toleration for which Locke argued then implied a complete revolution in the previously received view of human knowledge and belief. It implied a protest against those who demand absolute certainty in questions where balanced probability alone is within the reach of human intelligence. Locke argued that the foremost duty of the Church was to promote goodness and to give no countenance to theological wranglings. "No man", he maintains, "is hurt because his neighbour is of a different religion from his own, and no civil society is hurt because its members are of different religions from one another." An encouragement of variety in individual opinion may be advantageous to society because it tends to develop the intellectual resources of mankind and thus adds to the security for the discovery of truth.

But even Locke did not teach the duty of an unlimited toleration by the State. He argues for the forcible suppression of opinions that operate to the dissolution of society, or which subvert those moral rules which are necessary to the preservation of order. He even applies this principle so as to exclude from toleration all who are themselves intolerant.[2]

[1] Cf. *Ency. Brit.*, "Locke", p. 752.
[2] See Fraser, *Locke*, pp. 88–94; and *Ency. Brit.*, xiv, 752–6.

§ 3. His Views on Education

Locke's philosophical works are: *Some Thoughts on Education; The Conduct of the Understanding;* and *An Essay Concerning Human Understanding.*

In *Thoughts on Education*, information and mere learning are subordinated to the formation of character and practical wisdom. Accumulating facts in the memory without using the power to think, and without accustoming the youthful mind to apply reason to the evidence by which individual thoughts must be tested, is always referred to as the cardinal vice in teaching. "Truth needs no recommendation," says Locke, "and error is not mended by it; in our inquiry after knowledge, it little concerns us what other men have thought."[1]

§ 4. "The Conduct of the Understanding"

The *Conduct of the Understanding* was designed as an additional chapter to the *Essay*, but the main theme on which it treats is connected rather with the work of self-education than with the analysis of knowledge. The suggestive nature of this little volume will be seen from the following passages, culled almost at random:—

"Let not men think there is no truth but in the sciences that they study, or the books that they read. To prejudge other men's notions before we have looked into them is not to show their darkness but to put out our own eyes."[2]

"To those who would shake off the great and dangerous monster, prejudice, who dresses up falsehood in the likeness of truth, I shall offer this one mark whereby prejudice may be known. He that is strongly of any opinion must suppose that his persuasion is built upon good grounds, and that his assent is no greater than what the evidence of the truth he holds forces him to, and that they are arguments and not inclination or fancy that make him so confident and positive in his tenets. Now if, after all his profession, he cannot bear any opposition to his opinion, if he cannot so much as give a patient hearing, much less examine and weigh the arguments on the other side, does he not plainly confess it is prejudice governs him?"[3]

[1] Cf. *Ency. Brit.*, xiv, p. 756. The whole of *Thoughts on Education* is well worth reading. The teacher will pick up many valuable hints, though, of course, much of the advice Locke gives is now only of historical interest. Locke's views should be compared with those of Comenius, then with those of Rousseau. (See Fowler, pp. 173–6.)

[2] Section iii, "Reasoning".

[3] Section x, "Prejudice".

"Reading furnishes the mind only with materials of knowledge; it is thinking makes what we read ours. The memory may be stored, but the judgment is little better, and the stock of knowledge not increased, by being able to repeat what others have said."[1]

"I do not say but a man should embrace some opinion when he has examined, else he examines to no purpose; but the surest and safest way is to have no opinion at all till he has examined, and that without any the least regard to the opinions or systems of other men about it."[2]

§ 5. Locke's "Essay"

The object of Locke's great philosophical work, *Essay Concerning Human Understanding*, was "to inquire into the original, certainty, and extent of human knowledge". Locke desired to make a faithful report, based on what he actually found, as to the extent to which we can share in certain knowledge, and in what cases we can "only judge and guess" on grounds of probability. The *Essay* was intended to be a defence of intellectual freedom. Locke waged war against dogmas which refuse to be verified by facts; and against words and phrases for which there are no corresponding ideas or meanings. He believed that by insisting upon a recognition of "experienced" ideas, he was helping to put demonstration and well-calculated probabilities in the room of blind repose upon authority.[3]

Although discovery of the nature and extent of the few certainties that are within the scope of human understanding and of the ground and office of probability, is announced as the aim of the *Essay*, it is curious that only the last of the four books into which the *Essay* is divided is directly concerned with this subject. But before reference is made to the main theme of the *Essay*, there are two subsidiary topics which deserve passing mention: (1) the imperfections of language, which Locke deals with in his third book; and (2) the association of ideas, which forms the subject of the last chapter of the second book.

[1] Section xx, "Reading". [2] Section xxxv, "Ignorance with Indifferency".
[3] Cf. *Ency. Brit.*, p. 757; and Frazer, *op. cit.* p. 107.

§ 6. The Ambiguities of Language

To the ambiguities of language Locke gave special attention. We make two or three typical extracts from the *Essay*.

"To make words serviceable to the end of communication, it is necessary that they excite in the hearer exactly the same idea they stand for in the mind of the speaker. Without this, men fill one another's heads with noise and sounds, but convey not thereby their thoughts, and lay not before one another their ideas. Names of very compound ideas, such as for the most part are moral words, have seldom in two different men the same precise signification."[1]

"Men take the words they find in use amongst their neighbours; and that they may not seem ignorant what they stand for, use them confidently, without much troubling their heads about a certain fixed meaning; whereby, besides the ease of it, they obtain this advantage, that, as in such discourses they seldom are in the right, so they are as seldom to be convinced that they are in the wrong."[2]

"All the art of Rhetoric, besides order and clearness, all the artificial and figurative applications of words eloquence hath invented, are for nothing else but to insinuate wrong ideas, move the passions, and thereby mislead the judgment, and so indeed are perfect cheats; and therefore however laudable or allowable oratory may render them in harangues and popular addresses, they are certainly in all discourses that pretend to inform or instruct, wholly to be avoided; and where truth and knowledge are concerned, cannot but be thought a great fault either of the language or person that makes use of them. It is evident how much men love to deceive and be deceived, since rhetoric, that powerful instrument of error and deceit, has its established professors and is publicly taught."[3]

"Toga, tunica, pallium, are words easily translated by gown, coat, and cloak; but we have thereby no more true ideas of the fashion of those habits amongst the Romans than we have of the faces of the tailors who made them."[4]

§ 7. The Association of Ideas

The chapter on the Association of Ideas at the end of the second book of the *Essay*, was introduced in the second edition, "not in any way philosophically explanatory either of the thoughts or of

[1] Ch. ix, § 6. [2] Ch. x, § 4. [3] Ch. x, § 34. [4] Ch. xi, § 25.

the knowledge and probable beliefs of men, but as the chief source of human prejudices, as a cause of human errors against which men need, in an especial manner, to be warned". This useful chapter was a kind of afterthought.[1]

The term *Association of Ideas* is used to denote the tendency which one thought in our minds has to introduce another thought. Such an association is apt, as Dugald Stewart points out,[2] to warp our opinions both by blending together in our apprehensions things which are really distinct in their nature, and by connecting in the mind erroneous opinions with truths which command our assent.

"Some of our ideas have a natural correspondence and connexion one with another; it is the office and excellency of our reason to trace these and hold them together in that union and correspondence which is founded in their peculiar beings. Besides this there is another connexion of ideas wholly owing to chance or custom; ideas that, in themselves, are not all of kin, come to be so united in some men's minds that it is very hard to separate them; they always keep in company, and the one no sooner at any time comes into the understanding, but its associate appears with it; and if they are more than two which are thus united, the whole gang, always inseparable, show themselves together."[3]

The chapter abounds in interesting illustrative examples. For instance: children are often taught to associate figure and shape with the idea of the Deity, and in later life have the greatest difficulty in severing the two notions. Again: although goblins and sprites have no more to do with darkness than with light, yet children are often afraid of the dark because a foolish maidservant has perhaps associated the dark with the existence of goblins and sprites.[4]

Further reference to the Association of Ideas will be made in the next chapter. Meanwhile we must touch upon the main feature of Locke's philosophy.

§ 8. Locke the Founder of Modern Psychology

Locke was the founder of modern introspective Psychology. He was the first "to watch patiently the operations of the mind

[1] This chapter should be compared with Hobbes, *Human Nature*, ch. iv, and *Leviathan*, I, iii; Hume, *Human Nature*, i, § 4, and *Essays on Hum. Und.*, § 3; Condillac, *Essai sur l'origine des Connaissance Humaines*, I, iii. See Fowler, *Locke*, p. 141. Locke appears to have been the first to use the exact expression "Association of Ideas". The reader will find some beautiful lines on this subject in Byron, *Childe Harold*, iv, 23-4.

[2] Dugald Stewart, *Works*, Part I, ch. v, ii, (1), p. 184. [3] § 5. [4] §§ 10, 17.

in order that he might, if possible, surprise the evanescent thoughts and steal from them the secret of their combinations". Others before him had cast a hasty glance inwards, but they dogmatized on what they saw; they contented themselves with the thoughts as they found them. Locke's object, however, was to discover the *origin* of the thoughts,[1]—to find the origin of "ideas" in the individual man and their connection in constituting knowledge.

There are two propositions on which Locke is constantly insisting: one, that the object of his investigation is his own mind; the other, that his attitude towards this object is that of mere observation. He speaks of his own mind just as he might of his own body. He regards the "minds" of different men as so many different things—"thinking things". This "thinking thing", as he finds it in himself, the philosopher has merely and passively to observe, in order to understand the nature of knowledge.[2] We thus see that Locke had imbibed the spirit of the Baconian method: just as with the study of matter, so with the study of mind; observation of facts must come first, then classification, then reasoning.

Locke's philosophy is pre-eminently a philosophy of "experience". It accepts nothing on authority, and no foregone conclusions. "It digs, as it were, into the mind, detaches the ore, analyses it, and asks how the various constituents come there."[3]

§ 9. The Origin of our Ideas

Hobbes and Gassendi believed that all our ideas are derived from sensations; *nihil est in intellectu quod non prius fuerit in sensu.* But Locke's fundamental proposition was that there are *two* sources, not one source, and that these were *Sensation* and *Reflection.* He separated himself decisively from the upholders of the doctrine of innate ideas—of truths independent of experience—and declared that all our knowledge is founded on experience. And he separated himself equally decisively from those who saw no source of ideas but Sensation, declaring that although *Sensation* was the great source of most of our ideas, yet there was "another fountain from which experience furnisheth the understanding with ideas"; and this source, "though it be not sense, as having nothing to do with external objects, yet it is very like it, and might properly enough be called *internal sense*"; this he calls *Reflection.*[4]

[1] Cf. Lewes, 455, 456, 459.
[2] Cf. Green and Grose, *Hume*, pp. 5–7.
[3] Fowler, *Locke*, p. 150.
[4] Cf. Lewes, p. 463.

"Since there appear", says Locke,[1] "not to be any ideas in the mind before the senses have conveyed any in, I conceive that ideas in the understanding are coeval with *Sensation*;[2] which is such an impression, made in some part of the body as produces some perception in the understanding. It is about these impressions made on our senses by outward objects that the mind seems first to employ itself in such operations as we call remembering, consideration, reasoning, &c. In time the mind comes to reflect on its own operations about the ideas got by Sensation,[3] and thereby stores itself with a new set of ideas which I call ideas of *Reflection*. These impressions which are made on our senses by outward objects; and the mind's own operations, proceeding from powers proper to itself, are the original of all knowledge."

By *Sensation* Locke understands "the simple operation of external objects through the senses". The mind is herein wholly passive. The senses, therefore, may be said to furnish the mind with one portion of its materials.

By *Reflection* he understands that internal sense by means of which the mind observes its own operations. This furnishes the second and last portion of the materials out of which the mind frames knowledge.[4]

§ 10. Simple and Complex Ideas

"When the understanding is once stored with *simple* ideas, it has the power to repeat, compare, and unite them, even to an almost infinite variety, and so can make at pleasure new *complex* ideas. But it is not in the power of the most exalted wit, or enlarged understanding, by any quickness or variety of thought, to invent or frame one new simple idea in the mind not taken in by the ways aforementioned."[5]

Of "simple ideas of sensation", some come into our minds by one sense only. Such are the various colours, sounds, tastes, and smells. The ideas we get by more than one sense are those of size, shape,[6] number, rest, motion.

[1] *Essay*, II, i, §§ 23–24.

[2] For a carefully reasoned criticism of this assertion, see Lewes, p. 466. Reference should then be made to the chapters on Sensation and Perception in Herbert Spencer's *Psychology*, followed by the corresponding chapters in Prof. W. James's *Psychology*

[3] The words "operations about the ideas" are a little obscure, but the general drift of the whole sentence is pretty obvious.

[4] Cf. Lewes, *Biog. Hist. Phil.*, p. 466; Whewell, *Phil. of Disc.*, p. 202; Green and Grose, *Hume*, pp. 8, 9.

[5] Cf. *Essay*, Book II, ch. i.

[6] Orig., *extension, figure.*

The "simple ideas of reflection" which the mind acquires when "it turns its view inward upon itself, and observes its own actions about those ideas it has received from without", are mainly two, namely, Thinking and Willing.

"There be other simple ideas which convey themselves into the mind by all the ways of Sensation and Reflection, namely, Pleasure or Delight, Pain or Uneasiness, and so forth."[1]

In the reception of these simple ideas, the mind is regarded by Locke as merely passive. It can no more refuse to have them, alter or blot them out, than a mirror can refuse to receive, alter, or obliterate the images reflected on it. But having once received its simple ideas, the mind is able to create out of them *complex* ideas, and that in an infinite variety; this it does chiefly by combining, comparing, and separating them.[2] In other words, while the mind is passive in respect of its two sources of experience, sensation and reflection, it is also active, in that it can compare, distinguish, and abstract. Thus *simple* ideas are the ultimate constituents of experience, the uncompounded appearances of things; *complex* ideas are the workmanship of the mind.[3] The mind manipulates the materials derived from sensation and reflection, and so manufactures the complex ideas. Or the complex ideas may be regarded as resolvable into these elements of sensation and reflection, together with an active element of construction referable to the mind itself.

As an instance of the manner in which Locke attempts to resolve a complex idea into simple ones, we may take the idea of "substance".[4]

If we examine our idea of, say, a horse, a man, or a piece of gold, we are able to resolve it into a number of simple ideas, such as size, shape, solidity, weight, colour, &c., coexisting together. But we feel that there must be, in addition to all these qualities, a *substratum*,[5] as Locke says, "wherein they do subsist, and from which they do result". Now of the various qualities, we can form a clear idea and give a more or less intelligible account.

[1] *Essay*, Book II, ch. vii, § 1. [2] Fowler, *Locke*, p. 136; and *Essay*, II, xii, § 1.

[3] Alexander, *Locke*, p. 33. In regard to simple ideas of sensation, Locke distinguishes between the ideas of the *primary* qualities (size, shape, number, rest, and motion) and ideas of *secondary* qualities of bodies (colour, taste, smell, &c.) Bodies possess primary qualities, no matter what change they undergo; but secondary qualities change with varying circumstances. Lewes thinks the real distinction should be that the former are *invariable* conditions of a sensation, and the latter variable. Cf. Lewes, pp. 468-9; and Alexander, pp. 34-5.

[4] For Locke's division of Complex Ideas (modes, substances, relations), see *Essay*, II, xii, § 3, &c. Cf. also Green and Grose, *Hume*, p. 32, &c.

[5] Cf. ch. iii, § 2, and ch. vii, § 4.

But can we form a clear idea or give an intelligible account of the substratum? A complex idea of an *unsubstantial* aggregate of sensible qualities, without a centre of unity to which they may be "attributed", Locke finds unthinkable. An adjective without a substantive is meaningless; and when we say that all adjectives necessarily presuppose substantives, we express in another way this obligation to *substantiate* our simple ideas. But Locke confesses candidly that we cannot form an idea-image of the substratum. If we ask *what* the "substance" is to which this colour or that odour belongs, and are told that it is the solid and extended particles of which the coloured and odorous mass consists, this indeed gives a substance that is picturable, as such particles are; but then it is inadequate to the genuine idea of substance, for we find that we are mentally obliged to ask in turn what *their* substance is, and having got in reply only something else that is picturable, we have to repeat the question for ever, as long as we get nothing which transcends the imagination. We are, says Locke, "in a difficulty like that of the Indian who, after explaining that the world rested on an elephant, which in its turn was supported by a broad-backed tortoise, could at last only suppose the tortoise to rest on 'some thing'—*I know not what*". Locke was baffled, as we are all baffled, by this endless incomprehensible regress. Curiously enough, Locke does not seem to have asked himself the question why we are mentally unable to refrain from thinking more than we can mentally picture.[1]

§ 11. Innate Ideas

We have already noticed that Locke waged war against "innate" ideas. Locke believed that by insisting upon a recognition of "experienced" ideas only, he was helping to put demonstration and well-calculated probabilities in the room of blind repose on authority. That a part of human knowledge, and this the most important part, exists from the first, ready made consciously in our minds, independently of experience and prior to experience, might be an opinion eminently calculated to "ease the lazy from the pains of search", but would not bear examination at all. A blind prejudice that their assumptions were "innate" was enough "to take men off their own reason and judgment and to put them upon believing and taking upon trust without further examination".

[1] See **Fraser**, *Locke*, pp. 147–50; Fowler, *Locke*, pp. 137–8; *Essay*, **II**, **xxiii**, **§ 2**, and **I**, **iv**, **§ 18**; *Ency. Brit.*, "Locke", vol. xiv, p. 760. Cf. ch. iii, § 2.

Locke challenged the defenders of innate ideas to produce them and show what and how many there are. Did men find such innate ideas "stamped on their minds" when they came into the world, nothing could be more easy than this. Those who attempt such an enumeration differ in the lists which they draw up, and give no sufficient reason why many other propositions which they regard as secondary and derived should not be admitted to the same rank with the so-called innate principles, which they assume to be primary and independent. It is, of course, impossible clearly to discriminate between those propositious which appear to be axiomatic and those which are derived. Race, temperament, capacity, habit, education, produce such differences between man and man, that a proposition which to one man appears self-evident and unquestionable will by another be admitted only after much hesitation, while a third will regard it as false. Especially is this the case with many of the principles of religion and morals, which have now been received by so constant a tradition in most civilized nations, that they have, for the most part, come to be regarded as independent of reason, and if not "engraven on the mind" from its birth, are at least exempt from discussion and criticism. But the fact that they are not universally acknowledged shows that to mankind, in general, at any rate, they are not axiomatic, and that, however clear and convincing the reasons for them may be, at all events those reasons require to be stated.[1]

Although the *Essay* attacks innateness in human knowledge, yet the "self-evidence" which comes to us gradually, "in the slowly growing light of educated reason", of much that we know is vigorously asserted. Locke says that in some cases the intellect becomes able to perceive a truth, as the eye does light, by being directed to it by intuition alone. The innate principles which he so persistently attacks are entirely different from those intuitive elements of knowledge, which are, by degrees, seen by growing reason to be either self-evident or demonstrable.[2]

Locke left much unexplained, and, in spite of all the ingenuity of subsequent thinkers,[3] we are still profoundly ignorant of the precise nature of the so-called *a priori* element of our knowledge. But modern investigation seems to suggest that considerable light

[1] See Fowler, *Locke*, pp. 129–32.

[2] Fraser, *Locke*, pp. 115–6. Cf. Morel, *Phil.*, pp. 101–4; Fowler, *Locke*, pp. 129–31; Maurice, *Mor. and Metaph. Phil.*, p. 441.

[3] Kant, for instance, tried to prove that it was only through certain *a priori* elements in the mind that our *a posteriori* experiences were intelligible.

may be thrown on the subject by the principle of Heredity. It is possible that what are called innate ideas are certain *tendencies* of the mind "to group phenomena under certain relations and to regard them under certain aspects; and the existence of such tendencies, so far as the individual is concerned, seems to be explained by the principle of hereditary transmission. And it seems probable that we must assign the formation of these tendencies to the continuous operation, through a long series of ages, of causes acting uniformly, or almost uniformly, in the same direction,—in a word, of Evolution."[1] A more positive answer cannot at present be given; for we are forced to admit that the beginnings of our intellectual life are still enshrouded in mystery.

§ 12. Locke's Critics

The majority of Locke's numerous critics have woefully misrepresented him. Their attitude has usually been quite unreasonable, for he spoke plainly and sought the truth, and never attempted to mystify anyone. "All those men who still seek to penetrate impenetrable mysteries, and who refuse to acknowledge the limits of man's intelligence, treat Locke with the same superb disdain as the ambitious alchemists treated the early chemists. The tone in which modern Frenchmen and Germans speak of Locke is painful; the tone in which many Englishmen speak of him is disgraceful. To point out any error is honourable, but to accuse him of errors which are not to be found in his work, and to misinterpret his language and then accuse him of inconsistency and superficiality, deserve the severest reprobation."[2]

Victor Cousin's criticisms were, as Lewes conclusively shows, shallow and unfair, but Cousin did at all events take the trouble to read the *Essay* with some care; whereas another critic, Dr. Whewell, impatiently remarked, "We need not spend much time in pointing out the inconsistencies into which Locke fell, as all must fall, who recognize no source of knowledge except the senses". But this is, of course, a misrepresentation. Whewell must have known that Locke did recognize another source, namely, in Reflection, and his criticism is therefore quite beside the mark.[3]

Locke's greatest critic was Leibnitz, who was a great mathe-

[1] Fowler, *Locke*, pp. 145-6 See also M'Cabe, *Evolution of Mind*; Macdonald, *The Child's Inheritance*; and the works of Galton and Weismann.

[2] Lewes, pp. 473-4. [3] Cf. Lewes, pp. 474-9.

matician and a Cartesian. Leibnitz was a follower of Plato, and was therefore naturally repelled by Locke's doctrines. Schlegel[1] has well observed that every man is born either a Platonist or an Aristotelian, and Leibnitz and Locke were examples of this antagonism. "Our differences", says Leibnitz, "are important. The question between us is whether the mind in itself is entirely empty, like a tablet upon which nothing has been written (*tabula rasa*) according to Aristotle and the author of the *Essay*, and whether all that is there traced comes wholly from the senses and experience; or whether the mind (soul) originally contains the principles of several notions and doctrines, which the external objects only awaken on occasions, as I believe with Plato."

Perhaps we must admit that there is some little justification for the attitude taken up by Locke's critics, despite the prejudice exhibited by most of them. For there undoubtedly is in the *Essay* a tendency to bring into prominence the passive receptivities of the mind, and to ignore, to some extent, its activity. The metaphor of the *tabula rasa*, the "sheet of white paper", exercises an influence over the whole work. The author is so busied with the variety of impressions from without, that he seems sometimes almost to forget the reaction of the mind from within.[2] But Locke was fighting for a reform of method, and he felt the necessity of laying stress on the hitherto neglected and despised factor in our means of obtaining knowledge. Perhaps, too, he did rather underrate the importance of the mental reaction which is essential to the formation of even the simple ideas of sensation. But, at that time, Physiology was in far too backward a state to throw much light upon Psychology, and Locke probably knew little or nothing of nerve-action or nerve-energy.[3]

"Few books", says Mackintosh,[4] "have contributed more than Locke's *Essay* to rectify prejudice, to undermine established errors, to diffuse a just mode of thinking, to excite a fearless spirit of inquiry. In the mental and moral world, which scarcely admits of anything that can be called discovery, the correction of the intellectual habits is probably the greatest service which can be rendered to science. In this respect, Locke is unrivalled: his writings have diffused throughout the civilized world the spirit of tole-

[1] *Geschichte der Literatur*. Coleridge used to pass off this aphorism as his own. See Lewes, p. 481.

[2] Cf. Fowler, *Locke*, p. 147. [3] Cf. Fowler, pp. 147–8.

[4] *Ed. Rev.*, Oct. 1821, p. 243. See also Lewes, pp. 457–8; and cf. Fox Bourne's *Life of Locke*, vol. ii, p. 136.

ration and charity in religious differences; the disposition to reject whatever is obscure or fantastic in speculation; to reduce verbal disputes to their proper value; to abandon problems which admit of no solution; to distrust whatever cannot be clearly expressed; to render theory the simple expression of facts; and to prefer those studies which most directly contribute to human happiness. If Bacon first discovered the rules by which knowledge is improved, Locke has most contributed to make us observe them. If Locke made few discoveries, Socrates made none. Yet both did more for the improvement of the understanding, and not less for the progress of knowledge, than the authors of the most brilliant discoveries."

It has been well said that nothing but good can result from communion with such a mind as Locke's. If the teacher desires to obtain "clear and distinct ideas" of the inner nature of his work, let him read Locke again and again.[1]

CHAPTER XII

Hume

(1711–1776)

§ 1. Hume's Philosophical Writings

When, at the age of fifteen, David Hume left the University of Edinburgh, he had become keenly interested in "books of reasoning and philosophy", and, in his early speculations on the nature and certainty of knowledge, he was probably directed largely by the writings of Cicero, Seneca, and Bacon.[2] At the age of twenty-three

[1] But not with the idea of acquiring a knowledge of Psychology. Locke's *method* is all right, but he had no knowledge of practical Physiology, and, as in his time scarcely anything was known about physiological function, he could hardly have been conscious of the necessity of making Physiology the basis of his Psychology. Introspective Psychology has now, for the most part, been thrown overboard, and the newer experimental Psychology is still only in its infancy. It would be absurd now to attempt a study of Psychology unless it were preceded by and based upon a practical knowledge of Physiology. But if we consider the fundamental unit of living matter—the cell—we have to admit that our knowledge of its physiological function is of the slightest, and derived for the most part from mere morphological research. Living matter has no analogue in the inorganic world, and the secrets involved in its actions and activities seem to be insoluble enigmas. To talk of "vital force" existing in protoplasm is a scientific periphrasis whereby we affect to conceal our ignorance It seems, however, to be possible that a further study of the surface tension in cells will throw a good deal of light upon the origin of energy in the human machine. See any recent standard work on Physiology, and cf. Prof. Macallum's Brit. Assocn. Address, 1910.

[2] Cf. Huxley, *Hume*; Hill Burton, *Life of Hume*; Knight, *Hume*; *Ency. Brit.*, "Hume".

he went to France and settled at La Flèche, where he worked up his speculations into systematic form in the *Treatise of Human Nature*, which he published in 1739, a year or two after he had returned to England. In telling the tale of this first venture, Hume said, "never literary attempt was more unfortunate; it fell dead-born from the press". The work undoubtedly failed to do what the author expected from it. His expectations had been great, and his disappointment was therefore keen. In later years he was accustomed to explain his want of success as due to the immature style of his early thoughts and exposition, to the rashness of a young innovator in an old and well-established province of literature. Hume's reference to the *Treatise*, in his preface to the *Inquiry Concerning Human Understanding*,[1] is well known; but none of the principles of the *Treatise* are given up in the later writings, and no addition was made to them. Nor can the superior polish of the more mature productions overbalance the freshness and vigour of the earlier work. Hume is at his best in the *Treatise*.[2]

§ 2. Hume's Scepticism

There are those who maintain that Hume was a sceptic, since he regarded all definite opinion as to "first principles" to be a transgression of the limits of the knowable. "Nothing", he said, "can be more unphilosophical than to be positive or dogmatical on any subject. Where men are most sure and arrogant, they are commonly the most mistaken."[3] But his scepticism was really of the kind which stands apart, and declines to speculate on ultimate problems because of the apparent impossibility of penetrating the haze in which the whole region is enshrouded. Hume did not say that we should never make any definite assertions concerning the truth of things, nor did he make the positive affirmation that we can possess no knowledge of the sphere of reality.

It is the general misconception of the real nature of Hume's scepticism that is the cause of the antipathy felt by many people towards his teaching. His arguments are, however, directed, not against the truths of religion, but against metaphysical speculations. He desires to "confound those dangerous friends or disguised enemies to the Christian religion who have undertaken to defend it by the

[1] Published as Essays in 1749.

[2] Cf. *Ency. Brit.*, "Hume", p. 348.

[3] *Inquiry Concerning the Principles of Morals*, Section IX (this was published in 1751); and cf. *Treatise*, Part IV, § 1.

principles of human reason. Our most holy religion is founded on faith, not on reason." Both Locke and Hume have told us on what questions we must be content to remain in darkness,—all which relate to Metaphysics, all which involve the inner nature of essences and causes; and this metaphysical scepticism is maintained by the great majority of thinking men,—some from conviction, others from a vague sense of the futility of ontological speculation. To say that Hume was wanting in religious conviction is not only unjust, it is untrue.[1]

§ 3. His Method

"To me", says Hume, "it seems evident that the essence of mind being equally unknown to us with that of external bodies, it must be equally impossible to form any notions of its powers and qualities otherwise than from careful and exact experiments, and the observation of those particular effects which result from its different circumstances and situations. And though we must endeavour to render all our principles as universal as possible by explaining all effects from the simplest and fewest causes, 'tis still certain we cannot go beyond experience; and any hypothesis that pretends to discover the ultimate original qualities of human nature, ought at first to be rejected as presumptuous and chimerical."[2]—This is the spirit that prevails throughout Hume's writings. Hume had a true insight into scientific method. He saw clearly that Philosophy ought, in great measure, to be the exponent of the logical consequences of data established on the basis of experience.

§ 4. His Views of the Nature of Mind

Hume was keenly interested in Berkeley's[3] attack on the received notion of "substance". Philosophers had assumed the existence of a *noumenon*[4] underlying all phenomena,—a substratum supporting all qualities. Locke had declared such a substratum to be a necessary *inference* from our knowledge of qualities, but that we must always remain ignorant of its actual nature. Berkeley, however, denied the existence of the substratum entirely, declaring it

[1] Of course Hume was anything but orthodox, though he was most certainly not irreligious. The reader should refer to his *Dialogues on Natural Religion*, bearing in mind that Cleanthes is Hume's mouthpiece. Cf. Lewes, pp. 511-2; Maurice, *Mor. and Met. Phil.*, vol. ii, p. 575; Ueberweg, *Phil.*, vol. ii, pp. 378-9; *Ency. Brit.*, "Hume", p. 355; Knight, pp. 231-2.

[2] Cf. Huxley, *Hume*, p. 55.

[3] Berkeley (1684-1753), Bishop of Cloyne.

[4] Cf. ch. iii, § 2.

to be a mere abstraction. "If", he says, "by matter you understand *that* which is seen, felt, tasted, and touched, then I say matter exists; if, on the contrary, you understand by matter that occult substratum which is *not* seen, *not* felt, *not* tasted, and *not* touched, then I say I believe not in the existence of matter." Berkeley did *not* contradict the evidence of the senses; on the contrary he confined himself exclusively to the evidence of the senses.[1] He could see no necessity for Locke's inference, and got rid of substance ("matter") altogether by identifying "objects" with "ideas".

Locke had shown that all our knowledge was dependent upon experience. Berkeley had pronounced matter to be a mere abstraction. Hume, probing more deeply than Berkeley, decided that not only was matter an abstraction, but mind was no less so. If the occult substratum which men had inferred to explain material phenomena could be denied, because not founded on experience, so also, said Hume, must we deny the occult substratum (mind) which men have inferred to explain mental phenomena. All that we have any experience of are "impressions and ideas". The substance *of* which these are supposed to be impressions, is occult,—is a mere inference; the substance *in* which these impressions are supposed to be, is equally occult,—is a mere inference. In short, mind is but *a succession of impressions and ideas.*[2]

§ 5. His General Theory of the Origin of Knowledge

Hume's general theory of the origin of knowledge it will be best to give in his own words.

"All the perceptions of the human mind resolve themselves into two distinct kinds which I shall call *Impressions* and *Ideas.* The difference betwixt these consists in the degree of force or liveliness with which they strike upon the mind, and make their way into our thought or consciousness. Those perceptions which enter with most force and violence we may name *impressions*; and under this name I comprehend all our sensations, passions and emotions, as they make their first appearance in the mind.[3] By *ideas* I mean the faint images of these in thinking and reasoning.

[1] Cf. Berkeley, *Principles of Human Knowledge*, Sections 35-37, 40. See also "Berkeley and Idealism", *Blackwood's Magazine*, June, 1842; also Reid, *Enquiry*, ch. vi, p. 20; Knight, *Hume*, p. 122; Lewes, pp 490-1 Berkeley used the term *matter* ambiguously, but his intended meaning will have been gathered from what has been said above. He is often ridiculed by those who do not understand him: "And coxcombs vanquish Berkeley with a grin".

[2] Cf. Lewes, *Biog. Hist. Phil.*, p. 507.

[3] Orig., "soul".

"The circumstance that strikes the eye is the great resemblance betwixt our impressions and ideas in every other particular except their degree of force and vivacity. The one seems to be in a manner a reflection of the other.

"Every simple idea has a simple impression which resembles it, and every simple impression a correspondent idea.

"All our simple ideas in their first appearance are derived from simple impressions, which are correspondent to them, and which they exactly represent. The simple impressions always take precedence of their correspondent ideas, but never appear in the contrary order. The constant conjunction of our resembling perceptions is a convincing proof that the one are the causes of the other;[1] and this priority of the impressions is an equal proof that our impressions are the cause of our ideas, not our ideas of our impressions.

"Impressions may be divided into two kinds—those of *Sensation* and those of *Reflection*. An impression first strikes upon the senses. Of this impression there is a copy taken by the mind, which remains after the impression ceases; and this we call an idea. This idea, when it returns, produces new impressions of reflection, because derived from it. These, again, are copied by the memory and imagination and become ideas, which, perhaps, in their turn, give rise to other impressions and ideas. So that the impressions of reflection are posterior to those of sensation, and derived from them."[2]

When complex impressions or complex ideas are reproduced as memories, it is probable that the copies never give all the details of the originals with perfect accuracy, and it is certain that they rarely do so. No one possesses a memory so good, that if he has only once observed a natural object, a second impression does not show him something that he has forgotten. Almost all, if not all, our memories are therefore sketches, rather than portraits, of the originals,—the salient features are obvious, while the subordinate characters are obscure or unrepresented.[3]

[1] Cf. Mill's definition of *cause*.

[2] *Treatise*, Part I, §§ i-iii Cf. Ueberweg, *Phil.*, vol ii, pp. 132-3; and Knight, *Hume*, pp. 135-6. For some very acute, though unconvincing, criticism of Hume's theory, see Green and Grose, Introduction to *Hume*, especially pp. 161-5.

[3] Cf. Huxley, *Hume*, p. 94 Huxley gives in his own words the "broad outlines" of Hume's philosophy. His example of the "flashing red light" leads on to a very lucid explanation of Hume's general position See in particular pp. 68-73. The reader should compare the very divergent views of Huxley and Knight. Huxley is perhaps a little too dogmatic, but Knight's position is frankly that of the "pure metaphysician" with his back to the wall. He says, for instance, "No physiological explanation of mental states and processes is worthy of serious

Hume continues: "Were ideas entirely loose and unconnected, chance alone would join them; and 'tis impossible the same simple ideas should fall regularly into complex ones (as they commonly do) without some *bond of union* among them, some *associating quality* by which one idea naturally introduces another. The qualities from which this association arises, and by which the mind is after this manner conveyed from one idea to another, are three, namely, *Resemblance, Contiguity in Time or Place,* and *Cause and Effect.* Of the three relations, that of Causation is the most extensive."[1]

The general drift of Hume's meaning is perfectly clear, but his loose phraseology—his varying meaning of such terms as "quality" and "relations"—often obscures his arguments and gives many verbal victories to his critics.

§ 6. His Theory of Causation

It is Hume's great effort to prove that the relation of cause and effect is a particular case of the process of association; and no part of Hume's philosophy is more important to the teacher than his theory of causation.

We hear a particular sound succeed the discharge of a gun; and, when we have done so repeatedly, we come to associate the two together; but we are not thus warranted in setting down the firing of the gun as the *cause* of the sound we hear. Again, we take a flower and smell it; the sweet odour we experience we attribute to the flower, but this also is merely due to habit, and the sequence of the pleasant sensation from the proximity of the flower is all that we are warranted in affirming. In these two instances, the senses—of sound and of smell—take note only of antecedence and sequence. Any link of causality or causal connection between the phenomena is not in the objects but in ourselves who subsequently, by dint of habit and association, read into the objects what is not really there. My belief that the sound comes from the gun and the scent from the flower is due merely to the fact that I have had a reiterated and vivid "impression" of the *conjunction* of the two things. It is the

regard in the domain of philosophy"; and again: "To tell us, as the physiologists do, that the brain is the organ of mind, and that molecular changes of the brain always accompany mental acts, is to explain nothing". Let us grant that the case of the physiologist has not been absolutely proved. But what about that of the metaphysician? Whilst there is any amount of available evidence tending to confirm the hypothesis of the physiologists, there is nothing but introspective Psychology to support the metaphysician. It is merely a question, therefore, of balancing probabilities, and which side has the stronger case is fairly obvious.

[1] *Treatise,* Book I, 4. Cf. Huxley, p. 72, &c.

vivacity of the impression, its force and liveliness, that is the sole warrant (according to Hume) for my calling the antecedent—which I have been in the habit of associating with the consequent—a cause, and for naming that consequent its effect.

Hume did not deny that we are in the habit of attributing some kind of causality to the antecedent which produces the consequent. What he denied was that we have any philosophical justification for doing so. Of the supposed "necessary connection" he wished for an explanation. The problem lay in finding a reason for the fact that, given any particular phenomenon, another phenomenon must *of necessity* follow from it. Hume could discover no reason for the existing sequence of events except custom, and therefore no reason except the accident of habit for attributing efficiency to any single phenomenon. For the proposition that "every effect *must* have *some* cause", and that there is therefore a tie of necessity between the sequences of nature, altogether independent of the result that happens to emerge, Hume could see no speculative warrant whatsoever.[1]

"We can never by our utmost scrutiny", says Hume,[2] "discover anything but one object following another, without being able to comprehend any force or power by which the cause operates, or any connection between it and the supposed effect. All events seem entirely loose and separate. One event follows another, but we never can observe any tie between them. They seem *conjoined* but never *connected*. But as we can have no idea of anything which never appeared to our outward sense or inward sentiment, the necessary conclusion seems to be that we have no idea of connection or power at all, and that these words are absolutely without any meaning when employed either in philosophical reasonings or common life.

"The first time a man saw the communication of motion by impulse, as by the shock of two billiard balls, he could not pronounce that the one event was *connected*, but only that it was *conjoined* with the other. After he has observed several instances of this nature he then pronounces them to be *connected*. What alteration has happened to give rise to this new idea of *connection*? Nothing but that he now *feels* these events to be connected in his imagination, and can readily foretell the existence of one from the appearance of the other."

It is, however, very difficult to get rid of the idea from our

[1] Cf. Knight, pp. 150-3. [2] *Inquiry*, § 7.

minds that there is something of the nature of Force or Power or Energy resident in the cause which produces the effect. Hume explains Force and Power as the results of the association, with inanimate causes, of the feelings of endeavour or resistance which we experience when our bodies give rise to or resist motion.

"If I throw a ball, I have a sense of effort which ends when the ball leaves my hand; and if I catch a ball, I have a sense of resistance which comes to an end with the quiescence of the ball. In the former case there is a strong suggestion of something having gone from myself into the ball; in the latter, of something having been received from the ball. Let anyone hold a piece of iron near a strong magnet, and the feeling that the magnet endeavours to pull the iron one way in the same manner as he endeavours to pull it in an opposite direction, is very strong."[1] As Hume says, "No animal can put external bodies in motion without the sentiment of a *nisus*, or endeavour; and every animal has a sentiment or feeling from the stroke or blow of an external object that is in motion. These sensations, which are merely animal, and from which we can, *a priori*, draw no inference, we are apt to transfer to inanimate objects, and to suppose that they have some such feelings whenever they transfer or receive motion."[2] It is obviously as absurd to suppose the sensation of warmth to exist in a fire as it is to imagine that the subjective sensation of effort or resistance in ourselves can be present in external objects, when they stand in the relation of causes to other objects.

To the argument that we have a right to suppose the relation of cause and effect to contain something more than invariable succession, because, when we ourselves act as causes, we are conscious of exerting power, Hume replies that we know nothing of the feeling we call power except as effort or resistance; and that we have not the slightest means of knowing whether it has anything to do with the production of bodily motion or mental changes. And he points out that when voluntary motion takes place, that which we will is not the immediate consequence of the act of volition, but is something which is separated from it by a long chain of causes and effects. If the will is the cause of the movement of a limb, it can be so only in the sense that the guard who gives the order to go on is the cause of the transport of a train

[1] Cf. Huxley, pp. 116-7.

[2] *Inquiry*, Section VII, Part ii, § 60, note (Selby-Bigge's edition, p. 77). Cf. also Ueberweg, *Phil.*, vol. ii, pp. 133-4; and article on "Axiom", *Ency. Brit.*, vol iii, p. 161.

from one station to another.[1] "We learn from anatomy that the immediate object of power in voluntary motion is not the member itself which is moved, but certain muscles and nerves and animal spirits, and perhaps something still more minute and unknown, through which the motion is successively propagated, ere it reach the member itself, whose motion is the immediate object of volition. Can there be a more certain proof that the power by which the whole operation is performed, so far from being directly and fully known by an inward sentiment or consciousness, is to the last degree mysterious and unintelligible?"[2]

Needless to say, Hume meets with a good deal of criticism from the opposite school of thought. Even Lewes expresses disagreement.

Hume's theory, says Lewes, is neither a complete expression of the facts nor a correct analysis of the origin of our belief. When he says that invariable succession of antecedent and consequent is *all* that is given us in our experience of causation, he asserts that which every man who examines the matter attentively may contradict. "Ask yourself whether you have not a sense of *power* also given in the experience of causation. You cannot hesitate. You believe that fire has the *power* to burn your finger, that one billiard ball has the *power* of moving another when impinging on it." "The idea of power may be vague if by idea we understand anything like an *image*, but it is precise enough if we understand by it merely a conception formed by the mind. We cannot, indeed, frame an image of power any more than we can frame an image of mind or of substance; but we have a strong conviction of the existence of them all."[3] Possibly: but proof requires something more than personal conviction.

Professor Knight's criticism is, as might be expected, based upon the supposition that Hume attaches undue importance to knowledge derived from sense experience. "We are told that, in imagining efficiency, or causality, or productiveness (name it as you will) to be lodged within an antecedent, or even within a group of antecedents, as co-operative con-causes, we are the dupes of custom. But we know the cause as *productive* of the effect, or we do not know it at all; and we know the effect as *produced by* the cause, or we do not know *it* at all; and since all phenomena are, alternatively, both

[1] Huxley, p. 127.

[2] *Inquiry*, Selby-Bigge's edition, p. 66. Cf. also Selby-Bigge's *Treatise*, p. 632. See also Maurice, *Mor. and Met. Phil.*, ii, p. 570; Green and Grose, *Treatise*, i, p. 248, &c.

[3] See Lewes, *Biog. Hist. Phil.*, pp. 513–20.

causes and effects, according as we regard them—the cause being just the effect concealed and the effect being merely the cause revealed—we find an interior power or causality *within every link of the chain.* Take any small section of the continuous area of phenomenal succession (for we must remember that the chain is *never* broken), select two or three links. You apply a match to gunpowder, and you see the flame and smoke and hear the sound of an explosion. You perceive a violent change in the position of and the relations of certain particles of matter. The application of the spark to the powder you call the cause; the explosion you name the effect; but there were many things besides the application of the spark that were equally influential in determining the result, and without which the result could not have taken place,—elements, states and conditions, indefinitely necessary, but all concurring and co-operating. And all the result lay potentially within the cause, or the sum of the con-causes: the explosion merely made it visible. It displayed the working of the cause or causes in a certain manner. In other words, the force which separated the atoms formerly slumbered within them. It was latent and it became active. Of course we are not to suppose that there is a non-material entity lying in some sort of crypt amongst the material atoms, alternately caught and released, now passive and again active in its wanderings to and fro; but within *every* atom, as its interior essence, and therefore throughout the whole area of nature, this force or causal power resides. The special point to be noted is that while the *senses* take note of phenomenal succession only, the intellect strikes through the phenomenal chain, and it discerns the inner vinculum, the tie of causality binding antecedent to sequent in the grip of an *a priori* necessity."[1]—This is dogmatism with a vengeance!

Professor James Ward considers that Hume has settled once for all one point in the analysis of the causal relation,—that it does not rest upon or contain any immediate intuition of a causal *nexus.*[2] But the whole subject of causation is so difficult that it is advisable to quote the opinions of several recognized authorities.

[1] Knight, pp. 158–66. Cf Morel, *Phil.*, vol. i, pp. 274–84. On ulterior causes and the Supreme Cause, see Whewell's *Nov. Org. Ren.*, pp 250–6. Cf. also Huxley, pp. 182–96, 120–3

[2] See *Ency. Brit.*, vol. xx, p. 82.

§ 7. Other Views of Causation

1. *Reid.*—We can derive little light from the events which we observe in the course of nature. We perceive changes innumerable in things without us. We know that these changes must be pro duced by the active power of some agent, but we neither perceive the agent nor the power, but the change only. Whether the things be active or merely passive is not easily discovered.

We see an established order in the succession of events, but we see not the bond that connects them together.

Those very philosophers who attribute to matter the power of gravitation, and other active powers, teach us, at the same time, that matter is a substance altogether inert, and merely passive; that gravitation and the other attractive or repulsive powers which they ascribe to it, are not inherent in its nature, but impressed upon it by some external cause, which they do not pretend to know or explain. Now when we find wise men ascribing action and active power to a substance which they expressly teach us to consider as merely passive, and acted upon by some unknown cause, we must conclude that the action or active power ascribed to it are not to be understood strictly, but in some popular sense.

In all languages, action is attributed to many things which all men of common understanding believe to be merely passive. Thus we say the wind blows, the rivers flow, the sea rages, the fire burns, bodies move and impel other bodies.[1]

2. *Hamilton.*—When we are aware of something which begins to be, we are, by the necessity of our intelligence, constrained to believe that it has a cause. But what does the expression, *that it has a cause*, signify? If we analyse our thought we shall find that it simply means, that as we cannot conceive any new existence to commence, therefore, all that now is seen to arise under a new appearance, had previously an existence under a prior form. We are utterly unable to realize in thought the possibility of the complement of existence being either increased or diminished. We are unable, on the one hand to conceive nothing becoming something, or, on the other, something becoming nothing. "*Ex nihilo nihil, in nihilum nil posse reverti*",[2] expresses in its purest form the whole intellectual phenomenon of causality. The mind is compelled to recognize an absolute identity of existence in the effect and in the

[1] Reid, *Active Powers*, Essay I, ch. v, vi, vii (Wright's edition, pp. 98, 103-7).
[2] *Persius*, iii, 84.

complement of its causes,—between the *causatum* and the *causæ*. We think the causes to contain all that is contained in the effect; the effect to contain nothing but what is contained in the causes.[1]

3. *Brown.*—We see in nature one event followed by another. The fall of a spark on gunpowder, for example, followed by the deflagration of the gunpowder; and, by a peculiar tendency of our constitution, we believe that, as long as all the circumstances continue the same, the sequence of events will continue the same; that the deflagration of gunpowder, for example, will be the invariable consequence of the fall of a spark upon it.

There is nothing more, then, understood in the train of events, however regular, than the regular order of antecedents and consequents which compose the train; and between which, if anything else existed, it would itself be a part of the train. All that we mean when we ascribe to one substance a susceptibility of being affected by another substance, is that a certain change will uniformly take place in it when that other is present. All that we mean when we ascribe to one substance a power of affecting another substance, is that, where it is present, a certain change will uniformly take place in that other substance. Power, in short, is significant not of anything different from the invariable antecedent itself, but of the mere invariableness of the order of its appearance in reference to some invariable consequent,—the invariable antecedent being denominated a *cause*, the invariable consequent an *effect*. A cause is, perhaps, not that which has merely once preceded an event; but we give the name to that which has always been followed by a certain event, is followed by a certain event, and according to our belief will continue to be in future followed by that event, as its immediate consequent; and causation, power, or any other synonymous words which we may use, express nothing more than this permanent relation of that which has preceded to that which has followed.[2]

4. *Mach.*—In speaking of cause and effect, we arbitrarily give relief to those elements to whose connection we have to attend in the reproduction of a fact in the respect in which it is important to us. There is no cause or effect in nature; nature simply *is*. Recur-

[1] *Lectures on Metaphysics*, vol. ii, p. 377; *Discussions on Philosophy*, pp. 610, 621, 622. See also pp. 378–413 of former, and pp. 611–633 of latter. For Mill's criticisms of Hamilton's views, see Mill's *Examination* of Hamilton, pp. 344–363.

[2] *Physical Enquiry* (*Phil. of Human Mind*), p. 35. For a criticism of Brown's views, see Professor Wilson in *Blackwood's Magazine*, vol. xl, p. 122, &c.; or Hamilton's *Metaphysics*, vol. ii, pp. 379–94.

rences of like cases in which A is always connected with B, that is, like results under like circumstances, that is again, the essence of the connexion between cause and effect, exist but in the abstraction which we perform for the purpose of mentally reproducing the facts. Let a fact become familiar, and we no longer require this putting into relief of its connecting marks, our attention is no longer attracted to the new and surprising, and we cease to speak of cause and effect. A person of experience regards an event with different eyes from a novice. The new experience is illuminated by a mass of old experience. The notion of the *necessity* of a causal connection is probably created by our voluntary movements in the world, and by the changes which these indirectly produce, as Hume supposed. Cause and effect are things of thought, having an economical office. It cannot be said *why* they arise.[1]

5. *Clifford.*—The word represented by "cause" has sixty-four meanings in Plato and forty-eight in Aristotle. These were men who liked to know as near as might be what they meant, and it would only be the height of presumption in me to attempt to fix the meaning of a word which has been used by so grave authority in so many and various senses; and I shall evade the difficulty by telling you Mr. Grote's opinion.—You come to a scarecrow and ask, what is the cause of this? You find that a man made it to frighten the birds. You go away and say to yourself, "Everything resembles this scarecrow; everything has a purpose". And from that day the word "cause" means for you what Aristotle meant by "final cause". Or you go into a hairdresser's shop, and wonder what turns the wheel to which the rotatory brush is attached. On investigating other parts of the premises, you find a man working away at a handle. Then you go away and say, "Everything is like that wheel. If I investigated enough, I should always find a man at a handle." And the man at the handle, or whatever corresponds to him, is from henceforth known to you as "cause".[2]

When we say that every effect has a cause, we mean that every event is connected with something in a way that might make somebody call that the cause of it. But I, at least, have never yet seen any single meaning of the word that could be fairly applied to the *whole* order of nature.[3]

6. *Herbert Spencer.*—When we inquire what is the meaning of the various effects produced upon the senses, we are compelled to

[1] *Science of Mechanics*, pp. 483–5, 579. [2] Cf. Grote's *Plato*, vol. ii (*Phædo*).
[3] Clifford, *Lectures and Essays*, vol. i, pp. 149–51.

regard them as the effects of some cause. Be the cause we assign what it may, we are obliged to suppose *some* cause. And we are not only obliged to suppose some cause but also a First Cause. Whatever we assume to be the agent producing on us these various impressions must either be the First Cause of them or not. If it is the First Cause the conclusion is reached. If it is not the First Cause, then by implication there must be a cause behind it; which thus becomes the real cause of the effect. We cannot think at all about the impressions which the external world produces on us without thinking of them as caused; and we cannot carry out an inquiry concerning their causation, without inevitably committing ourselves to the hypothesis of a First Cause.

But now if we go a step further, and ask what is the nature of this First Cause, we are driven by an inexorable Logic to certain further conclusions. Is the First Cause finite or infinite? If we say finite, we involve ourselves in a dilemma. To think of the First Cause as finite is to think of it as limited. To think of it as limited necessarily implies a conception of something beyond its limits; it is absolutely impossible to conceive a thing as bounded without conceiving a region surrounding its boundaries. What now must we say of this region? If the First Cause is limited, and there consequently lies something outside of it, this something must have no First Cause—must be uncaused. But if we admit that there can be something uncaused, there is no reason to assume a cause for anything. If beyond that finite region over which the First Cause extends there lies a region which we are compelled to regard as infinite, over which it does not extend—if we admit that there is an infinite uncaused surrounding the finite caused; we tacitly abandon the hypothesis of causation altogether. Thus it is impossible to consider the First Cause as finite. And if it cannot be finite it must be infinite. Another inference concerning the First Cause is equally unavoidable. It must be independent. If it is dependent it cannot be the First Cause; for that must be the First Cause on which it depends.

These are inferences forced upon us by arguments from which there appears no escape. But it might easily be proved that the materials of which the argument is built, equally with the conclusions based on them, are merely symbolic conceptions of the illegitimate order.[1]

7. *Professor Carveth Read.*—Occult causes were regarded as enter-

[1] *First Principles*, pp. 36-46.

ing into the tissue of natural processes, but as essentially unsearchable; and under the name of powers or virtues were the discouragement of induction, the refuge of ignorance, and a fastness of scepticism. Hume's great service was to supersede occult causes by the notion of a sequence of phenomena. His definition of Cause amounts to this, that the cause of a phenomenon is its constant antecedent.[1]

There is not in nature one set of things called causes and another called effects, but everything is both cause of the future and effect of the past; and whether we consider an event as the one or the other, depends upon the direction of our curiosity or interest. Still, taking the event as effect, its cause is the antecedent process; or, taking it as a cause, its effect is the consequent process. This follows from the conception of causation as essentially motion; for that motion takes time is an ultimate intuition. But, for the same reason, there is no interval of time between cause and effect, since all the time is filled up with motion.[2]

8. *Professor Karl Pearson.*—That a certain sequence has occurred and recurred in the past is a matter of experience to which we give expression in the concept *causation*; that it will continue to recur in the future is a matter of belief to which we give expression in the concept *probability*. Science in no case can demonstrate any inherent *necessity* in a sequence nor prove with absolute certainty that it must be repeated. Science for the past is a description; for the future a belief.

Some more or less superficial works on natural science give currency to the notion that mechanics supply a code of rules which nature of inherent necessity obeys. We are told that mechanics is the science of force, that force is the cause that produces or tends to produce change of motion, and that force is inherent in matter. Force thus appears to the popular mind as an agent inherent in unconscious matter producing change.—Mechanics is the science of *motion*.

The whole tendency of modern Physics has been to describe natural phenomena by reducing them to conceptual motions. From these motions we construct the more complex motions by aid of which we describe actual sequences of sense-impressions. But in no single case have we discovered *why* it is that these motions are taking place. Science describes *how* they take place, but the *why* remains a mystery. Science knows nothing of first causes. Causa-

[1] *Metaphysics of Nature*, p. 329. [2] *Logic*, p. 170.

tion, says Mill, is uniform antecedence, and this definition is perfectly in accord with the scientific concept.[1]

9. *Bain.*—The Law of Causation may be expressed thus: In every change there is a uniformity of connection between the antecedents and the consequents.

In Causation, the same cause always produces the same effect; but the converse does not hold; the same effect is not always produced by the same cause. There may be a plurality of causes.—A sufficiently severe blow on a man's head will always cause death; but death is not always caused by a blow on the head.—The fact of plurality renders the causation of an event ambiguous; there may be several alternative antecedents. But plurality of causes is more an incident of our imperfect knowledge than a fact in the nature of things. *As knowledge extends we find less of plurality.*

In common language, the cause of an event is some one circumstance selected from the assemblage of conditions, as being practically the turning-point at the moment. A man slips on a ladder, falls, and is killed. The cause of the fatality is said to be the slipping; for, if this one circumstance had been prevented, the effect would not have happened. Yet, in order to the result, many other conditions were necessary:—the weight of the body (gravity), the height of the position, the fragility of the human frame. Yet, for practical purposes, we leave out of sight at the moment all the elements that are independent of us and secure. By a common ellipsis, all arrangements that are fixed and settled are passed over in silence.

But when in the statement of a cause there is not merely the ellipsis of understood circumstances, but an omission of some essential fact, the consequence is positive error.—When, for example, the healthy effects of residence at a medicinal Spa are attributed exclusively to the operation of the waters, there is a fallacy of causation; the whole circumstances and situation being the cause.[2]

10. *Mill.*—When I speak of the cause of any phenomenon, I do not mean a cause which is not itself a phenomenon. I make no research into the ultimate cause of anything. The causes with which I concern myself are not *efficient* but *physical* causes. They are causes in that sense alone in which one physical fact is said to be the cause of another. Of the efficient causes of phenomena, or whether such causes exist at all, I am not called upon to give an opinion.

[1] *Grammar of Science*, pp. 113, 114, 120, 128, 131.
[2] *Inductive Logic*, pp. 15-20; *Senses and the Intellect*, pp. 428-434.

The Law of Causation, the recognition of which is the main pillar of inductive science, is but the familiar truth that invariability of succession is found by observation to obtain between every fact in nature and some other fact, which has preceded it, independently of all considerations respecting the nature of "things in themselves".

To certain facts, certain facts always do, and, as we believe, will continue to succeed. The invariable antecedent is termed the cause; the invariable consequent the effect.

It is seldom between a consequent and a single antecedent that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents, the concurrence of all of them being requisite to produce, that is to be certain of being followed by, the consequent. In such cases it is very common to single out one only of the antecedents under the denomination of cause, calling the others merely *conditions*. Thus, if a person eats of a particular dish, and dies in consequence, people would be apt to say that eating of that dish was the cause of death. There needs not, however, be any invariable connection between eating of the dish and death; but there certainly is, among the circumstances which took place, some combination or other, in which death is invariably consequent; as, for instance, the act of eating of the dish, combined with a particular bodily constitution, a particular state of present health, and perhaps even a certain state of the atmosphere; the whole of which circumstances perhaps constituted in this particular case the *conditions* of the phenomenon, or, in other words, the set of antecedents which determined it, and but for which it would not have happened. The real cause is the whole of these antecedents; and we have, philosophically speaking, no right to give the name of cause to one of them exclusively of the others. Nothing can better show the absence of any scientific ground for the distinction between the cause of a phenomenon and its conditions than the capricious manner in which we select from among the conditions that which we choose to denominate the cause.

For example, a stone thrown into the water falls to the bottom. What are the conditions of the event? In the first place, there must be a stone and water, and the stone must be thrown into the water. The next condition is, there must be an earth, and accordingly it is often said that the fall of a stone is caused by a force exerted by the earth. It is not, however, enough that the earth should exist, and we have another condition in the fact that the body must be within that distance from the earth, in which

the latter's attraction preponderates over that of any other body. A further condition is that, if the stone is to reach the bottom, its specific gravity must exceed that of the surrounding fluid.—Each and every condition of the phenomenon may be taken in its turn, and, with equal propriety in common parlance, though not in scientific discourse, as if it were the entire cause. And, in practice, that particular condition is usually styled the cause, whose share in the matter is superficially the most conspicuous, or whose requisiteness to the production of the effect we happen to be insisting on at the moment. So great is the force of this last consideration, that it sometimes induces us to give the name of cause even to one of the negative conditions. We say, for example, that the army was surrounded because the sentinel was absent from his post.

It is necessary in our using the word cause that we should believe not only that the antecedent always *has* been followed by the consequent, but that as long as the present constitution of things endures, it always *will* be so. We do not believe, for instance, that night is the cause of day and day the cause of night; for we do not believe that night will be followed by day under all imaginable circumstances, but only that it will be so *provided* the sun rises above the horizon. Therefore, we do not call night the cause or even a condition of day; for day does not follow night independently of the rising of the sun. The succession of day and night is *conditional* on the occurrence of other antecedents. That which will be followed by a given consequent when and only when some third circumstance also exists, is not the cause. Invariable sequence, therefore, is not synonymous with causation, unless the sequence, besides being invariable, is unconditional.

Philosophically speaking, the cause is the sum total of the conditions, positive and negative, taken together; the whole of the contingencies of every description, which being realized, the consequent invariably follows.[1] It is the antecedent, or the concurrence of antecedents, on which the phenomenon is (1) *invariably*, and (2) *unconditionally*, consequent.[2]

If we accept Professor Bosanquet's amendment of Mill's definition and say that, cause is the "*totality* of the conditions", instead of "sum of the conditions", we have a definition generally acceptable to, and accepted by, men of science. As Professor Bosanquet says,

[1] "The negative conditions, however, of any phenomenon, a special enumeration of which would generally be very prolix, may be all summed up under one head, viz., the absence of preventing or counteracting causes." [2] Mill, *Logic*, Book III, ch. v, §§ 3-6.

the word "sum" is unfortunate because it indicates a special way, which may be inappropriate, of combining the factors.[1]

§ 8. Is "Time-sequence" an Element of Causation?

It will be observed that the weight of opinion is all on the side of Mill's conception of the nature of causation. Mill has, it is true, been repeatedly attacked by the rationalist school, but he has, in the long run, invariably proved the victor.[2] It will be sufficient here to touch upon the strictures of Professor Welton.

"It is", says Professor Welton, "the continual endeavour to retain time-sequence at any cost, which vitiates Mill's discussion, an endeavour due to his fundamental position that reality is nothing but phenomena—in the sense of mere transitory sensuous impressions — which are in their nature distinct and separate and only conjoined for consciousness by the operation of psychological association."[3]

Professor Welton himself defines cause as "a totality of conditions whose existence secures the effect". It is not "a phenomenal event in time". "Whenever the cause is present, the effect is present". "Cause and effect are not two but one. In *content* they are absolutely identical." Now let us briefly examine one or two of the illustrations he uses to support his views that time-sequence is not an element of causation.

1. "The weight of the atmosphere", Professor Welton says, "is the cause of the height of the mercury in the barometer, but the two are coexistent."—Let us then suppose that the barometer has been standing steadily for a considerable period at 30 inches. At a given instant, additional pressure makes itself felt. Is the increase in the height of the mercury absolutely simultaneous with the increase of pressure? Is it not rather that the mercury *instantly responds* to the increased pressure?

2. "The cause of the formation of water is the combination in definite proportions of hydrogen and oxygen, but this combination does not precede the formation of water, it *is* that formation." "The combination of hydrogen and oxygen . . . determines that the effect shall be water, but the combined elements and the water

[1] Bosanquet, *Logic*, vol. i, pp 264-5, &c.; vol. ii, pp. 212, 221-5. Of course the term "factors" is used here in a very loose sense. It is interesting to compare Sigwart's opinions with those of Bosanquet. See Sigwart's *Logic*, vol. i, pp. 71, 371; vol. ii, pp. 10, 95-121, 334-61.

[2] See, for example, Mill's *Examination of Hamilton's Philosophy*.

[3] *Logic*, vol. ii, p. 19.

are one and the same identical substance, and this substance is the content both of the cause and of the effect."—Now let us suppose[1] that the molecules of oxygen and hydrogen have been brought into contact under such conditions that they are just about to exchange atoms,—to "combine". Can we conceive the contact and the exchange to take place simultaneously? Is it not rather a question of *successive instants* of time? Does not the act of combination involve a *process*? If so, can a process be absolutely *time-less*?

3. Professor Welton thinks that the plain man would admit that in certain cases (for instance, the barometer) cause and effect are synchronous and not successive, but would point to other cases "in which what he calls the effect is subsequent to that which he calls the cause. Thus, for example, he will say that a man takes poison first, and that death follows at a longer or shorter interval. But here the words 'cause' and 'effect' are used very arbitrarily. By cause is meant the beginning of a chain of subsequent events, and by effect one of those events selected because of its interesting character. But an infinite number of intermediate links can be inserted in the chain, each of which may be equally well regarded as the effect of that which precedes and the cause of that which follows, and thus the 'cause' is, at best, only the remote cause and is separated from its 'effect' by many intermediate proximate causes."—That such a chain must be conceived is obvious. If our knowledge of Physiology was perfect, and we could watch the succession of physiological changes between the time the poison was taken and the time when death took place, we should see that the conception of such a chain was justified. But let us suppose that the poison was taken at 9.0 and that death resulted at 9.30. Granting the chain, how can we totally eliminate time from its successive constituent elements?

It would, of course, be entirely inadmissible to interpose *an interval* of time, however small, between the action of the cause and the production of the immediate effect. As Sir John Herschel says, "In the production of motion by force, though the effect be cumulative, with continued exertion of the cause, yet each elementary or individual action of the force is, to our apprehension, *instanter* accompanied with its corresponding increment of momentum in the body moved".[2]

We may conclude this chapter with some remarks by Mr. F. H.

[1] Accepting the atomic theory, as Professor Welton does. Cf. p. 390.

[2] Herschel, *Essays*, pp. 206-7.

Bradley: "To apprehend causation we must first distinguish the elements before they have come together. And thus we get to perceive what may be called the conditions. But these conditions, when asunder, are not yet the cause. To make the cause they must come together, and their union must set up that process of change which, when fixed artificially, we call the effects."

"Though the effect *succeeds*, it succeeds immediately. Causation is really the ideal reconstruction of a *continuous*[1] process of change in time. Between the coming together of the separate conditions and the beginning of the process, is no halt or interval. Cause and effect are not divided by time in the sense of duration, or lapse, or interspace. They are separated *in* time by an ideal line which we draw across the indivisible process."[2] They are thus *immediately successive* in time, rather than identical, as Professor Welton seems to say.

All the ingenuity of all the metaphysicians has failed to throw any real light on the inner nature of cause. We must, at all events for the present, be content to know *how* things happen; for we cannot find out *why* they happen. Beyond Mill's definition we cannot go. Students of Science, when dealing with causes and effects, must be particularly careful to rid their minds of any lurking notions of concealed "agents" or "powers". Matter contains no hidden demons.

[1] If it were not continuous, we could then take a solid section from the flow of events, solid in the sense of containing no change. But any such section, being divisible, must have duration. But, if so, we should have our cause enduring unchanged through a certain number of moments and then suddenly changing But this is clearly impossible, for what could have altered it? If the cause can endure unchanged, even for ever so short a time, it must endure for ever; it cannot pass into the effect, and therefore is not cause at all. See Prof. Bradley's "Dilemma", *Appearance and Reality*, pp. 60-61.

[2] Bradley, *Principles of Logic*, pp. 485-8.

BOOK II

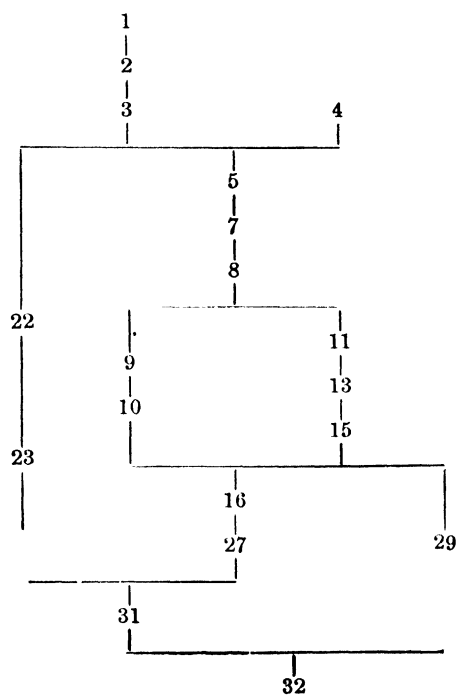
THE LOGIC OF SCIENTIFIC METHOD

CHAPTER XIII

The Function of Logic in Scientific Method

§ 1. Deductive Reasoning: General Notions

The Geometry of Euclid is often referred to as one of the most perfect examples of continued logical development, and if we examine any particular proposition, say the thirty-second of the



first book, it is easy to follow back the chain of reasoning[1] which leads to the conclusion, until we come to the first and fourth propositions, and so at last to the first and tenth axioms on which these propositions respectively depend.[2] If we accept these axioms,[3] we feel bound to accept the whole of the subsequent reasoning, for at each step we are merely bringing a particular case under a generalization, the truth of which we consider to have been established at the previous step. There seems to be no danger, and no difficulty, until we get back to our first assumptions,—the axioms; and the truth of these it is our general practice to accept unquestioned. Whether this is legitimate or not, we shall see presently.

§ 2. Syllogistic Reasoning

Let us examine a simple argument of a rather different kind:—

All schoolmasters are scholars;
Smith is a schoolmaster,
Therefore Smith is a scholar.

The first proposition tells us that schoolmasters form a part, but not the whole, of the class scholars. The fact may be represented by one of Euler's diagrams:—

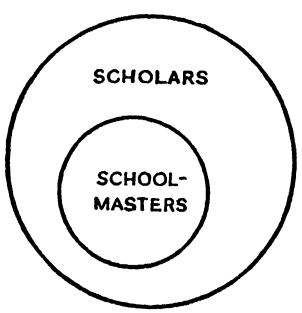


Fig. 2

The small circle is supposed to contain the schoolmasters and nothing else. The large circle is supposed to contain the scholars and nothing else. But as the small circle is wholly within the larger one, it follows that all the schoolmasters must be counted as scholars.

[1] Cf. *Euclid and his Modern Rivals*, by C. L. Dodgson (Lewis Carroll).
[2] Unfortunately Euclid sacrificed his Geometry to his Logic.
[3] The definitions, as distinguished from the axioms, are purposely ignored here; so are the so-called postulates.

It should, however, be noticed that we know nothing at all about that part of the circle of scholars outside the circle of schoolmasters. Bishops, statesmen, and others may be there, but of this possible fact we have no knowledge.

Now if Smith is contained amongst the schoolmasters, he must obviously be contained amongst the scholars, so that the essence of our argument consists in showing that a given particular case falls under a general rule.—This is Formal Logic in a nutshell. The leading idea of the syllogism is the recognition that when any fact is produced as sufficient to prove a conclusion, the sufficiency of such fact for such purpose depends on the acceptance of a generalization which covers it and connects it with the conclusion. We seem almost instinctively to feel that every particular case has a general rule behind it, and therefore that proof consists in finding a general rule to cover the particular case.[1]

The supposition underlying the syllogism of Formal Logic is, then, that every assertion, regarded as disputable, requires for its proof two others related to it in a particular manner and accepted as true; these are known as the major and minor *premisses*. The assertion supposed to be proved by them is called the *conclusion*, and the three assertions together form a *syllogism*.

– Smith is a scholar (conclusion);
– For Smith is a schoolmaster (minor premiss),
– And all schoolmasters are scholars (major premiss).

It will be clear that this process of syllogizing consists in showing the conclusion to be a particular case coming under a general rule.

In the typical syllogism, the statement of the general rule is the major premiss, and the function of the minor premiss is to connect the conclusion with it.[2]

§ 3. The Limited Value of Formal Logic

Nowadays Formal Logic is greatly discredited, and many of our acutest thinkers and ablest reasoners openly scoff at it. Why is

[1] Sidgwick, *Use of Words in Reasoning*, pp. 71–4.

[2] Formal Logic admits that the value of the syllogistic process stands or falls by that of its typical form, which therefore need alone concern us here

Logicians have a convenient custom of typifying arguments by using letters instead of words; for instance. SMP. Thus, the typical syllogism is often written: All M are P; S is M, therefore S is P. (S = subject of conclusion; P = predicate of conclusion; M = middle term, i.e. the common term of the two premisses, connecting them with each other; it does not appear in the conclusion.)

this, seeing that it has been in almost constant use, with very few modifications or additions, ever since the time of Aristotle?

When the logicians of the older school speak of a piece of reasoning as being formally valid, they mean that its validity is determined solely by its *form*, and is in no way dependent upon the particular subject-matter to which it relates. They try to regard the process of reasoning as something distinct from the *subject-matter* about which it is employed, and the errors of reasoning which they contemplate are those only which occur in the process so conceived.[1] Archbishop Whately, for instance, would readily admit the conclusion of the following syllogism, simply because the syllogism is formally valid:—

All good men are wise;
Smith is a good man,
Therefore Smith is wise.

He would maintain that it is no part of the business of Logic to consider the truth of the premisses. He restricts the process of reasoning to syllogizing, or concluding from generals to particulars; and he regards errors in reasoning as simple failures in verbal consistency.[2] It is a remarkable thing that such logicians should overlook the fact that, in order to get an assertion expressed in "logical form", they are usually bound to consider its meaning, and therefore cannot, to this extent at least, escape the necessity of "material"[3] considerations.[4]

Formal Logic commits itself to the assumption that the mere *form* of a sentence is sufficient to bind asserter and defender to a single indisputable meaning. It cannot therefore hope to be of any great practical service in dealing with the chief differences of opinion about matters of fact. For in the vast majority of cases, fallacies and disputes are due to the difficulty of making our meanings clear.[5]

Formal Logic is, in short, merely a Logic of consistency. When we apply Logic to the investigation of objective reality, we are in the domain of *Material* Logic.

Let us consider the syllogism—

All disciplinarians are martinets;
Smith is a disciplinarian,
Therefore Smith is a martinet.

[1] Cf. Sidgwick, *Fallacies*, pp. 18–19.
[2] Cf. Sidgwick, *Use of Words*, pp. 9, 10.
[3] "Material" is opposed to "Formal".
[4] Cf. *Use of Words*, pp. 10, 11.
[5] *ib.* p. 19.

If the truth of the premisses be granted, and if the "middle term" disciplinarian has precisely the same meaning in the two premisses, any schoolboy will draw the correct conclusion, for he has merely to engage in the mechanical operation[1] of fitting a particular case under a general rule. But are we sure of the truth of the statement ("the theory") contained in the major premiss? How do we know that "all disciplinarians are martinets"? Is this a mere assumption, or a carefully established induction? Are we sure of the truth of the "fact" contained in the minor premiss? Have we verified the supposed fact that Smith is a disciplinarian? Do we use the term "disciplinarian" in exactly the same sense in each of our assertions? Do we agree as to the exact significance of the term "martinet"?—*These* are the points about which we are likely to differ in opinion, these the points likely to render our reasoning abortive.

In Formal Logic, the conclusion is always implicitly contained in the premisses, and mere consistency compels assent to the conclusion if the premisses are once admitted. As Dr. Keynes says,[2] "Of context and subject-matter, Formal Logic has no cognizance". And for this very reason, Formal Logic has no appreciable practical value. Its reasoning operations are restricted to the manipulation of ready-prepared material,—sentences in "logical form",—pairs of sentences with a common term (M) which we drop out of account in the "conclusion". It is quite true that syllogizing may prove a very interesting pastime, but we delude ourselves if we think it is likely to be of any real service in the serious business of reasoning.

§ 4. "Forward" and "Reflective" Reasoning

It is necessary to draw a distinction between "Forward" and "Reflective" reasoning, and Mr. Sidgwick makes the distinction clear. "'Forward' reasoning starts from facts accepted as true and asks what unseen conclusion they point to. 'Reflective' reasoning starts from a questioned conclusion, and examines its truth by exploring its grounds."[3]

"Forward" reasoning is the only kind of reasoning which Formal Logic cares about. "Reasoning is thus regarded as a process of building a structure by putting together isolated bricks of thought."

[1] Reference has already been made to Jevons's Logical Machine. Cf. also the "*Dictum de omni et nullo*" of Aristotle. See, for instance, Whately, *Logic*, p. 23.

[2] *Logic*, pp. 1-3.

[3] *Use of Words*, p. 59.

Chief among these are mathematical reasonings—or rather mathematical demonstrations[1]—but these form virtually a class by themselves. There are, it is true, occasionally "other cases where we get a piece of knowledge and then another piece of knowledge, and then suddenly see that these are premisses and yield a logical conclusion";[2] and in Science we often require the forward use of the syllogism for the purpose of deducing conclusions from a hypothesis in order to compare them with facts. But whenever doubt arises as to whether a given conclusion is justified by its premisses, the use of the syllogism has already ceased to be forward deductive reasoning, and become "reflective". Hence syllogistic reasoning as a movement of thought from seen truth to truth not yet seen, applies only to cases which are comparatively free from doubt and difficulty.[3]

In ordinary cases we suspect a truth before we prove it; "our reasonings lag behind our guesses, and are an attempt to review the grounds of belief which has already begun to take shape".[4] The occasions on which we get premisses before we have an inkling of the conclusion are by no means so frequent as Formal Logic commonly supposes.

In reflective reasoning we call for the production of facts sufficient to support the conclusion, the sufficiency being made up partly of the *truth* of the facts produced and partly of their *relevance*. Thus a syllogism used for proof is a conclusion expanded so that the two disputable elements of it shall be open to inspection. In a reflective syllogism, the premisses come out of the conclusion rather than precede it. Of course this view materially alters the notion of *proof* as popularly conceived, for the superstition still prevails that "proof" is bound to take the form of "mathematical demonstration".

The most that any proof can do, in the case of disputed conclusions, is to challenge the objector to find definite fault with the reasons given for belief. If our assertion that "Smith is a scholar" is disputed, we bring forward our reasons: "Smith is a schoolmaster, and all schoolmasters are scholars"; and we challenge a denial of these reasons. Thus the truth of the premisses now becomes the basis of the argument. When, then, we question the truth of a conclusion, we call for facts on which it may rest. After the facts are produced, they may still be open to objection on one of two grounds; we may dispute their truth, or we may dispute their

[1] We shall see later on that analysis plays a far larger share than synthesis in the work of the mathematician.

[2] *Use of Words*, p. 128. [3] *Use of Words*, pp. 286-9, &c. [4] *ib.*

relevance (even if true), to prove the conclusion. And whichever line of objection is adopted, a new question is thereby raised, which takes precedence of the question originally in dispute. Till this new question is answered, the argument is at a standstill.[1]

If we leave the simplest cases of argument out of account, it is easy to see that the whole process, as between two persons arguing, is the search on the part of each for false views held by the other as to the way in which certain things are connected in the ordinary course of nature. The personal aim in every disputed question is to show not only that your opponent's *general* knowledge is somehow defective, but also that in consequence of his ignorance he has reached a false conclusion in this particular case.

A accuses B, in effect, of being misled by appearances, or by words. B accuses A of making too much of some small differences. —How is Logic, with its merely mechanical rules, to decide between them? The argument turns upon the real meaning of the subject-matter, and until agreement on this point is reached, Logic can only stand still and look on. Whenever matters of real doubt and dispute arise, it is one of the hardest things in the world to provide a perfectly unambiguous syllogism that will support either side. So uncertain and treacherous are the words we use.[2]

§ 5. Deduction and Induction

Many logicians separate their books into two portions, called, respectively, deductive and inductive logic, but there is some danger in drawing too sharp a line of distinction between deduction and induction: the two processes are so closely interrelated. Broadly speaking, we may distinguish between them by saying that deduction includes all reasoning in which, from given particulars, we draw a conclusion supposed to be contained in their meaning, while induction includes all reasoning in which we reach a conclusion from observation of facts. Induction is therefore the interpretation of facts, while deduction is the interpretation of sentences assumed to be true.[3]

We shall see later that the great weapon of induction, and therefore of scientific method, is the so-called "method of difference"; but just as the "formal logician"[4] pays almost exclusive attention to the mere machinery of the syllogism, so he seems to think that th·

·he

[1] Cf. *Use of Words*, pp. 59–61. [2] Cf. *Use of Words*, pp. 79–85. [3] *ib.* p

[4] This unfortunate term is now in rather common use.

virtue of the "method of difference" lies in the method itself, instead of in the care and knowledge with which the material for the application of the method has been previously prepared. The very best rules of inductive logic may lead us astray. Men of science of one generation have often been at the utmost pains to establish an important induction by a rigorous application of accepted methods to such facts as were known, but the next generation has discovered the complexity of some circumstance which was supposed formerly to be simple, and the old induction has in consequence broken down. The important thing is to recognize that Science rests on a limited but constantly increasing knowledge of facts. Inquiry is never finished. Science is ever reviewing its facts and theories in the constant effort to make them harmonize, for there is always the possibility of error being concealed in accepted truths; imperfections are therefore constantly sought, and, when discovered, are removed. While, therefore, induction is the great instrument of scientific research, we must concentrate our attention upon the preparation of the materials rather than upon the inductive process itself, and we must always be ready to revise an induction in the light of new facts. It must always be remembered that inductive *methods* are of use rather in a Logic of *Proof* than in a Logic of *Discovery*.[1]

§ 6. Some Common Logical Terms

There are in common use certain logical terms which sometimes appear to be wanting in definite connotation. To these we must briefly refer.

"Inference" is a very ambiguous word. "When we *infer* one fact from another or others, we believe that fact 'by reason of' our belief in those others; and when we *prove* one fact by means of another, exactly the same expression is commonly used. In both cases there is 'reasoning', and, accordingly, both that from which the inference is drawn and that on which the proof is based, are indiscriminately called, in popular language, the *reason*. We reason when we proceed from premisses to conclusion, arriving at new truths by means of old ones; and we reason when, having already an assertion before us, we produce arguments to support it, even if such arguments be then for the first time thought of. Again, the

[1] V.

the mat. *Use of Words*, pp. 27-30, 321-30; and W. L. Courtney, *Life of John Stuart Mill*,

[2] *Use* VI.

term 'premisses' is sometimes used for the *grounds of proof*, and sometimes for the *data of inference*; 'conclusion' sometimes means that which is *discovered* and sometimes that which is *proved*."

Inferences are of very varying degree. They may be merely our first vague guesses; they may be the final and certain results of the most careful enquiry.

It would be convenient to restrict the term Inference to the process of reaching a belief, and to speak of a Conclusion following from its "premisses" or "data"; and to regard Proof as the process of establishing a belief on a firm foundation after it is already somehow reached. Thus, in the case of Proof we should speak of an assertion "guaranteed by" its "reasons", or "resting upon" its "grounds". The problem of Proof is thus narrower and more definite than that of Inference. Instead of asking at large, "*what* conclusion may be drawn?" Proof asks, "Is such and such a reason warranted?"[1]

It is evidently immaterial to an argument whether the conclusion is placed first or last. But a premiss placed *after* its conclusion is usually called the *reason* of it, and is introduced by a causal conjunction (*since*, *because*, &c.). The illative adverbs (*therefore*, &c.) designate the conclusion.

Perplexity often arises from the fact that these conjunctions and adverbs have also another signification, being employed to denote, respectively, *cause* and *effect*, as well as *premisses* and *conclusion*. For example:—

(1) The soil is rich *because* the trees on it are flourishing;
or (2) The trees are flourishing and *therefore* the soil must be rich.

In both examples the italicized words denote the connection between *premisses* and *conclusion*; for clearly the luxuriance of the trees is not the cause of the soil's fertility but only the cause of *my knowing* it. But if I say:—

(1) The trees flourish *because* the soil is rich;
or (2) The soil is rich and *therefore* the trees flourish;

I use the same words[2] to denote the connection of *cause* and *effect*, for in this case the luxuriance of the trees, being evident to the eye, would hardly need to be *proved*, but might need to be accounted for.

In some cases the cause is employed to *prove* the existence of the

[1] Sidgwick, *Fallacies*, pp. 32–5. [2] i.e. *because* and *therefore*.

effect. For instance, when from favourable weather anyone argues that the crops are likely to be abundant, the *cause* and the *reason* coincide. And this contributes to their often being confounded together in other cases.[1]

The reader should spare no pains in acquiring an accurate knowledge of common terms used in the process of argument. The word *why*, for instance, as an interrogative, is employed in three senses, viz., "By what proof?" (or reason); "From what cause?"; "For what purpose?"—"Why is the triangle ABC equal to the triangle DEF?" "Why does a stone fall to the earth?" "Why did you go to London?"[2]

§ 7. Conclusions as to the Value of Logic

Logic is of little use for the purpose of enabling us to reason; it rather enables us to know whether in a given case we *have reasoned* correctly, or at least to discover where the weak point in our reasoning must lie. Logic does not discover, but it tests discoveries which claim to be already made.[3] It is also a useful instrument for combating fallacy and sophism.[4]

Whether students of Science can profitably spend much time in the study of deductive logic is open to serious doubt. Obviously the mathematician will feel no such need, although it certainly has to be remembered that Mathematics is an extremely abstract branch of reasoning. The "pure" mathematician may possibly be less apt in detecting fallacy than the trained logician, for the latter is the more accustomed to deal with "facts" that are not certainties. But the study of induction is quite another matter. No student of Science can afford to ignore it, or even to be content with a superficial knowledge of it; and his wisest course, perhaps, is to begin with some elementary treatise on deduction, and follow that up by wide reading of inductive logic, and perhaps by some standard work on the theory of knowledge. The essentials of induction are, however, dealt with in the next few chapters.

As Herbert Spencer and others remind us, the very first condition for avoiding fallacy is a calmness which is ready to recognize

[1] Whately, *Logic*, p. 18, and compare his *Rhetoric*, pp. 53–7.
[2] *ib.* p. 230. The whole chapter on ambiguous terms is well worth reading.
[3] *Fallacies*, pp 18–20.
[4] "Fallacy is honest error; sophism is intentional deception." Syllogisms involving "fallacies" have been manufactured by all logicians from Aristotle downwards, and are the examiners' stock-in-trade. A few instances are given at the end of the chapter.

or to infer one truth as readily as another. If we make sure of our facts, if we can agree upon the precise significance of the general and abstract terms we use, if we can conduct our arguments frankly and dispassionately, we need have little fear of the reasoning process, and can well afford to give Formal Logic a long holiday.[1]

CHAPTER XIV

The Methodologists

We shall frequently have occasion to refer, in the next few chapters, to various authorities on the logic and the method of Science. Brief personal reference may here be made to the following writers who hold first place amongst such authorities.

§ 1. Whewell

William Whewell (1794–1866) was the son of a Lancaster carpenter. He obtained a local Exhibition which enabled him to proceed to Trinity College, Cambridge, in 1812. He graduated as second Wrangler in 1816, was elected Fellow in 1817, and in 1841 was appointed Master of the College. Of his numerous works, the chief are the *Philosophy*, and the *History of the Inductive Sciences*, and the *Novum Organon Renovatum*.[2] Whewell's wide acquaintance with the different branches of Science enabled him to write a comprehensive account of their development, an account which, in fact, has never been superseded.

[1] The reader will find examples of syllogisms involving "fallacies" in almost any textbook on Formal Logic. Here are a few, chosen at random:—

1. All fixed stars twinkle; yonder star twinkles; therefore it is fixed. (Bain.)
2. You are not what I am; I am a man; therefore you are not a man. (Port Royal Logic.)
3. All birds are animals with feathers; but all birds are animals with a heart; therefore all animals with a heart are animals with feathers. (Hamilton.)
4. He who says that you are an animal speaks truly; he who says that you are a goose says that you are an animal; therefore he who says that you are a goose speaks truly. (Port Royal.)

The detection of the fallacies is left as an exercise for the reader. (It may be mentioned that ambiguity of the Middle Term is recognized generally as the most fruitful source of fallacy.)

[2] Aristotle wrote the *Organon*, Bacon the *Novum Organon*, and Whewell the *Novum Organon Renovatum*.

Whewell's philosophy of Science was opposed to the so-called "empiricist tendency" prevalent among many English thinkers. He maintained the distinction between necessary and contingent truths,—the former involved in the innate constitution of the mind, the latter coming from experience. On this and other points he had a sharp controversy with Mill. He defended the *a priori* necessity of axioms attacked by the latter, and in his inductive theory attributed more importance to the function of the mental idea in the colligation of facts than Mill did.

§ 2. Mill

John Stuart Mill (1806–1873) was the son of James Mill the historian and political philosopher. The son's education was from first to last undertaken by the father, and is likely long to remain a standing subject for wonder and discussion.[1] From very early childhood his greatest pleasure seemed to consist in overcoming intellectual difficulties, and the wonderful mastery which, as a boy, he acquired in getting at the exact meaning of general terms, seems to account for the singular and quite unparalleled ease with which he treated of politics and sociology, always in close relation with facts. Mill's knowledge seems to have been encyclopædic.

It was in 1837, on reading Whewell's *Inductive Sciences*, and rereading Herschel, that Mill at last saw his way clear to formulating the methods of scientific investigation. His great work, *Logic*, was regarded as epoch-making, from the multitude of new views opened up. Mill has been described as the father of Induction, just as Aristotle is sometimes called the father of Deduction. He has been assailed by many critics, but although a few unimportant outworks have been taken, his main position is as secure as ever.

§ 3. Herschel

Sir John Herschel (1792–1871) was the son of Sir William Herschel. Both father and son were famous astronomers. The son graduated as Senior Wrangler in 1813. His book, *Discourse on Natural Philosophy*, though of very modest proportions, is one of the best treatises on scientific method ever written.

[1] The reader should turn to Mill's *Autobiography*, a delightfully interesting book.

§ 4. Bain

Alexander Bain (1818–1903) was for many years Professor of Logic at the University of Aberdeen, and was famous as a Logician and as a Psychologist. Bain and Mill were lifelong friends, and the two had much in common. Bain's *Inductive Logic* is a well-known standard work.

§ 5. Jevons

William Stanley Jevons (1835–1882) graduated at the University of London, and in 1866 was elected "Professor of Logic and Mental and Moral Philosophy and Cobden Professor of Political Economy" at Owens College. He felt the absurdity at having to deal with so many branches of knowledge, and was glad to exchange in 1876 for the professorship of Political Economy at University College, London. Although Political Economy appears to have been his principal subject, he had in his early days been greatly interested in Science, and he gave special consideration to the logic of inductive science. His *Principles of Science* is his important work, and we shall have to refer to it frequently. Jevons's views of induction were very similar to those of Whewell, and, like Whewell, he attacked Mill. His life was prematurely cut short by drowning at Hastings.

§ 6. Professor Welton

Professor Welton occupies the Chair of Education at the University of Leeds. The second volume of his *Manual of Logic* deals at length with the various logical aspects of the method of Science.

Although Professor Welton's dearest wish seems to be to consign, with bell, book, and candle, the "mere" empiricist to utter darkness, the mere empiricist will gain much by reading the second volume of the *Logic*, which is a mine of useful hints to those who wish to master scientific method.

§ 7. Mr Alfred Sidgwick

The present writer owes a great deal to the various works of Mr. Alfred Sidgwick, perhaps the best known among modern logicians to show conclusively how extremely limited is the value of Formal Logic. His works include *Fallacies*, *The Process of Argument*, *The Use of Words in Reasoning*, and *The Application of Logic*.

CHAPTER XV

Induction

§ 1. General Notions of Induction

Induction has been defined as the legitimate inference of propositions applicable to cases hitherto unobserved and unexamined, from propositions which are known to be true of the cases observed and examined. In every argument it is implied that, wherever and whenever the same circumstances are repeated, the same effects will follow. Induction, therefore, may also be defined as *the legitimate inference of the general from the particular, or of the more general from the less general.*[1]

To illustrate this definition, Fowler makes use of the well-known "guinea and feather" experiment.[2]—A guinea and a feather are placed at the same height under the exhausted receiver of an air-pump. When released they are observed to reach the bottom of the vessel at the same instant of time, or, in other words, to fall in equal times.

From this fact, we *infer* that a repetition of the experiment, either with the same two bodies or with any other bodies, would be attended with the same result, and that, if it were not for the resistance of the atmosphere and other impeding circumstances, all bodies, whatever their weight, would fall through equal vertical spaces in equal times.

Now here we have performed an *experiment*,—we have arranged that, for purposes of *observation*, a certain thing shall happen under certain conditions; and we have drawn an *inference*.—What assumptions underlie this inference, and on what grounds does it rest?

Clearly there was some definite object in working the experiments. The question had previously been asked whether bodies, if subject to the action of gravity alone, would fall in equal or unequal times. By exhausting the air in the receiver, it was possible *to isolate the phenomenon*, all circumstances affecting the bodies, except the action of gravity, being thus removed; the effect of this cause acting alone could then be watched.

We are, however, assuming that the *effect*, whatever it may be, will be entirely due to the *cause* (or causes) then and there in action.

[1] Fowler, *Inductive Logic*, pp. 9-10. [2] *ib.*

In other words, we are assuming that nothing can happen without a cause.[1]

But why do we infer that if the experiment be repeated, the same two bodies, or any other bodies, will behave in the same way? Because we feel assured that the same cause will invariably be followed by the same effect, or, to speak more accurately, that the same cause or combination of causes will, if unimpeded by the action of any other cause or combination of causes, be invariably followed by the same effect or combination of effects.—We assume the "uniformity of nature".[2]

Our argument, then, turns upon the truth of two important assumptions, viz. the *Law of Universal Causation* and the *Law of the Uniformity of Nature.*

The general argument may be summarized as follows:—

1. We observe that the two bodies, though of unequal weight, reach the bottom of the receiver at the same moment.

2. The fact must be due to some *cause*, or combination of causes.[3]

3. The only cause operating in this instance is the action of gravity.[4]

4. Therefore the fact that these two bodies reach the bottom of the receiver at the same moment is due to the action of gravity operating alone.

5. But whenever the same cause, or combination of causes, is in operation, and that only, the same effect will invariably follow.[5]

6. Therefore when these two bodies, or any other two or more bodies, even though of unequal weight, are subject to the action of gravity only, they will reach the bottom of the receiver at the same moment, or, in other words, will fall in equal times.[6]

It is exceedingly important to notice exactly how in this way we are able, if we assume the truth of the Laws of Universal Causation and the Uniformity of Nature, to make a great generalization from a single experiment. Unfortunately, however, it is very seldom that we can eliminate all operating causes save one, and induction is generally far more subtle and difficult than this easy example seems to suggest.

There are other so-called inductions which are not really induc-

[1] [2] These two assumptions will be considered presently.

[3] Law of Universal Causation.

[4] Of course we have no knowledge of the nature of gravity, which *may* be a complex, and not a simple, phenomenon. It is possible, therefore, that if and when we discover the nature of gravity, we shall have to revise many of our scientific conceptions.

[5] Law of Uniformity of Nature. [6] Fowler, *Inductive Logic*, pp. 1-7.

tions at all, though the opinion commonly prevails that induction is in some way connected with the collection or counting of a large number of instances. Suppose, for example, we wish to establish the fact that every month contains more than twenty-seven days. We simply examine an almanac, get at the actual fact for every month, and make a complete record. In such a case there is no *inference* of any sort or kind, and no induction.

But suppose we make the statement that "all swans are white". We may have examined ten, perhaps a hundred, perhaps a thousand, instances, but, even so, we are not justified in making any such generalization. It would at the best be only a probability, for we are not acquainted with any *causal connection* between a swan and a particular colour of plumage. And the first black swan we saw or knew to exist would, of course, show the generalization to be false.

Take another instance. Gold is known to have a definite specific gravity and to melt at a certain temperature, but these are merely coexisting facts, which may *possibly* be due to some causal connection; but, if so, such connection is at present entirely unknown to us. Such facts of coexistence are arrived at by "simple enumeration". There is no inference, no real induction at all.

Yet inductions of this class—inductions of simple enumeration —really include the Laws of the Uniformity of Nature and Causation, as well as the axioms of Mathematics and such facts of coexistence as that already referred to. We shall have to refer to this point again.

So much for induction in its broader and simpler aspects.

§ 2. The Guiding Principles of Bacon, Newton, and Herschel

It will be remembered that *Bacon's* great merit was his insistence on the necessity of basing all generalizations on a patient collection, classification, and comparison of facts. He did not, however, provide us with inductive machinery of any appreciable value.

Newton, like Bacon, made little by way of direct contribution to the methods either of Discovery or Proof, but he set an example of scrupulously careful and cautious inquiry, and raised the standard of proof enormously. His "Rules of Philosophising" were long quoted as authoritative.[1]

[1] For instance: (1) "Only real causes (*veræ causæ*, actually existing causes) are to be admitted in explanation of phenomena"; (2) "No more causes are to be admitted than

Herschel insists that experience is our sole source of knowledge. He urges the importance of recording observations with numerical precision, dwells upon the value of classification, and gives several rules which are useful aids to Discovery.[1]

§ 3. Whewell's "Colligation of Facts" and "Explication of Conceptions"

But it is not until we come to Whewell that we find a method worked out in detail. His *Novum Organon*[2] *Renovatum* claims to be "a revision and improvement of the methods by which Science must rise and grow".

Whewell considered that the great problem of Science is to "superinduce" Ideas or Conceptions[3] upon Facts. The business of the discoverer is, first, to familiarize himself with facts, then to compare them with conception after conception, in order to find out after a longer or shorter process of trial and rejection, what conception is (1) clear and distinct, and (2) exactly "appropriate" to the facts under consideration. When the investigator has at length, by a happy guess, hit upon the appropriate conception, he is said to "colligate" the facts,—to "bind them into a unity".

Throughout Whewell's scheme there is a sharp antithesis between Ideas or Conceptions, and Facts. With him generalization consists not in evolving notions from a comparison of facts, but in "superinducing" upon facts conceptions supplied by the mind. The particular facts are not merely brought together, but there is a *new element* added to the combination by the very act of thought by which they are combined. There is a conception of the mind introduced, which did not exist in any of the observed facts.[4]

Let us take one of Whewell's examples with which he illustrates his arguments.—Why do we infer that the earth is of globular form?

Our chief *facts* are these: (1) As we travel to the north, we find that the apparent pole of the heavenly motions, and the constella-

such as suffice to explain the phenomena". In other words, when one cause is proved to be present in sufficient amount for the effect, we are not at liberty to suppose the presence of other causes. (Cf. the maxim known as Occam's razor: "*Entia non sunt multiplicanda praeter necessitatem*".) (3) "In as far as possible, the same causes are to be assigned for the same kind of natural effects"—For instance, the respiration in man and beasts; the fall of stones in Europe and in America.

[1] One such rule recommends the tabulation of facts "in the order of intensity in which some peculiar quality subsists". See *Phil. of Disc.*, Part II, ch vi; and cf. Bain, pp. 403–11.

[2] Whewell used the Greek word, Bacon the Latin (*Organum*).

[3] Ideas—"the higher generalities"; Conceptions—"the lower generalities".

[4] *Nov. Org. Ren.*, pp. 72–4, 106–7. Cf. Bain, pp. 411–2.

tions which are near it, seem to mount higher; and as we proceed southwards, they descend. (2) If we proceed from two different points, considerably to the east and west of each other, and travel directly northwards from each, as from the south of Spain to the north of Scotland, and from Greece to Scandinavia, these two north-and-south lines will be much nearer to each other in their northern than in their southern parts.

These facts, namely, the visible descent of the North pole of the heavens as we travel south, and the convergence of the meridians to the north, are *seen to be consistent* with the supposition that the surface of the earth is convex, and with no other supposition. *If* the earth be supposed globular, the facts at first brought forward are at once seen to be mere consequences. And the supposition is further confirmed by observing that the boundary of the earth's shadow upon the moon is always circular.[1]

Upon the actual facts, then, we *superinduce the conception* that the earth is globular in form. Or we may say that we have conceived a new and general proposition which *includes* the more particular ones. But these particulars constitute the general truth, not by being merely enumerated and added together, but by being seen *in a new light.* The inductive truth is made into something more than the sum of the facts by the introduction of a new mental element; and the mind, in order to be able to supply this element, must already be stored with appropriate knowledge. In order, for instance, that an investigator may see that a convex surface of the earth necessarily follows from the facts above brought forward, he must have a sound knowledge of Geometry, especially the geometry of the sphere. The conception that the earth is globular would never occur to an investigator ignorant of Mathematics.[2]

We see, then, that Whewell considered the inductive step to consist in the suggestion of a new conception for binding the facts together. But precisely how such conceptions really originate, he does not clearly say. He speaks of them as being gradually worked out by the discussions and reflections of successive speakers, a view

[1] It being supposed to be already established that the moon receives her light from the sun, and that lunar eclipses are caused by the interposition of the earth.

The further illustration of the fact that the earth is globular, often given, viz. that the horizon is always circular, is a little dangerous. The eye may be misled by mere perspective effects.

[2] To "explain" to children, who are entirely ignorant of the geometry of the sphere, that the earth is globular in form is a mere waste of words.

not inconsistent with their gradual development from the comparison of particulars. But he says also that they are supplied by the mind, while facts are supplied by sense; and he seems tacitly to assume that the mind is a sort of storehouse of conceptions accumulated there independently of the experience of particulars.[1]

§ 4. Mill's Views of Induction

Mill defines induction as that operation of the mind by which we infer that what we know to be true in a particular case or cases will be true in all cases which resemble the former in certain assignable respects. In other words, induction is the process by which we conclude that what is true of certain individuals of a class is true of the whole class, or that which is true at certain times will be true in similar circumstances at all times.

Induction is thus a process of inference; it proceeds from the known to the unknown; and any operation involving no inference, any process in which what seems the conclusion is no wider than the premisses from which it is drawn, does not fall within the meaning of the term.

Such a definition therefore excludes the so-called "perfect" inductions, or inductions of "simple enumeration". If, for instance, we were to say, All the planets shine by the sun's light, from observation of each separate planet; or, all the Apostles were Jews, because this is true of Peter and Paul and every other Apostle; these, and such as these, would, in mediæval phrascology, be called "perfect", and the only perfect, inductions. But such an induction is not an inference from facts known to facts unknown, but a mere shorthand registration of facts known. The two simulated arguments quoted are not generalizations; the propositions purporting to be conclusions from them are not really general propositions. "A general proposition is one in which the predicate is affirmed or denied of an unlimited number of individuals, viz. all, whether few or many, existing or capable of existing, which possess the properties connected by the subject of the proposition." "All men are mortal" does not mean merely all now living, but all men past, present, and to come.

In short, Mill regards induction as "the operation of discovering and proving general propositions".[2]

[1] Cf. Bain, p. 412.

[2] Mill, *Logic*, Book III, ch. ii. Cf. Aristotle's views (see ch. vii, § 4).

§ 5. How Mill Differs from Whewell

Mill's view of induction differs materially from Whewell's. The difference will be best understood by referring to Kepler's "First Law".

The ancients noticed, just as untrained observers now notice, that the stars maintained an apparently constant position on the uniformly rotating star-sphere, but amongst them was to be seen a number of other bodies (which they called planets) moving in paths made up of a series of successive loops. They, very naturally perhaps, regarded the earth as the common centre of both solar and planetary orbits, the looped paths of the planets being explained by supposing each planet to travel, in a circle, round a centre which itself travelled, in a circle, round the earth. In other words the path of each planet was an *epicycle*[1]. Copernicus[2] was not satisfied with this old geocentric theory, and he conceived the sun, instead of the earth, to occupy the centre of the solar system, a conception which much simplified the real planetary movements. But the old axiom that the celestial motions must be *circular* and *uniform* appeared to Copernicus to have strong claims to acceptance, and he therefore felt bound still to regard the planetary paths as epicyclic. But Kepler[3], the disciple of Tycho Brahe,[4] was convinced that the theory of epicycles was wrong, and, making use of his master's great mass of accurate observations of the orbit of Mars, set to work to discover the truth. On a firm basis of actual facts of observation, he constructed hypothesis after hypothesis, all modifications of the old theory of epicycles, till he was finally led to change the epicyclical into an *elliptical* theory.[5] The failure of many of his earlier hypotheses was due to his acceptance of the old notion that the path of a planet was a perfect circle. Altogether he made no less than nineteen hypotheses with regard to the motion of Mars, and with enormous labour calculated the results of each, before he finally established the fact that the planet's path is an ellipse. It must not, however, be thought that Kepler made any serious alterations of relations which occurred in the first hypothesis. His elliptical theory of Mars' motion involved relations of lines and angles much of the same nature as all his previous false hypotheses.[6]

[1] The reader should construct an epicycle for himself. The epicycle describes, with approximate correctness, the apparent motion of a planet when the earth is assumed as *fixed*.

[2] 1473-1543. [3] 1571-1630. [4] 1546-1601.

[5] Cf. Whewell, *Hist. Ind. Sci*, vol. i, pp. 316-26.

[6] *Nov. Org. Ren.*, pp. 65-6. Cf. the section on the "Method of Curves", ch. xxv, § 10. The non-mathematical reader may hardly appreciate the difficulties encountered when

Before examining this example as a possible instance of induction, Mill gives what he considers to be an analogous case:—

A navigator sailing in the midst of the ocean discovers land; he cannot, at first, or by any one observation, determine whether it is a continent or an island; but he coasts along it, and, after a few days, finds that he has sailed completely round it. He then pronounces it an island. Now there was no particular time or place of observation at which he could perceive that this land was entirely surrounded by water; he ascertained the fact by a succession of partial observations, and then selected a general expression which summed up in two or three words the whole of what he so observed. But there is nothing of the nature of an induction in this process. He inferred nothing that had not been observed, from something else which had. He had observed the whole of what the proposition asserts. That the land in question is an island is not an inference from the partial facts which the navigator saw in the course of his circumnavigation; it is the facts themselves; it is a summary of those facts; the description of a complex fact, to which those simpler ones are the parts of a whole.

Now Whewell maintained that Kepler had established *by induction* the fact that Mars' orbit is an ellipse. But Mill urged that there was no difference in kind between the navigator's simple operation and that by which Kepler ascertained the nature of the planetary orbit; that Kepler's operation, or at least the characteristic part of it, was not more an inductive act than that of our supposed navigator.

Kepler's object was to determine the real path described by the planet Mars. To do this there was no other mode than that of direct observation; and all that observation could do was to ascertain a great number of successive places of the planet, or rather of its apparent places. That the planet occupied successively all these positions, and that it passed from one of them to another without any apparent breach of continuity, thus much the senses could ascertain. What Kepler did more than this was to find what sort of a curve would result, supposing a line drawn through all these

the heliocentric theory was first suggested. If we could watch the motions of the planets from some point right outside the solar system, we might determine their orbits as easily as we do those of Jupiter's satellites. Even if we could observe them from the sun, our task would be comparatively easy. But our observations have to be made from one of the moving planets themselves; and the consequent apparent motions of the other planets are so complicated that the real motions are exceedingly difficult to determine exactly. The apparent motion of, for instance, Mars amongst the stars is far more suggestive of an epicycle than an ellipse.

points. He unified the whole series of observed positions of Mars by what Whewell calls the "general conception" of an ellipse.[1] This operation, though much more difficult than our supposed navigator's, is, Mill says, the very same sort of operation; and if the one is not an induction but a description, this must also be true of the other. Kepler merely found an expression for a set of facts; he made no inference. Nor did he (which is the true test of a general truth) add anything to the power of prediction already possessed. Astronomers had long known that the planets periodically returned to the same places.—Thus Mill argues.

Mill agrees that Whewell's expression "colligation of facts" is aptly chosen for the descriptive operation which enables a number of details to be summed up in a single proposition. But he denies that such colligation is induction at all. He also agrees that, for such descriptive operations, a conception of the mind is required; the conception of an ellipse must have presented itself to Kepler's mind before he could identify with it the orbit of Mars. According to Whewell, the conception was something added to the facts. But Kepler did not, says Mill, put something into the facts by his mode of conceiving them. The ellipse was in the facts before Kepler recognized it, just as the island was an island before it had been sailed round. Kepler did not *put* what he had conceived into the facts, but *saw* it in them. If the elliptic path were visible, no one, Mill thinks, would dispute that to identify it with an ellipse is to describe it; and Mill cannot see why any difference should be made by its not being directly an object of sense, when every point in it is as exactly ascertained as if it were so.

Mill admits that Whewell's account of the manner in which a conception is selected to express the facts is probably just; and believes that the experience of all thinkers will testify that the process is tentative; that it consists of a succession of guesses, many being rejected, until at last one occurs fit to be chosen. Successive expressions for the colligations of observed facts, or, in other words, successive descriptions of a phenomenon as a whole, which has been observed only in parts, may, though conflicting, be all correct as far as they go. "But it would surely be absurd to assert this of conflicting inductions." "Different descriptions may be all true,

[1] The reader should bear in mind that, in textbooks on Astronomy, the illustrations of the planetary orbits are, as regards eccentricity, grossly exaggerated. If, for instance, the orbit of the earth be accurately drawn, it is quite impossible for the untrained eye to see that it is not a perfect circle.

but not, surely, different explanations." "Colligation is not always induction, but induction is always colligation."

Whewell, in reply, denied that there was any validity discoverable in the distinction which Mill attempts to draw between descriptions like Kepler's Law of Elliptical Orbits, and other examples of induction.

But Mill again insisted that such distinction is necessary. Dr. Whewell "allows of no logical process in any case of induction other than what there was in Kepler's case, namely, guessing until a guess is found which tallies with the facts; he considers the process of invention, which consists in framing a new conception consistent with the facts, to be not merely a necessary part of all induction, but the whole of it". But, says Mill, "induction is generalization from experience. It consists in inferring from some individual instances in which a phenomenon is observed to occur, that it occurs in all instances of a certain class, namely, in all which *resemble* the former, in what are regarded as the material circumstances."[1]

The real difference between Mill and Whewell is mainly one of definition, though of course there is the further fundamental difference of philosophic faith. To the practical man the difference is of no particular consequence.[2]

§ 6. Jevons's Views

Jevons defines induction as the inference of general from particular truths, and regards the process as the inverse operation of deduction. He admits, however, that the inverse operation is incomparably more difficult than the direct, just as integration is more difficult than differentiation, or just as finding the factors of a given large number is more difficult than finding the product of such factors. Exactly the same difficulty exists in determining the law which certain things "obey". Given a general mathematical expression, we can easily ascertain its value for any required value of the variable, but given a series of numbers like the following,

$$\frac{1}{6},\ \frac{1}{30},\ \frac{1}{42},\ \frac{1}{30},\ \frac{5}{66},\ \frac{691}{2730},\ \frac{7}{6},\ \frac{3617}{510},\ \frac{43867}{798},\ \&c.,$$

[1] See Mill, *Logic*, Book III, ch. i, ii.

[2] Mill has often been attacked because of his "empiricist" attitude. Professor Welton, for instance, says that Mill has two incompatible theories of inference: (1) Inference is based on resemblance between phenomena; (2) inference is grounded in the essential conditions of phenomena "In the former view Mill keeps fairly close to the empiricist position. But the latter position is quite inconsistent with empiricism; for analysis of conditions necessarily involves the synthetic activity of thought." But such an argument suggests considerable misapprehension of the real empiricist position. Cf. ch. xi.

a series which seems to set all regularity and method at defiance, and it would puzzle anyone not a mathematician to detect any form of symmetry in the relations amongst them.[1]—"Induction is the deciphering of the hidden meaning of natural phenomena."

Jevons calls an induction *perfect* "when all the objects or events which can possibly come under the class treated have been examined". In all other cases, "induction is *imperfect* and is affected by more or less uncertainty". Thus, Jevons's views of what constitutes induction are almost diametrically opposite to those of Mill. In answer to the objection that the process of "perfect induction" can give us no information, and is merely a summing up, in a brief form, of a multitude of particulars, Jevons says, "but mere abbreviation of mental labour is one of the most important aids we can enjoy in the acquisition of knowledge". No doubt; but a mere shorthand regis tration of facts is a very different thing from inference.

Jevons considers that we pass from perfect to imperfect induction "when once we allow our conclusions to apply, at all events apparently, beyond the data on which it was founded". But "imperfect induction never makes any real addition to our knowledge, in the meaning of the expression sometimes accepted. The results of imperfect induction, however well authenticated and verified, are never more than probable." "The theory of probability shows how far we go beyond our data in assuming that new specimens will resemble the old ones."[2]

In Jevons's opinion, "there are but three steps in the inductive process: (1) Framing some hypothesis as to the character of the general law; (2) Deducing consequences from that law; (3) Observing whether the consequences agree with the particular facts under consideration".[3] But our final conclusion, he thinks, never passes from the realm of probability to that of absolute certainty.

§ 7. Professor Welton's Views

Professor Welton's views are clearly expressed and very suggestive, though he constantly shows impatience with the empiricist position.

He considers there is general agreement that "induction is essentially an analysis of the process by which a universal judg-

[1] The numbers are known as those of Bernouilli. The first thirty-one of these numbers were published by Ohm in vol. xx of *Crelle's Journal*. Prof. J. C. Adams has calculated the next thirty-one. See the *Brit. Assoc. Rpt.* for 1877.

[2] See *Principles of Science*, pp. 146-51, 218-19. [3] *ib.* pp. 265-6.

ment about reality can be established, and that this process starts with the particular". Owing, however, to the complexity of the data of experience, the process of analysis is very liable to error. There is thus an advantage of plurality of instances, for the observer is then more likely to detect unessential elements. If, however, the conditions can be exactly ascertained in a single instance, plurality of instances is unnecessary. This is often the case in chemical experiments. "The only cases in which an inference is made from number of instances as such is when it is impossible—at any rate for the time—to ascertain the conditions of the phenomenon in question; and then the inference is not inductive, but belongs to the domain of mathematical probability."—This view should be compared with Jevons's.

Professor Welton gives the following as the essential steps in the inductive process:—

1. The formation of a hypothesis suggested by a first observation of facts.
2. The deduction of the consequences of this hypothesis.
3. The testing of these consequences by a careful analysis of phenomena.
4. The consequent exact definition of the hypothesis, which then, as expressing the true universal nature of reality, is verified and received as an established theory or law.[1]

§ 8. No Hard-and-fast Rules Universally Applicable

It is clear that there is considerable divergence of views as to the nature and method of induction. Since, however, induction is an inverse process, it is practically impossible to reduce the process to any stereotyped method. Every case presents its own difficulties, and sometimes these are so great as apparently to defy solution. For instance, despite the ingenuity of some of the most brilliant men of science, we are still absolutely ignorant of the real nature of gravitation. Although, therefore, Professor Welton has given us a set of admirable rules for the general solution of the inductive problem, we shall find that men of science by no means always work exactly on these lines, and that Mill's Canons of induction are certainly not to be tossed away as useless lumber, as logicians of a certain school of thought demand. Nature presents her problems to us in an almost infinite variety of ways, and it

[1] See Welton, *Manual of Logic*, vol. ii, pp. 55–60.

is a great mistake to think that those problems can all be solved by any single set of simple rules. It is amazing to find what a number of expedients are adopted by men of science for pursuing their work. Research is not such a simple task as many theorists regard it.

§ 9. The Ground of Induction

It remains to consider the *ground* of induction.

The induction of the ancients, like the induction of unlettered moderns, consisted in ascribing the character of general truths to all propositions which are true in every instance that happens to be known. It is the "perfect" induction of "simple enumeration", and is the kind of induction natural to the mind when unaccustomed to scientific methods. The unprompted tendency of the mind is to generalize its experience, provided this points all in one direction, and provided no other experience of a conflicting character happens to present itself. The notion of interrogating nature is of much later growth. "The observation of nature by uncultivated intellects is purely passive; they take the facts which present themselves without taking any trouble of searching for more. It is only a superior mind which asks itself what facts are needed to enable it to come to a safe conclusion, and then looks out for these."[1]

Yet, even in the most scientific induction, we are making a tremendous assumption,—an assumption with regard to the course of nature and the order of the universe,—that what happens once will, under a sufficient degree of similarity of circumstances, happen again, and not only again, but as often as the same circumstances recur.

This universal assumption which is our warrant for all inferences from experience, is often referred to as the *uniformity of nature*; and it must be regarded as the fundamental principle, or general axiom, of induction.[2]

It is exceedingly difficult to justify such an assumption. Can we regard such a vast generalization as itself an instance of induction? Mill thinks we can. But Professor Welton and his school will not admit this at all.

Yet our sole guarantee for inductive inference *is* the uniformity of nature, or indeed for inference of any kind. If we put a piece of wood into the fire and see it burned, we infer that another piece will

[1] Mill, *Logic*, Book III, ch. iii, § 2. [2] *ib.* III, iii, § 1.

be consumed in like manner. This is to take for granted that what has happened will, in the same circumstances, happen again; in other words, that nature is uniform.[1] We are bound to pass across the gulf, from the experienced known, either present or remembered, to the unexperienced and unknown; we must perform the leap of real inference. We are doing the same kind of thing, and making just the same kind of fundamental assumption, every day of our lives.

We can give no final logical reason for our assumption that nature is uniform. But we make the assumption, and feel bound to make it, though at the same time we are forced to admit that by so doing we are begging the whole question. Theoretical proof seems to be absolutely impossible, but the probability of the truth is so enormous that the practical man does not hesitate to accept it. For though it be admitted that the assumption is the outcome of an induction of mere simple enumeration, the facts enumerated are coextensive with all human experience.[2]

CHAPTER XVI

Some General Principles of Investigation Observation and Experiment

§ 1. Preliminary Notions

When a railway accident takes place, an enquiry is held as to its *cause*. This may prove no easy matter, may in fact be so difficult that the cause never is absolutely determined. The accident may have been due to an error of a signalman, to an oversight of the engine-driver, to a defect in the permanent way, to an obstruction, to the collapse of a wheel, or to any one or more of a large number of possible other causes. A witness may suggest what he considers to have been the cause; the suggestion is made a working hypothesis, the consequences of which are traced out, and a comparison made with known facts; the hypothesis is thus verified or shown to be wrong: and so on. The important point to notice is that the

[1] See Bain, vol. i, p. 19.

[2] *ib.* pp. 273-4. The reader may usefully consult Dugald Stewart, *Phil. of Human Mind*, pp. 459-92; Reid, *Active Powers*, p. 22, &c.; Hamilton, *Metaph.*, vol. i, pp. 96-109; Sigwart, *Logic*, pp. 334 *et seq.*; Clifford, *Essays*, vol. i, pp 131, 155; Karl Pearson, *Gram. of Sci.* pp. 53-9; De Morgan, *Logic*, pp. 211-26; MacColl, *Symbolic Logic*, pp. 100-101.

procedure is often exceedingly difficult when we are given an *effect* and have to discover the cause. On the other hand, if we are given a *cause*, it is usually a comparatively simple matter to produce an effect. We see a dead bird lying on the roadside; to discover the cause of its death might be impossible; but if we *knew* the cause, if, for instance, we knew that death resulted from the shot of a sportsman's gun, we could quite easily bring about a similar effect by bringing the known cause into action. Or, we might decide to use heat as a cause, in which case we could devise experiments to show its various effects; but if we discover heat as an *effect*, say, in a fermenting mass, we cannot, by any simple and certain means, determine the cause. We have, first, to *conjecture* a cause; we then devise experiments to find out the effect of that conjectured cause; then, if these tally with the effect in question, we have probably determined *its* cause.[1] So generally: given an antecedent, the consequent is easily determined; but, given the consequent, the antecedent is usually determined only with difficulty. It is this latter operation which is perhaps the greatest problem of induction.

The problem is rendered more difficult by the fact that, in practice, circumstances are nearly always of a complex character, and it is more than probable that the apparent simplicity of even a very easy experiment may be quite deceptive. Let us, for instance, consider Jevons's experiment of rubbing two sticks together, and make an exhaustive statement of the conditions. There are the form, hardness, organic structure, and the numerous chemical qualities of the wood; the pressure and velocity of the rubbing; the temperature, pressure, and chemical qualities of the surrounding air; the proximity of the earth with its attractive and electric powers; the temperature and other properties of the persons producing the motion; the radiation from the sun; and so forth. On *a priori* grounds, it is unsafe to assume that any one of these circumstances is without effect, and it is only by experience that we can single out those precise conditions from which the observed heat of friction proceeds.

Obviously we must, if we can, remove one at a time those conditions which may be suspected of having an influence on the result. To decide, for instance, in the above experiment, whether the presence of air is a contributory factor to the result, we repeat the experiment exactly as before except that it is done *in vacuo*. If heat still appears, we infer that air is not, in the presence of the

[1] Cf. Bain, *Inductive Logic*, p. 44.

other circumstances, a requisite condition. The conduction of heat from neighbouring bodies may be a condition; to determine this we make all the surrounding bodies ice-cold, which is what Davy aimed at in rubbing two pieces of ice together. And so on.[1]

Again, suppose we wish to determine why meat putrefies when exposed to the air. We know that the atmosphere contains oxygen, nitrogen, carbon dioxide, water vapour, ammonia, numerous other gases, and solid particles, partly dust and partly living germs; and we know that it possesses at any given moment a certain temperature, a certain pressure, a certain electrical condition, and no doubt other peculiarities. Evidently the possible variety of antecedents is very great, and this must always be the case when the air is presented to us as a cause or agency. As before, we try to adopt the method of elimination, though the disentangling process may prove to be both tedious and difficult. And the same thing is true generally.[2]

§ 2. "Varying the Circumstances"

In order to discriminate the necessary from the unnecessary elements of cause and effect, our only course is to *vary the circumstances.* We suspect a plurality of antecedents, and a plurality of consequents, or both, and the problem is to single out the connected couples of antecedent and consequent. This requires us to look for other instances where the groupings are different, and to note what happens when particular antecedents and consequents are wanting.[3] It will be readily seen that

(1) Whatever antecedent *can be left out* without prejudice to the effect, can be no part of the cause; and

(2) When an antecedent *cannot be left out* without the consequent disappearing, such antecedent must be the cause or part of the cause.[4]

Let A represent a cause, and a an effect. In nature we seldom have A followed by a alone. What we find is A in combination with other things, as ABC, and a also in combination, as abc. But these conjunctions are not rigid and invariable, or our task would be easy, the fact being that, though a cause may always be in combination with other agents, it is not always in the same combination. At one time the union is ABC, at another time ABD, and again

[1] Cf. Jevons, *Principles of Science*, pp. 416-7.
[2] Cf. Bain, pp. 42-5.
[3] Cf. Bain, p. 43.
[4] *ib.* pp. 47-8.

ACE, there being corresponding conjunctions in the effects *abc*, *abd*, *ace*.

If we suppose, then, the instances *ABC* giving *abc*, *ABD* giving *abd*, *ACE* giving *ace*, we reason thus: so far as the first instance is concerned, *ABC* giving *abc*, the effect *a* may be produced by *A* or by *B* or by *C*. In the second instance, *ABD* giving *abd*, the cause *C* is absent, the effect *a* still remaining; hence *C* is not the cause of *a*. In the third instance, *ACE* giving *ace*, *B* is absent, *a* remaining; hence *B* is not the cause of *a*. The only antecedent persisting through all the instances is *A*; when *a* is present as a consequent, *A* is always present as an antecedent. If, then, we are sure that every other antecedent circumstance has been removed in turn, the consequent *a* still surviving, we have conclusive evidence that *A* is a cause, condition, or invariable accompaniment of *a*.[1]

It is scarcely possible to pay too much attention to this line of argument, as it is the *kind* of reasoning that occurs in all scientific investigations. We shall have to consider it further in connection with Mill's Canons.[2]

Of course we must not assume the conditions to be independent:[3] they seldom are. And this is one of the investigator's greatest difficulties. It is often impossible, too, to alter one condition without altering others at the same time, and thus we may not get the pure effect of the condition in question. Perhaps, however, the most treacherous source of error is the existence of unknown conditions which, of course, we cannot remove except by accident. Even the greatest investigators have failed because of the existence of unsuspected conditions.[4]

§ 3. Observation

The first work of the investigator is, however, the recording of all necessary facts, and it will be convenient at this stage to consider a little more clearly the nature of observation and experiment.

[1] Cf. Bain, p. 50. [2] See the next chapter.

[3] Suppose we have five or six or more antecedents. We ought to try the effect of the absence of each condition, both in the presence and absence of every other condition, and every selection of those conditions (excluding, of course, the case where the whole of the conditions might be imagined to be absent together). Perfect and exhaustive experimentation would, in short, consist in examining natural phenomena in all their possible combinations. But such exhaustive investigation is practically impossible because the number of experiments would be so great. Six antecedents would require $2^6 - 1$ or 63 experiments. The experimenter therefore has to fall back upon his own insight and experience in selecting those experiments which are most likely to yield him significant facts. (See Jevons, pp. 417–8.)

[4] *ib.*

Let us suppose we are early arrivals at a theatre. Our attention at first is of a general character; our eyes wander round the auditorium, and we feel no special interest either in the building or in the people present. The curtain rises: our attention is at once confined to the stage. An actor enters: our attention is withdrawn from the stage itself and concentrated upon him, his dress, and his bearing. He takes a letter from his pocket, and our attention is still further narrowed down to that particular act.—From first to last there has been observation, but the observation has gradually become more intensive; the act upon which the attention is last concentrated has become isolated. For close observation, we *select* and *isolate*. If anything distracts the attention and renders the isolation less complete, the observation is rendered imperfect.

But with even the closest attention, our observations may be entirely incorrect. Any one of our organs of sense is easily deceived, a fact which enables the magician to make his living. Then it is seldom that we see the whole of any event that occurs: a cab and a bicycle collide, and half a dozen "witnesses", all perfectly honest, may—probably will—give accounts which differ materially and may be mutually destructive. It is always difficult to keep fact and inference distinctly apart. In the middle of the night we "hear a dog bark in the street". But really all that we hear is a noise; that the noise comes from a dog, and that the dog is in the street, are inferences, and the inferences may be wrong. For instance, a boy may be imitating a dog; and everybody knows how easily the ear is deceived in regard to the direction of sound. It is almost impossible to separate what we perceive from what we infer; and we certainly cannot obtain a sure basis of facts by rejecting all inferences and judgments of our own, for in all facts such inferences and judgments form an unavoidable element. Even when we seem to see a solid body occupying, as it does, space in all dimensions, we really see only a perspective representation of it, as it appears depicted on a surface. Our knowledge of its solid form is obtained by inference. A clever painter may deceive us even here.

But we can do one thing at least, and that is to make all facts depend upon the intellect alone, and not to allow ourselves to be in the least swayed by any feelings of admiration, fear, and the like.[1] A scientific observer always records all evidence against as well as for, and he immediately abandons a hypothesis or view as soon as any new facts demand it.

[1] Cf. Whewell, *Nov. Org. Ren.*, pp. 50–6.

It is the essence of good observation that the eye shall not only see a thing itself, but of what parts that thing is composed. And if an observer is to become a successful investigator in any department of Science, he must have an extensive acquaintance with what has already been done in that particular department. Only then will he be prepared to seize on any one of those minute indications which often connect phenomena apparently quite remote from each other. His eyes will thus be struck with any occurrence which, according to received theories, ought not to happen; for *these are the facts which serve as clues to new discoveries.* The deviation of the magnetic needle, by the influence of an electric current traversing a wire, must have happened hundreds of times to a perceptible amount, under the eyes of persons engaged in electric experiments, with apparatus of all kinds standing around them, but it required the eye of Oersted to seize the indication, refer it to its origin, and thereby connect two great branches of Science.[1]

§ 4. Experiment

By observation alone it is often impossible to find out precisely what conditions are operative, and thus, when possible, we call in the aid of experiment. The object of an experiment is to get one or more of the conditions under our control,—to set them in action or stop them, to raise or to lower their intensity, and to eliminate, if and when possible, the unessential conditions of the phenomenon under investigation.

The great rule in experiment is *to vary only one circumstance at a time,* and to maintain all other circumstances rigidly unchanged. Evidently there are two reasons for this rule: in the first place, if we vary two conditions at a time, and find some effect, we cannot tell whether the effect is due to one or the other condition, or to both jointly; in the second place, if no effect ensues, we cannot safely conclude that either of them is indifferent; for the one may have neutralized the effect of the other.[2] If we want to prove that oxygen is necessary to life, it is useless to put a mouse into a vessel from which the oxygen has been removed by a burning candle. We should then have not only an absence of oxygen, but the presence of carbon dioxide, and the carbon dioxide itself might cause the animal's death. For a similar reason, Lavoisier avoided the use

[1] Cf Mill, *Logic*, Book III, ch. vii, § 2; and Herschel, *Phil.*, p. 132.
[2] Cf. Jevons, *Principles of Science*, p. 423.

of atmospheric air in experiments on combustion, because air was not a simple substance, and the presence of nitrogen might impede or even alter the effect of oxygen.[1]

An observation does not necessarily become an experiment when we call in the aid of an instrument. When, for instance, we use a telescope for viewing a distant object, or a microscope for observing a small object, we are clearly not performing an experiment, for we have no sort of control over any of the conditions which determine the phenomenon under observation. As Bosanquet says,[2] experiment is observation under artificial conditions, that is, conditions produced or arranged by human action. But observation with the telescope or microscope is observation under natural and not artificial conditions. Yet observation tends gradually to take on the character of experiment, and the transition between the two is quite gradual. We may consider experiment to begin when we pass on to actual interference with the conditions that determine the phenomenon under observation, though even before this line is reached observation passes into something which may properly be called "natural experiment". "When the earliest astronomers simply noticed the ordinary motions of the sun, moon, and planets, they were pure observers. But astronomers now select precise times and places for important observations. They make the earth's orbit the basis of a well-arranged *natural experiment*, as it were, and take well-considered advantage of motions which they cannot control. Meteorology might seem to be a science of pure observation, because we cannot possibly govern the changes of weather which we record. Nevertheless we may ascend mountains, or rise in balloons or aeroplanes, and may thus so vary the points of observation as to render our procedure experimental."[3]

Again, a microscope is, *par excellence*, an observing instrument, but the moment we modify the object under observation (for instance, by applying chemical reagents on the stage of the microscope), we are experimenting.[4] If a student is told to dissect a rabbit, expose the recurrent laryngeal nerve, and show that it loops round the subclavian artery, he does not perform an experiment; he is merely clearing away obstructions for the purpose of adequate observation. But if he is told to cut the sympathetic nerve in order that the muscles of the bloodvessels of the ear may become relaxed, and the vessels themselves dilated and filled with

[1] Jevons, *Prin. of Sci.*, p. 423.
[2] *Logic*, vol. ii, p. 143.
[3] Jevons, *Prin. of Sci.*, pp. 400-1
[4] Cf. Bosanquet, *Logic*, vol. ii, pp. 144-5.

blood (for the purpose of producing artificial "blushing"), he is performing an experiment, for the act of cutting the nerve has brought under his control certain conditions determining the phenomenon under investigation. And if, further, he irritates the cut end of the sympathetic which remains connected with the vessels, in order to cause contraction of the latter, he is again performing an experiment, for again he is controlling some of the determining factors.

§ 5. Experiment not always Possible

It need hardly be pointed out that there are many operations in nature which we cannot imitate by experiment. Our object is to study the conditions under which a certain effect is produced, but one of these conditions may involve a great length of time. All metamorphic rocks, for example, have doubtless endured high temperatures and enormous pressure for inconceivable periods of time, so that at least one part of Geology is quite outside the scope of experiment. A similar remark applies to Darwin's theory of the origin of species,[1] and in fact to hundreds of other questions in the domain of Geology, Botany, and Natural History generally. Why, for instance, is the average height of a horse greater than that of a dog? How can we answer such a question? We may put forward a conjecture, but we have no possible means of proving its truth. These branches of knowledge, in contrast to such branches as Chemistry and Physics, are essentially *observational*; experiment can play only a minor part. Hence their state of relative uncertainty and undevelopment.

Observers should be on their guard against coming to the conclusion that non-observation of a phenomenon necessarily implies its non-occurrence. There are sounds which we cannot hear, rays of heat we cannot feel, multitudes of worlds we cannot see, and myriads of minute organisms of which not even the most powerful microscopes can give us a view. Inferences have often been drawn from the non-occurrence of particular facts or objects, but a more extended and careful examination has often proved their falsity. It must not, however, be supposed that negative arguments are of no force or value, though negative conclusions are by their very nature treacherous. In Natural History, for instance, the utmost patience will not enable an observer to watch the behaviour of a particular living thing in all circumstances continuously for a great

[1] See Jevons, pp. 437-8.

length of time. There is always a chance that the initial act or change may take place when the observing eyes are withdrawn. Darwin himself adopted one conclusion on negative evidence, namely, that certain orchids secrete no nectar. But his caution and unwearying patience in verifying the conclusion give an impressive lesson to the observer. For twenty-three consecutive days he examined flowers in all states of the weather, at all hours, in various localities. Flowers of different ages were subjected to irritating vapours, to moisture, and to every condition likely to bring on the secretion, and only after invariable failure of this exhaustive enquiry was the barrenness of the nectaries assumed to be proved.[1]

We may conclude this chapter by reference to a few well-known experimental researches, from which we may learn useful lessons.

§ 6. Experimental Researches.—(1) By Newton

(i) Newton's work in connection with the spectrum teaches us a particularly valuable lesson,—the fundamental necessity, in experimenting, of *varying the circumstances*. Newton says,[2] "The different magnitude of the hole in the window shut, and different thickness of the prism where the rays passed through it, and different inclinations of the prism to the horizon, made no sensible changes in the length of the image. Neither did the different matter of the prisms make any; for in a vessel made of polished plates of glass cemented together in the shape of a prism, and filled with water, there is the like success of the experiment according to the quantity of the refraction." Yet even Newton overlooked one important point.—Throughout his researches on the spectrum, he was quite unsuspicious of the fact that if he reduced the hole in the shutter to a narrow slit, all the mysteries of the bright and dark lines were within his grasp, provided, of course, that his prisms were sufficiently good to define the rays. *He forgot to vary the circumstances of one condition*, though he took the greatest pains to vary them in the case of all the other conditions.[3]

(ii) In many experiments we wish to study only one condition, the other conditions being interfering forces which we avoid if possible. One of the determining conditions of the motion of a

[1] Darwin, *Fertilization of Orchids*, p. 48. Cf. Jevons, *Prin. of Sci.*, p. 413.
[2] *Opticks*, p. 25.
[3] Cf. Jevons, *Prin. of Sci.*, pp. 418, 420, 424; Newton, *Opticks*, p. 25. See also ch. xxxvii.

pendulum is the resistance of the air or other medium in which it swings. But when Newton desired to prove the "equal gravitation of all substances", he had to avoid this interfering resistance, for his object was to observe the result of the action of the one force due to gravitation only. He therefore made his pendulums, of which the oscillations were to be compared, of equal boxes of wood, hanging by equal threads, and filled with different substances, so that the total weights should be equal, and the centres of oscillation at the same distance from the points of suspension. Hence the resistance of the air became approximately a matter of indifference; for the outward size and shape of the pendulums being the same, the absolute force of resistance would be the same, so long as the pendulums vibrated with equal velocity; and the weights being equal, the resistances would diminish the velocity equally. Hence if any inequality were observed in the vibrations of the two pendulums, *it must arise from the only circumstance which was different*, namely, the chemical composition of the matter within the boxes. But no inequality was observed, and the conclusion therefore was that the chemical composition of substances could have no appreciable influence upon the force of gravitation.[1]

§ 6. (2) By Faraday

An experiment of Faraday's shows how *the alteration of a single circumstance* sometimes conclusively explains a phenomenon. It was known that lycopodium powder scattered on a vibrating plate collected together at the points of greatest motion, whereas sand and all heavy particles collected at the nodes where the motion was least. It occurred to Faraday to try the experiment under the exhausted receiver of an air-pump, and it was then found that the light powder behaved exactly like the heavy powder. The obvious conclusion was that the presence of air was the differentiating and determining factor, doubtless because it was thrown into eddies by the motion of the plate, and carried the lycopodium to the points of greatest agitation. Sand was too heavy to be carried by the air.[2]

§ 6. (3) By Brewster

One of Brewster's experiments may be quoted as an example of the possibility of *none of the most obvious of the antecedents taking any*

[1] *Principia*, III, vi; and cf. Jevons, pp. 443–4.
[2] See Jevons, *Prin. of Sci.*, p. 419. (See also ch. xxxviii.)

part in the production of a phenomenon. If we were asked to account for the peculiar colours of mother-of-pearl, we should most probably suggest that they must be due to some peculiarity in the chemical composition of the substance. Brewster himself was ignorant of the real explanation, but he had occasion to fix a piece of mother-of-pearl to a goniometer with a cement of resin and beeswax, and upon removing it was surprised to see the whole surface of the wax shining with the prismatic colours of the mother-of-pearl. He first thought that a film of the substance had been left on the wax; but this was soon found to be a mistake, and it became manifest that the mother-of-pearl really impressed upon the cement its own power of producing coloured spectra. Further investigation showed that the colours were produced by a particular configuration of the surface. The surface, examined with a microscope, presented a grooved structure, like a section of the annual growths of wood, the grooves being obviously the sections of all the concentric strata of the shell. If we examine the actual surface of any one stratum (and the ordinary surface of the pearl itself is such a stratum) none of the colours are seen.[1] Of course, the grooves having been detected, the proper explanation of the colours was obvious.[2] The point to notice is that the easily observed antecedents were all misleading; the real antecedent might, but for accident, have remained concealed until this day.[3]

§ 6. (4) By Franklin

Experiments sometimes lead to wrong conclusions *because of the impossibility of carrying out the rule of varying one circumstance at a time.* Franklin's experiment concerning the comparative absorbing powers of different colours is well known. He took a number of little square pieces of broadcloth from a tailor's pattern-card, of various colours. He laid them all out upon the snow on a bright sunny morning. "In a few hours, the black being the most warmed by the sun, was sunk so low as to be below the stroke of the sun's rays; the dark blue was almost as low; the lighter blue not quite so much as the dark; the other colours less as they were lighter. The white remained on the surface of the snow, not having entered it at all." To the uninitiated, the inferences to be drawn seem to

[1] Brewster, *Optics*, pp. 118-20 Cf. Jevons, p. 419.
[2] See any standard textbook on Light.
[3] Cf. *Camb. Nat. History*, "Mollusca", pp. 253-4; Carpenter, *B. A. Address*, p. xiii, ff.

admit of little doubt, but Leslie, in his researches upon the nature of heat, was forced to the conclusion that the colour of a surface has very little effect upon the radiating power, the mechanical nature of the surface appearing to exert a greater influence. He considered the question incapable of solution, since no substance can be made to assume different colours without at the same time changing its internal structure,—that therefore it was impossible to vary one circumstance at a time. The whole subject is, of course, complicated and difficult.[1]

§ 6. (5) By Davy

Sometimes, *unsuspected conditions may lead to erroneous results.* The early alchemists were misled by the unsuspected presence of traces of gold and silver in the substances they proposed to transmute. The unsuspected presence of common salt in the air at one time gave great trouble, and, in the earlier work on electrolysis, led to the erroneous conclusion that electricity had the power of generating acids and alkalis. Davy undertook a systematic investigation of the circumstances, by varying the conditions. For his glass vessel he substituted one of agate or gold, and then found that far less alkali was produced; excluding impurities by the use of distilled water, he found that the quantities of acid and alkali were still further diminished; and having thus obtained a clue to the cause, he completed the exclusion of impurities by avoiding contact with his fingers, and by placing the apparatus under an exhausted receiver, no acid or alkali being then formed. He thus detected *a previously unsuspected antecedent.*[2]

[1] Jevons, pp. 424-5; Leslie, *Enquiry into the Nature of Heat*, p. 95. Cf also any standard work on Heat.

[2] *Works of Sir Humphry Davy*, vol. v, pp 1-12. Cf. Jevons, p. 429. The reader may usefully compare these different investigations with modern views of the phenomena mentioned, and see if any additional facts have come to light rendering new explanations necessary.

CHAPTER XVII

Mill's "Canons"

§ 1. The Basis of the Canons

Let us suppose that, by observation and comparison of a number of cases of an *effect*, we have found an antecedent which appears to be, and perhaps is, invariably connected with it. Whether this antecedent is the *cause* we do not yet know, and cannot know until we have reversed the process and by experiment produced the effect by means of the antecedent. Observation without experiment is very unlikely to prove causation. In the case of most of the phenomena which we find conjoined, we cannot know with certainty which is cause and which effect, or whether either of them is so, or whether they are not both effects of causes yet to be discovered. We thus resort to experiment, in the hope of being able to isolate and examine separately the different conditions.[1]

But, as we have seen, isolation is often impossible, and experimental enquiry cannot therefore be reduced to any form of mechanical routine. Although Mill was well aware of the difficulties of such enquiry, he thought rules of general procedure ought to be possible, and set himself the task of formulating them.

As he says, the simplest and most obvious modes of singling out, from among the circumstances which precede or follow a phenomenon, those with which it is really connected by an invariable law, are two in number: (1) by comparing together different instances in which the phenomenon occurs; (2) by comparing instances in which the phenomenon does occur with instances in other respects similar in which it does not. Thus we have a Method of Agreement, and a Method of Difference; and these form the basis of all Mill's rules.

§ 2. The Method of Agreement

If two or more instances of the phenomenon under investigation have only one circumstance in common, that circumstance may be regarded as the probable cause (or effect) of the phenomenon.[2] (Mill's first Canon.)

The following are illustrations:—

1. After taking a particular kind of food, whatever else I may

[1] Cf. Mill, Book III, ch. vii, § 4.

[2] The wording of the various Canons has been slightly simplified. Fowler's modified forms will serve for purposes of comparison

eat or drink, and however various my general state of health, the climate in which I am living, and my other surroundings, I am invariably ill. I am therefore justified in regarding the food as the probable cause of my illness, and avoid it accordingly.

2. I find that a certain plant always grows luxuriantly on a certain kind of soil; if my experience of the other conditions be sufficiently various, I am justified in concluding that the soil probably possesses certain constituents which are peculiarly favourable to the production of the plant.[1]

3. We compare instances in which bodies are known to assume a crystalline structure, but which have no other point of agreement. In the great majority of instances, though not in all, we find that these bodies have assumed their crystalline structure during the process of solidification from a fluid state, and, so far as can be observed, these cases have no other circumstance in common. We may therefore reasonably infer that the passage from a fluid to a solid state is a probable cause, though not the only cause, of crystallization.[2]

In all such cases, we have only probability, not certainty. All our inferences *may* turn out to be wrong, for we can never be sure, by the Method of Agreement alone, that we have eliminated all the casual circumstances. The difficulty is especially felt when we are attempting to find the cause of a given effect. The same event may be due to a great number of distinct causes (as is exemplified in the familiar cases of motion, disease, death, &c.). We are not justified, therefore, in neglecting to take account of what is sometimes called Plurality of Causes, though by the multiplication and variation of instances, the possible error due to the Plurality of Causes may be rendered less and less probable. In practice, of course, we generally have a great number of antecedents and a great number of consequents (or rather a great number of antecedents contributing to a complex effect), and it often becomes virtually impossible to find in a collection of instances of a phenomenon one circumstance[3] in which alone they all agree. The Method of Agreement, then, has a "characteristic imperfection".[4]

It should be noticed that the Method of Agreement is mainly, though not exclusively, a method of Observation rather than of

[1] Cf. Fowler, *Induc. Log.*, pp. 138–9. [2] *ib.* p. 145.

[3] Really one *other* circumstance, not the circumstance that brought the instances together for comparison.

[4] It will be understood that when we say "one circumstance in common", we ordinarily exclude such common circumstances as gravity, exposure to the atmosphere, &c.

Experiment, and is usually applied for the purpose of inquiring into the causes of given effects, rather than into the effects of given causes.[1] But it is essentially a method for *suggesting a clue*; it is rarely final.

§ 3. The Method of Difference

If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon. (Mill's second Canon.)

Both the Method of Agreement and the Method of Difference are methods of *elimination.* This term, borrowed from the theory of equations, is employed to denote the process of excluding, one after the other, the various circumstances which are found to accompany a phenomenon in a given instance.

The Method of Agreement stands on the ground that whatever *can* be eliminated *is not* connected with the phenomenon by any law; the Method of Difference, that whatever *cannot* be eliminated *is* connected with the phenomenon by a law.

Difference plays a great part in our everyday inferences. The usual form is the sudden introduction of some limited and definite agency or change, followed by an equally definite consequence. (1) We drink some water, and there is a cessation of thirst; we do not hesitate to pronounce the former fact the cause of the latter; (2) we make a noise, and a sleeping child awakes; (3) we rub a match, and it bursts into flame. In every case the new agency is followed by the new effect.

A large part of our knowledge of nature is gained by making experimental changes and watching the consequences. An immediate response is usually satisfactory evidence. In fact, wherever Difference can be resorted to, a knowledge of causes is gained at once. We introduce a new antecedent x, to which the new consequent y must be due. But if the omission of one circumstance be attended by the omission of another, we may argue with equal confidence. A man is deprived of food and he dies, and we do not hesitate to ascribe the disappearance of what we call life to the withdrawal of the means by which it is maintained.[2]

[1] Cf. Fowler, *Induc. Log.*, pp. 130-48; Bain, pp. 49-57; Mill, Book III, viii, §§ 1, 2.
[2] Cf. Bain, pp. 58-9.

The Method of Agreement is often of great use for suggesting applications of the Method of Difference. If the former, for instance, suggests that *A* is an invariable antecedent, we try to produce *a* by experiment, and so find out whether *A* is an *unconditional* invariable antecedent, i.e. the *cause*, of *a*. For example, on the first use of some new coal, we notice in the case of the five or six fires in the house, rather frequent slight "explosions", fragments of burning material being projected into the room. An examination shows that in each fire there are one or more stones. This one common circumstance suggests, by the Method of Agreement, the probable cause of the "explosions". We now try the experiment of actually putting a stone in the fire. An "explosion" follows; and so we get a decisive test by the application of the Method of Difference; for *A* is now seen to be the real cause of *a*.

The Method of Difference is very extensively used in experimental Science, especially Chemistry. We mix, for instance, a solution of chloride of mercury with a solution of potassium iodide; the result is a red precipitate in a colourless liquid. We at once infer that the *change produced* (the effect, the consequent) is due to the *mixing* (the cause, the antecedent). Of the truth of this inference we are absolutely certain from the experiment alone. But any further inference (for example, as to the composition of the red precipitate) is not legitimate *from the experiment*, though it may be from our stock of previous knowledge.

Any textbook of Science will afford an abundance of further examples. We may mention one other.—Arago, having suspended a magnetic needle by a silk thread and set it in vibration, observed that it came much sooner to a state of rest when suspended over a plate of copper than when no such plate was beneath it. In the two experiments there was but *one Difference*,—the presence or absence of the plate of copper. Clearly, therefore, the retarding influence was exerted by the copper itself.

In the employment of the Method of Difference, the greatest care should be taken to introduce only one new antecedent, or at least only one new antecedent which can influence the result. As the whole force of the argument based on this method depends on the assumption that any change that takes place in the phenomenon is due to the antecedent then and there introduced, clearly we can place no reliance on our conclusion unless we feel perfectly assured that no other antecedent has intervened. Had the ancients recognized that instead of one cause acting on falling bodies, as appeared

to them to be the case, there were really two, the action of gravity tending downwards and the resistance of the atmosphere pressing upwards, they would probably never have fallen into the gross error of supposing that bodies fall in times inversely proportional to their weights.[1]

§ 4. The "Joint" Method[2]

If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances (in the same department of investigation), in which it does not occur, have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ is the effect, or the cause, or an indispensable part of the cause, of the phenomenon. (Mill's third Canon.)

This method is simply an extension of the Method of Agreement, by extending it to *agreement in absence*. When such cases are conjoined with those where the agreement is *in presence*, there is an approach to the conclusiveness of the Method of Difference. We first of all compare cases in which the phenomenon occurs, and, so far as we can ascertain, find them to agree in the possession of only a single circumstance.[3] But though we may not be justified in regarding this inference as certain, we may increase our assurance by proceeding to compare cases in which the phenomenon does not occur. By our positive instances we are, as it were, put on the track of the one circumstance in which the instances agree, and by our negative instances we strengthen the probability of the accuracy of our conclusion.

1. When I take a particular kind of food, I suffer from a particular form of illness; when I leave it off I cease to suffer, and my suspicion that the food is the cause of my illness is confirmed.

2. I notice that a particular plant is invariably plentiful on a particular soil; I fail to find it growing on any other soil; my belief that there is in this particular soil one or more chemical constituents favourable to the growth of the plant is thus strengthened.

3. Suppose I am asked why it is that a north-east wind is specially injurious to many persons.—I already know that winds are characterized by various qualities, such as velocity, temperature, humidity, electricity, ozone, &c., but on investigation I find that no one mode of any one of these qualities uniformly accompanies the

[1] Cf. Fowler, pp 148-59; Bain, pp. 57-61; Mill, Book III, ch. viii, § 3.

[2] Sometimes called "Joint Method of Agreement and Difference", or "Method of Double Agreement".

[3] Rather, a single *other* circumstance. Cf. § 2, *ante*.

north-east wind. But further examination shows that the wind, which blows from the Pole towards the Equator, travels for several thousand miles *close upon the surface of the ground.* This single circumstance is common to all north-east winds, and, on this point alone, the agreement seems to be constant. I now apply the second part of the Joint Method. I examine other winds, for instance the south-west wind, and find it does *not* blow for a long distance close to the surface of the ground, and that it is *not* noxious. My first tentative inference, that the north-east wind, during its contact with the ground, picks up dust, germs, &c., and so becomes noxious, is thus confirmed by negative instances.

Of course the evidence necessary to give absolute proof is not yet complete, but the Joint Method has taken us a long way on the road towards absolute proof.[1]

§ 5. The Method of Residues

Subtract from any phenomenon such part as is known to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents. (Mill's fourth Canon.)

If we know that a total result is due to a certain number of antecedents, and that part of the result is due to a portion of those antecedents, the residue of the result must necessarily be due to the remaining antecedents.—This method is extensively used in the present advanced state of Science.

Herschel makes a suggestive remark when he says, "It was a happy thought of Glauber to examine what everybody else threw away".[2]

1. The discovery of the precession of the equinoxes resulted, as a *residual phenomenon*, from the imperfect explanation of the return of the seasons by the return of the sun to the same apparent place amongst the fixed stars.[3]

2. The Method of Residues was employed in the discovery of the planet Neptune.—From the year 1804 it had been noticed that the orbit of the planet Uranus was subject to an amount of perturbation which could not be accounted for from the influence of the known planets. There was an unknown *residual disturbance*, beyond the disturbances produced by Jupiter and Saturn.[4] Astronomers

[1] Cf. Bain, pp. 61-2; Mill, III, viii, § 4; Fowler, pp. 160-73.
[2] Herschel, *Nat. Phil.*, p. 158. [3] Herschel, *Outlines of Astronomy*, § 856.
[4] The only two of the old planets exercising any sensible action on Uranus.

therefore suspected the existence of an unknown planet, and they were faced with the difficult inverse problem, "*given the disturbances, to find the orbit and the place in that orbit of the disturbing planet*". The problem was successfully solved by two different astronomers,[1] working quite independently, and Neptune was located on September 23, 1846.[2]

3. The inquiry into the cause of sound had led to conclusions respecting its mode of propagation, and from these its velocity in the air could be precisely calculated from purely theoretical considerations. The calculations were performed, but when compared with known experimental fact, though the agreement was quite sufficient to show the general correctness of the assigned cause and mode of propagation, yet the *whole* velocity could not be shown to arise from this theory. There was still a *residual velocity* to be accounted for. At length Laplace struck on the happy idea that this might arise from the *heat* developed in the act of that condensation which necessarily takes place at every vibration by which sound is conveyed. The matter was subjected to exact calculation, with the result that the residual phenomenon was completely explained.[3]

§ 6. The Method of Concomitant Variations

Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation. (Mill's fifth Canon.)

This method is really a peculiar application or series of applications of the Method of Difference. It is employed in those cases where a phenomenon cannot be made to disappear altogether, but where we have the power of augmenting or diminishing its quantity, or at least where Nature presents it in greater or smaller amounts.

1. We know that, if we confine some mercury in a suitable tube, every sensible increase of the temperature of the surrounding atmosphere is accompanied by a sensible increase of the volume of mercury in the tube, and *vice versa*. Now each successive experiment is an application of the Method of Difference, and we arrive at last at the conclusion that the volume of the mercury is invariably dependent on the temperature of the surrounding medium; in other

[1] Mr. Adams and M. Le Verrier. [2] Herschel, *op. cit.*, §§ 767–8.
[3] Cf. Mill, III, ix, § 5; Tyndall, *Sound*, ch. i; Fowler, *Induc. Logic*, p. 181.

words, that augmentation of temperature is the *cause* of expansion It should be noticed that we cannot in such a case employ the Method of Difference, because the phenomenon is one which is only capable of augmentation or diminution, and cannot be made to vanish. We may more and more diminish the heat of a body, but we cannot wholly deprive the body of its heat.

The Method of Concomitant Variations may be used for two purposes, either to establish a causal connection, or to determine the law according to which the phenomena vary. Thus, it may either establish the fact that any increase of temperature causes mercury to expand, or it may determine the exact rate according to which this expansion takes place, a determination which is, in fact, effected by the ordinary thermometer. But when attempting to determine the numerical relations according to which two phenomena vary, the utmost caution is required as soon as our inference outsteps the limits of our observations. There is always the possibility of the intervention of some counteracting cause. For instance, water at 39°, instead of continuing to contract as it becomes colder, ceases at that point to do so, and thenceforward begins to expand. Then, again, whenever the range of our observations is confined within comparatively narrow limits, there must be an element of uncertainty. Different laws of variation may produce numerical results which differ but slightly from one another within narrow limits; and when, therefore, such variations in the quantity of the antecedents as we have the means of observing are small in comparison with the total quantities, there is much danger lest we should mistake the numerical law, and be led to miscalculate the variations which would take place beyond the limits.

2. The experimental proof of the First Law of Motion affords an interesting case of the method. Obviously we cannot entirely remove the various obstacles (friction, resistance of the atmosphere, &c.) which retard motion; we can only diminish them. For instance, we may cause a pendulum to oscillate for a considerable period by reducing the friction at the point of suspension. Borda contrived to reduce this friction to such an extent in the case of a pendulum arranged to oscillate *in vacuo*, that the oscillation continued for more than thirty hours. From such an experiment we do not hesitate to draw the inference that the whole of the retardation to continued motion is due to the obstacles.[1]

[1] Cf. *Nov. Org. Ren.*, III, viii, 2, § 9; and Fowler, p. 195. See also Fowler, pp. 185–206: and Mill, Book III, ch. viii § 7

§ 7. Plurality of Causes

It will be seen that, for all practical purposes, Mill's five Rules reduce to two,—Agreement and Difference, the last three being really modifications or variants of the others.

The Rules take two conditions for granted: first, that an effect has only one cause, or set of antecedents; secondly, that different effects are kept apart and are distinguishable. But both these conditions may be wanting and the methods therefore frustrated. We have then to consider what is sometimes called "Plurality of Causes" and "Intermixture of Effects".

In many cases the same effect may be produced by a *Plurality of Causes;* as motion, or heat. We see a body in motion, but it may be quite impossible to say which one of many possible agents may have set it in motion. We discover a hot body, and it may be equally impossible to say how it became heated.[1]

The operation of Plurality is to give uncertainty to the Method of Agreement. For example, we observe numerous cases of unhealthy human beings whose parents were unhealthy; this fact suggests a possible inference from Agreement. But many unhealthy persons are the children of perfectly healthy parents. We cannot, therefore, draw any safe inference. Ill-health may be due to one or more of many causes.

One remedy for this failure of the Method of Agreement is *multiplication of instances.* This will tend to bring out all the causes, and inquiry is narrowed down to determining, in a given case, which of the causes are present and whether these are free to operate. If, for instance, we are aware of the various antecedents of dyspepsia, —bad food, too much food, too little food, hard labour, want of exercise, intemperance, mental wear and tear, bad air, hot climate, &c.,—we can infer with considerable certainty what brought on the disease in a given instance.

The second remedy is the use of the "Joint Method". We should seek out cases of Agreement *in absence*, which are of a very decisive nature. If, in all cases where a particular effect fails, one particular cause is absent, there is, in spite of possible plurality, a strong presumption that these two circumstances are cause and effect in those instances.

[1] In a great number of inferences in everyday affairs, politics for instance, elimination is vitiated by plurality, and we have the fallacy *post hoc ergo propter hoc.*

§ 8. Intermixture of Effects

Mill's methods suppose different effects to remain separate and distinguishable, whereas cases frequently arise when the effects of different causes unite in a homogeneous total.

An invalid goes to some health resort and adopts every possible means of restoration to health. Many influences combine to the result, but the effect is one and indivisible. A good crop of wheat, the general prosperity of a country, or the demand for some new legislative enactment, is seldom the effect of any one cause exclusively, yet we commonly regard such effects as homogeneous.

The intermixture of effects is a serious obstacle to the successful use of the experimental methods. *ABC* acting together yield not *abc* but *a*; and if *ABD* yield still *a*, nothing is eliminated, and there is no progress.—We have zinc, sulphuric acid, and water in a flask; hydrogen is generated, and we wish to discover the source of the hydrogen. We replace the zinc by magnesium; the result is the same as before, and no inference is therefore possible. We replace the sulphuric acid by hydrochloric acid; again the result is the same, and again we can draw no inference.[1]

In many cases of this kind we have no alternative but to devise some other form of experiment and set to work in an entirely different way. But use may often be made of the Method of Concomitant Variations. For instance, the assigning of the respective parts of the sun and moon, in the action of Tides, may be effected, to a certain degree of exactness, by the variation of the amount according to the positions of the two attracting bodies.[2]

§ 9. Criticism of Mill's Methods

Whewell's opinion of Mill's Methods of Inquiry was unfavourable, mainly because "they take for granted the very thing which is most difficult to discover, the reduction of the phenomena to formulæ. When we have any set of complex facts offered to us, and when we would discover the law of nature which governs them, where are we to look for our *ABC* and *abc*? Nature does not present to us the cases in this form, and how are we to reduce them to this form? You say *when* we find the combination *ABC* with

[1] Of course in practice we should not stop at this point in such a case. Continued substitution would soon furnish us with suggestive data, although the final solution of the problem would be effected another way.

[2] Cf. Bain, pp. 76–84; Mill, Book III, ch. x.

abc, and *ABD* with *abd*, then we may draw our inference. Granted, but when and where are we to find such combinations?"[1]

But Mill himself points out that his Methods are, primarily, rules and models to which, if inductive arguments conform, those arguments are conclusive, and not otherwise. He not only makes no claim that the Methods are infallible; he admits their many imperfections. He particularly points out how the plurality of causes and intermixture of effects may frustrate the Methods. He freely admits the difficulty and the frequent impossibility of disentangling Nature and reducing it to the form of *ABC*, *abc*. Whewell's criticisms are based on the assumption that Mill intended his methods to be the very last word in induction, to be final and absolute. Mill plainly shows that he had no such intention.[2]

Professor Welton is also somewhat unduly critical of the Methods. He enumerates the various claims that Mill makes on behalf of the Methods, and then states that these claims are by no means granted by logicians. But he immediately admits[3] that Mill himself clearly pointed out the strictly limited possibilities of the Methods. Further, he says, "Mill himself supplies us with abundant evidence that the Methods can never prove any general law"; "he also undermines the authority of his Canons and practically destroys it." But it seems beside the mark to criticize Mill for pointing out what his Canons were never intended to do. No one ever saw more clearly than Mill the limitations of the Methods he had taken so much trouble to formulate.

Professor Welton admits, though a little grudgingly, that "the Methods are not worthless. It is true they are not inductive in the empiricist sense, but they are inductive in the sense that they are based upon principles which are operative in inductive inquiry, though in every case they need more exact formulation. The Methods suggest hypotheses."[4]

There is, in actual practice, little to choose between the two schools of logical thought as represented by Mill and Professor Welton. Both admit that the inductive method must begin with an examination of facts. The latter tells us we are now to form a hypothesis, as suggested by such examination. The former tells us *how* to examine our facts,—to discover points of agreement and

[1] *Phil. of Disc.*, p. 262.
[2] The reader should refer to the controversy in Mill's *Logic* and Whewell's *Philosophy of Discovery*. [3] *Logic*, vol. ii, pp. 146-7. [4] *ib.* pp. 156-7.

points of difference, and gives us "methods" for discovering clues for framing tentative hypotheses. Both now tell us to deduce the consequences of our hypotheses. And both point out the importance of final comparison with fact, of verification.

The fact is, the Methods have two entirely different functions to perform. In the first place, they afford us *a general line of inquiry* in scientific investigation. In using them for this purpose, we have to be particularly on our guard against their imperfections and their inherent dangers; and many of these, though pointed out by Mill himself, are wisely emphasized by Professor Welton, as well as by such well-known logicians as Bradley[1] and Sigwart[2]. In the second place, they form admirable *tests of logical proof* in scientific investigation, just as the syllogism is a useful testing instrument when doubt has arisen over a particular deductive inference. The Methods "set us on the road" to discovery, though they do not take us quite to the goal.

Professor Welton will not for a moment accept "time sequence" in the causal relation: such acceptance would carry him straightway into the enemy's camp. But if we will consent to regard antecedent and consequent not as implying time sequence but simply as the respective equivalents of "determining" and "determined", he will meet us willingly. Assuredly we can meet him thus far.[3]

But let the philosophers quarrel. For the practical man it will be sufficient to bear in mind that *all scientific investigation is, at bottom, the seeking of points of agreement and points of difference.*

[1] Bradley, *Logic*, p. 331. [2] Sigwart, *Logic*, vol ii, p 341

[3] Cf. Welton, *Logic*, ii, pp 158–9. The reader should, if possible, read the chapter in Mill, and then Prof. Welton's criticism in *Logic*, vol. ii. The criticisms involve objections which are largely of a theoretical character, but they are nevertheless suggestive in many ways.

Mr. A. Sidgwick makes some useful critical remarks on Mill's methods In the Method of Difference we should be careful to distinguish "between any attempt to apply the Method in the concrete, and the Method itself as an abstract ideal. In the abstract, no fault can be found with the rule, but it is always in concrete cases that any rule must be applied, and nothing is easier than to apply it wrongly What is insufficiently dwelt on in the formal account of the method is that the guarantee of correctness is not to be found in the Method of Difference itself, but in the wisdom and care with which we apply it." (*Use of Words in Reasoning*, pp 88–95.)

CHAPTER XVIII

Classification

§ 1. General Notions

Let us imagine a not over-intelligent young servant sent to a room containing a miscellaneous collection of books, and told to arrange them on some available shelves. There is little doubt that she would arrange them according to their size or to the colour of their binding. Her principle of classification would be determined by the immediately obvious points of resemblance, and would not in any way be the result of that kind of careful examination we should expect from a trained librarian. Mere words,—"large", "small", "red", "green", and the like,—would, in the main, control her labours. Her groups would be a mere incidental effect consequent on the use of names intended for a totally different purpose. In a word, her classification would not be scientific.

In scientific classification we endeavour to avoid being led away by the fact that general names imply the recognition of classes of things corresponding to them. We begin by a careful examination of the properties of the objects to be classified, in order to find some natural connecting link amongst them. We try to detect some form of identity, and then bring together those objects amongst which the identity has been detected. The naming of the groups avowedly conforms itself to the actual distribution into the groups; it does not govern the distribution.[1]

There is no property of objects which may not be taken, if desired, as the foundation of a classification. We might, for instance, classify animals according to the colour of their eyes, or houses according to the number of windows they contain. But, clearly, such classifications would have no practical value. Our aim should be to form objects into groups respecting which the most important and the greatest number of general propositions can be made. The properties, therefore, according to which objects are classified should as far as possible be those which are sure marks of them. The test of the scientific character of a classification is the number and importance of the properties which can be regarded as common to all the objects included in a group. Properties on which the general aspect of things depends, are sometimes of com-

[1] See Jevons, *Prin. of Sci.*, pp. 673-4.

paratively trifling importance and are likely to lead us astray. The old division, for instance, into trees, shrubs, and herbs, answers to so few differences in the other properties of plants, that a classification founded on it would be both artificial and useless.[1] It often happens that natural groups must be founded not on the more obvious but on the less obvious properties of things, when these are of greater importance; and this suggests, what is certainly the fact, that an extensive knowledge of the properties of objects is almost always necessary for making a good classification of them.

§ 2. What Constitutes a Good Classification

Bain gives us the following rule of classification: *Place together in classes the things that possess in common the greatest number of attributes.*[2] Thus the vertebrate animals have been classed according to the leading points of their anatomy and physiology, rather than according to the "element" they live in (earth, water, and air). The bat flies in the air but has more real affinities with quadrupeds than birds; the whale, seal, and porpoise have warm blood and suckle their young like land quadrupeds, although living in the sea as fishes.

But importance of attributes is to a certain extent governed by purpose in view. For practical purposes, whales are classed as fishes (we speak of the whale fishery), because their living in the sea determines the manner of their being caught. So trees, shrubs, flowers, grasses, and weeds form groups of practical importance to the gardener, but do not coincide with the classifications of botany.[3]

There is evidently an almost unlimited number of modes of classifying a group of objects. From time to time different ways have been adopted of classifying plants, none completely satisfactory. Some authorities have made the form of the fruit the basis of classification; others, the number and arrangement of the parts of the corolla; others, the calyx; others, the leaves; and so forth.[4] Again, the elements may be classified according to their atomicity; according as they are metallic or non-metallic; useful or useless; abundant or scarce; solid, liquid, or gaseous. Obviously, however, elements cannot be defined as solid, liquid, or gaseous absolutely, but only within certain degrees of temperature.[5]

How much is implied in a good classification may be seen by considering the grouping of the metals that has been made by chemists.

[1] Cf Mill, *Logic*, IV, vii, §§ 1, 2. [2] *Inductive Logic*, p. 185. [3] *ib.* p. 186
[4] Cf. Jevons, *Prin. of Sci*, pp. 677-8. [5] Cf. Carveth Read, *Logic*, p. 316.

Take, for example, the alkaline metals, potassium, sodium, rubidium, cæsium, and lithium. On comparing the qualities of these metals, they are all found to combine very energetically with oxygen, to decompose water at all temperatures, and to form strongly basic oxides which are highly soluble in water, yielding powerfully caustic and alkaline hydrates from which water cannot be expelled by heat; their carbonates are also soluble in water, and each metal forms only one chloride. Such a class provides us with a powerful instrument for possible future inference. If, for example, we discovered a new metal possessing one or two of the above properties in a marked degree, we should infer that it possessed the other properties; we should, however, at once proceed to verify such inference practically.

§ 3. "Kinds" and "Types"

We often find that, after "natural" groups have been determined, especially in Botany and Zoology, one group seems gradually to shade off into the other, and that many members are on the border line of both groups. According to Dr. Whewell, natural groups cannot be circumscribed within a Definition, for they are determined by characters which do not admit of being precisely expressed in words; they are determined rather by *Type*. Propositions concerning them state not what happens in all cases, but only usually. The classes are not left quite loose; they are steadily fixed though not precisely limited; each is determined not by a boundary line without but by a central point within.[1] In Natural History we often find anomalous members of groups, which neither conform to the verbal definition nor yet differ sufficiently from the other members to be excluded from the group. We may imagine a group formed upon the basis, say, of ten qualities, but consisting of individuals that vacillate, some differing in one quality and some in another, while yet agreeing in by far the greater number. We may even make the extreme supposition that the vacillation is such that no single quality of the ten persists in every individual; hence, in strictness, there would be no common feature, and yet there would be a very large amount of resemblance.[2]

The difficulty is constantly felt by all students of Botany and Zoology. Mill himself felt it but disliked Whewell's method of overcoming it. Mill discussed at some length the possibility of dis

[1] Cf. Mill, *Logic*, Book IV, ch. vii, § 3; and Whewell, *Hist Ind. Sci.*, vol. ii, pp. 120-2.
[2] Cf. Bain, pp. 191-2.

tinctions of "kinds", that is, classes between which there is an impassable barrier; and was of opinion that the element of uncertainty could sometimes be eliminated.[1] But if we admit continuity in Nature, there must always be a doubtful margin between our groups,[2] and the notion of absolutely separate and distinct kinds must be accepted with great limitations.

Of course the recognition of a "type" is bound to involve illogical consequences. The type itself is an individual, not a class, and no other object can be exactly like the type. If some objects resemble the type in some points, and others in other points, then each definite collection of points of resemblance virtually constitute a separate class. The naturalist in his endeavour to mark out living forms in definite groups is constantly perplexed by the discovery of forms of an intermediate character. The only remedy is the frank recognition of the fact that, according to the theory of hereditary descent, gradation of characters is probably almost always the rule, and precise demarcation between groups the rare exception.[3]

The general recognition of the theory of evolution has exploded all notions about natural groups resulting from specific creations. Naturalists long held that every plant belonged to some species, marked out by invariable, never-changing characters. They acknowledged variable differences as well, and so explained sub-species and varieties. Similarly, a natural genus was a group of species, and was marked out from other genera by eternal differences of still greater importance. We now perceive, however, that the existence of any such groups as genera and species is an arbitrary creation of the naturalist's mind. All resemblances of plants are natural so far as they express hereditary affinities; but this applies as well to the variation within the species as to the species itself or to the larger groups. All is a matter of degree. The deeper differences between plants have been produced by differentiating action extending over probably millions of years; sub-species may sometimes have arisen within historical times, and varieties approaching to sub-species may often be produced by the horticulturist in a few years.[4] It is thus easy to see how specific differences may arise among the descendants of a common stock.[5]

The fixity of species in the organic world is, in fact, now entirely discredited. During a given period of a few thousand years, "kinds" may be recognized, because, under such conditions as now

[1] Cf. Mill, IV, vii, §§ 4, 5. [2] Cf. Bain, p. 192. [3] Cf. Jevons, *Prin. of Sci*, pp. 722-4.
[4] Cf. Jevons, *Prin. of Sci.*, pp. 724-8. [5] Cf. Bain, p. 190.

prevail in the world, that period of time is insufficient to bring about great changes. The horse, the dog, and the cat[1] have had a common ancestor from whose type they have gradually diverged, and their present distinctness results only from the destruction of intermediate types. Could we restore all the descendants of the common ancestor, we should find nowhere a greater difference than between offspring and parents. Of "kinds" there would be none.[2]

In practice, then, natural groups must be determined by considering not only those qualities which are strictly common to all the objects included in the group, but the entire body of qualities, all of which are found in most of those objects, and most of them in all. And hence the image which we conceive in our minds to represent the class is that of a specimen possessing the whole of the qualities in a high degree, for such a specimen is alone really fitted to show clearly what those qualities are. It is by a mental reference to this standard that we usually and advantageously determine whether any individual or species belongs to the class or not.[3]

§ 4. Principles of Logical Division

In order that a classification may facilitate the study of a particular phenomenon, we must first bring into one class all kinds of things which exhibit that phenomenon, in whatever variety of forms or degrees; and secondly we must arrange these kinds in a *series* according to the degree in which they exhibit it. We must therefore be able to recognize the essential similarity of a phenomenon, in its minuter degrees and obscurer forms, with what is called the *same* phenomenon in its most complete development.

The main principle of division must of course be natural affinity; the classes formed must, as far as possible, be natural groups, but the principles of natural grouping must be applied in subordination to the principles of a natural series. The groups must not be so constituted as to place in the same group things which ought to occupy different points of the general scale. The precaution necessary to be observed for this purpose is that the *primary* divisions must be grounded not on all distinctions indiscriminately, but on those which correspond to well-marked variations in the degree of the

[1] This is, of course, quite a haphazard choice; any or all animals might be mentioned instead.

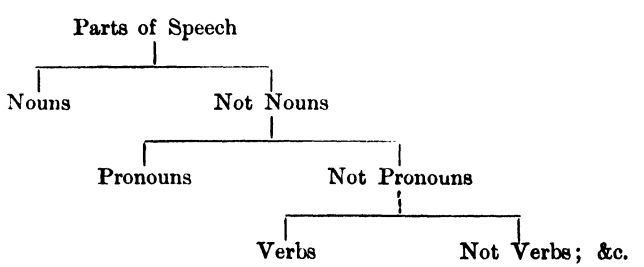
[2] Cf. Carveth Read, *Logic*, p. 314.—But there are many perplexing difficulties and many unsolved problems in connection with the accepted doctrine of evolution through the medium of natural selection. For some of these, the reader may refer to Professor Bateson's six lectures on Genetics, given at the Royal Institution in Jan. and Feb., 1912.

[3] Cf. Mill, IV, vii, § 5.

main phenomenon. In classifying animals, for example, the series as a whole should be broken into parts at the points when the variation in degree begins to be attended by conspicuous changes in the various animal properties.[1] Such well-marked changes take place, for instance, where the class mammalia ends; at the point where the fishes are separated from insects; insects from mollusca; and so on.[2]

Apart, however, from the strictly logical difficulties already mentioned, no classification of animals can ever be perfect until the present great gaps in our knowledge of animal life are filled up. For a perfect classification of animals, two conditions are necessary: (1) a full knowledge of the adult structures of every animal, recent and extinct; (2) the mode of development of every animal. For, as Huxley says, it is the sum of all the structural conditions of an animal which constitutes the totality of its structure; and if two animals, similar in their adult state, were unlike in their development, it is clear that the latter circumstance would have to be taken into account in determining their position in a classification.[3] Now these conditions are impossible to meet, for our knowledge is still very incomplete. Thus it comes about that whatever classification of the animal kingdom is adopted, it is open to logical criticism. The very best grouping is necessarily subordinate to our present state of knowledge.

To obtain a strictly logical division, every superior class should be divided into two inferior classes, distinguished by the possession or non-possession of a single specified difference. Each of these minor classes is again divisible by any other quality whatever which can be suggested. Every such classification may be called *bifurcate*. Theoretically, such a method is alone productive of a system logically perfect. For example:



[1] Applied to animals, the logical term "properties" seems awkward, but the meaning is obvious.

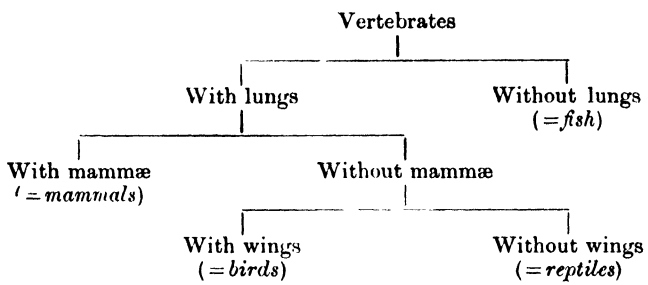
[2] Cf. Mill, Book IV, ch. viii, §§ 4, 5.

[3] Cf. Mill, IV, viii, § 5; and Huxley, "Animal Kingdom", *Ency. Brit.*

But such a plan is usually cumbrous, and at each step the negative term is entirely undefined in its extent.[1] In practice, therefore, it is seldom used, though its logical necessity was insisted on by Bentham, who took, for an example, the vertebrate animals and divided them into four classes as follows:—

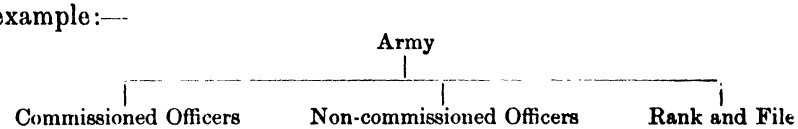
Mammals: having mammæ and lungs.
Birds: having lungs and wings but not mammæ.
Fish: not having lungs.
Reptiles: having lungs but not mammæ or wings.

We have then, according to Bentham, this bifurcate division:—



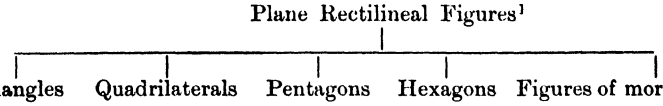
But, as Jevons points out, even this scheme is theoretically imperfect. The sub-class mammals must either have wings or not; we must either subdivide this class, or assume that none of the mammals have wings, which is, as a matter of fact, the case (the wings of bats not being true wings in the sense of wings applied to birds). Fish, again, ought to be considered with regard to the possession of mammæ and wings; and in leaving them undivided we really imply that they never have mammæ or wings (the wings of flying-fish being again no exception).—Although the example affords an illustration of logical division which, for practical purposes, is perfect, it shows the extreme difficulty of conforming to the rigorous demands of theory.[2]

The strictly bifurcate form of division may often be dispensed with, and yet the logical aspects be strictly maintained; for example:—



[1] See Welton, *Logic*, vol. ii, p. 130.
[2] Cf. Jevons, *Prin. of Sci.*, pp. 604–8; Bentham, *New System of Logic*, p. 115.

And it is quite unnecessary to adopt the bifurcate plan to classify things admitting of numerical discrimination; for example:—



And perhaps the same remark applies in the case of a classification of, for instance, the different countries of Europe. On the other hand, we might make our first step consist of France and not-France; then not-France might be divided into Germany and not-Germany; and so on. Absurd as this seems, logicians are by no means unanimously of opinion that we could, with strict logical consistency, adopt any other basis of division.[2]

The ordinary rules of logical division should always be borne in mind: (1) there must be only one basis of division (and the subclasses will therefore be mutually exclusive; (2) the division must be exhaustive; (3) in continued division, each step must be a proximate one; and (4) the division must be appropriate.[3] The necessity for the last rule will be seen if a proposal be made to classify, for instance, the boys in a school according to whether their names consisted of one or more syllables.

Difficult and practically impossible as strictly logical classification is in Botany and Zoology, it is comparatively simple in Chemistry. Taking nature generally, however, rigorously logical grouping is more often impossible than not. There are, for instance, substances varying by insensible degrees; the granites are a case in point. Similar difficulties confront us in any attempt to classify odours, the emotions, human faces, personal characters, and so forth. In such cases, diversity is far too great for logical classification to be possible.

§ 5. Definition

Closely associated with Classification is the question of Definition. We have already referred to the Scholastic mode of defining, and it will suffice here to add, by way of further suggestion, an instructive instance of Bain's method of working out the definition of a common term. We will take the term *Food.*

We begin by assembling representative examples of all the substances ever recognized under this name. We have before us the flesh of animals, the esculent roots, fruits, leaves, &c. We have also a number of substances of purely mineral origin, as water and com-

[1] Cf. p. 21. [2] *ib* [3] See Bain, p. 196, or any standard work on Logic.

mon salt. Our work lies "in generalizing these, in detecting community in the midst of much difference".

Were man a purely carnivorous feeder, his food might be generalized as "the flesh of animals taken into the mouth and passed into the stomach, to be there digested and thence to be applied to the nourishment and support of the system". But when we include vegetable and mineral bodies, we must leave out "flesh", and substitute "animal, vegetable, and mineral substances"; the other part of the statement being applicable. Even as amended, however, the definition is still tentative, and needs to be verified by comparison in detail with everything that is ever put forward as food. We must challenge all informed critics to say where the definition fails. Thus, nourishment is afforded by substances absorbed through the skin, a fact which would invalidate the exclusive mention of the medium of the mouth and stomach, and narrow the definition to nourishing and supporting the system. Again, it is doubtful whether alcohol and tea nourish the system. This is a far more serious objection; and the manner of dealing with it will illustrate the principles of defining.

In the first place, there may be a contest as to the matter of fact. Could it be shown that these substances do give nourishment and support to the system, the difficulty is at once overcome; in that case they fall at once under the definition. On the contrary supposition —that they do not nourish and support the system, two courses are open. First, we may exclude them from the class "food", and retain the definition. Or, secondly, we may include them, and alter the definition. As modified to suit the extension, the definition would be, "substances that either nourish and support or stimulate the system". To decide between those two courses, we must refer to the golden rule of classification, which recommends the adherence to a smaller class based on the greatest number of important qualities, rather than to a larger where the properties in common are reduced to comparative insignificance. It is better, therefore, to retain two groups—Foods and Stimulants—each with its own Definition. In that way we should derive much more information respecting any individual thing designated either "Food" or "Stimulant", than if the word "Food" covered both. It may be that some substances, beef tea, for instance, combines both functions, which would entitle them to be named in both classes.[1]

[1] See Bain, *Induc. Logic*, pp. 158-9. Of course much more is now known about Foods and Stimulants than when Bain wrote this, but the principle underlying the general argument is unaffected.

It has already been pointed out that the need for exact definitions of terms very seldom arises, until the terms are actually used in assertions.[1]

CHAPTER XIX

The Analysis of Phenomena

§ 1. Unsuspected Associations of Phenomena

In a former chapter[2] it was stated that the association of ideas, by blending together things that are really distinct in their nature, tends to introduce perplexity and error into every process of reasoning in which we may be engaged.

One intimate association between two ideas which have no necessary connection is that which exists in every person's mind between *colour* and *extension*. The word "colour" expresses a sensation in the mind; "extension" denotes a quality of an external object. There is no more real connection between two such notions than between pain and solidity. And yet, in consequence of our always perceiving extension at the same time as the sensation of colour is excited in the mind, we find it impossible to think of that sensation without conceiving extension along with it.[3]

Again, most people are under the impression that the relation which the different notes of the musical scale bear to one another, and the relation of high and low position among material objects, are analogous. They seem to think that the notes to the right of the piano are "higher" than those to the left, that an acute sound is higher than a grave sound. But the association is entirely misleading, and is, in fact, the very reverse of an association once equally prevalent. The more ancient of the Greek writers looked upon grave sounds as high, and acute ones as low; the present mode of expression is a later innovation.[4] There is, of course, no scientific reason for calling acute or grave sounds high or low, though the practice perhaps arose from the relative positions of the strings in ancient instruments.[5]

[1] See ch. ii.
[2] On Locke.
[3] Cf. Dugald Stewart, *Phil. of Human Mind*, p. 184.
[4] Cf. Dugald Stewart, *op. cit.* p. 185, and Gregory's *Euclid*, preface.
[5] Cf. Dr. Beattie's *Essay on Poetry and Music*; and Dugald Stewart, p 560.

If, then, such erroneous associations remain unsuspected, and we proceed to reason from the underlying "facts", our reasoning will probably be fallacious. The necessity for disentangling our facts thus becomes evident. It has been well said that progress in scientific investigation depends much more on that severe and discriminating judgment which enables us to separate ideas that nature or habit has closely combined, than on acuteness of reasoning or fertility of invention. Whenever two subjects of thought are intimately connected in the mind, it requires the most determined effort of attention to conduct any process of reasoning which relates to only one.

Since one of the main objects of Science is to ascertain the laws which regulate the succession of events in nature, the investigator has constantly to deal with different events presented to him nearly at the same time, and he has therefore to be particularly careful that phenomena closely connected in time do not mislead him into thinking that they are necessarily invariably conjoined. The disposition to confound together accidental and permanent connections is one great source of popular superstitions,—palmistry, phrenology, planetary influence, haunted houses, miraculous wells, unlucky days, and so on. Such combinations are confined, in great measure, to uncultivated and unenlightened minds, but there are other accidental combinations which are apt to lay hold of the minds of even the very ablest of investigators.[1]

We have already seen that when a phenomenon is preceded by a number of different circumstances, we cannot determine, by any *a priori* reasoning, which of these circumstances are to be regarded as the constant, and which the accidental, antecedents of the effect. If, in the course of our experience, the same combination of circumstances is always exhibited to us without any alteration, and is invariably followed by the same result, we must necessarily remain ignorant whether the result be connected with the whole combination, or with only one or a few of the circumstances combined; and therefore if at any time we wish to produce a similar effect, there is no alternative but to imitate in every particular circumstance the combination which we have seen.

Let us suppose, for instance, that a savage who, on some occasion, had found himself relieved of some bodily ailment by a draught of cold water, is a second time afflicted with a similar disorder and is desirous of repeating the same remedy. With the limited degree

[1] Cf. Dugald Stewart, *op. cit.* pp. 186-7.

of knowledge and experience which we have here supposed him to possess, it would be impossible for the greatest of modern investigators, in his situation, to determine, whether the cure was due to the water which was drunk, to the cup in which it was contained, to the fountain from which it was taken, to the particular day of the month, or to the particular age of the moon. In order, therefore, to ensure the success of the remedy, the savage will, very naturally and very wisely, copy, as far as he can recollect, every circumstance which accompanied the first application of it. He will make use of the same cup, draw the water from the same fountain, hold his body in the same position, and turn his face in the same direction; and thus all the accidental circumstances in which the first experiment was made, will come to be associated equally in his mind with the effect produced. The fountain from which the water was drawn will be considered as possessed of particular virtues; and the cup from which it was drunk will be set apart for exclusive use on all future similar occasions.

Now the mind cannot be cured of these associations by any progress in the art of reasoning; the cure can be effected only by an enlargement of experience. It is experience alone which will teach us to break up, and how to break up, complex phenomena into its parts, to combine them together again in various ways, and to observe the effects which result from these different experiments. It is only after we have eliminated from physical causes their accidental and unessential concomitants that we can ascertain with precision the general laws of nature.[1]

But in spite of every care we may take in analysing the phenomenon under investigation, we may quite probably find it impossible to effect the pure connection between the conditions and their consequences, unimpeded by any irrelevant details. Much of the "residue" in the phenomenon, which we are presuming to be indifferent and leaving out of account, is incapable of actual removal. The exclusion from consideration of this residue is, therefore, only justified as far as the residue has been analysed.[2]

§ 2. Herschel on the Analysis of Phenomena

Herschel's remarks[3] on the analysis of phenomena are so valuable and instructive that we cannot do better than summarize them here.

[1] Cf. Dugald Stewart, *op. cit.* pp. 188–9.
[2] Cf. Welton, *Logic*, vol. ii, pp. 121–41.
[3] *Nat. Phil.*, Part II, ch. iii.

Phenomena, or appearances, as the word is literally rendered, are, says Herschel, the sensible results of processes carried on among external objects, of which they are, so to speak, *signals*, conveyed by the wonderful mechanism of our sense organs to our minds, which receive and review them, and by habit and association connect them with corresponding qualities in the objects; just as a person writing down and comparing the signals of a telegraph might interpret their meaning.

Now these processes themselves may be in many instances *analysed* and shown to consist in the motions or other affections[1] of the external objects. For instance, the phenomenon of the sound produced by a musical string, or a bell, when struck, may be shown to be the result of a process consisting in the rapid *vibratory motion* of its parts communicated to the air, and thence to our ears; though the intermediate effect on our organs of hearing does not seem to suggest the slightest idea of such a motion.

On the other hand, there are innumerable instances of sensible impressions which we seem to be incapable of tracing beyond the mere sensation; for example, the sensations of bitterness and sweetness. These, therefore, if we were inclined to form hasty decisions, might be regarded as ultimate qualities; but the instance of sounds, just mentioned, alone would teach us caution in such decisions, and incline us to believe them to be mere results of some secret process going on in our organs of taste, too subtle for us to trace.

§ 3. His Remarks on our Notions of Force

There seems to be little hope of attaining a knowledge of the ultimate and inward processes of Nature. Let us, for instance, consider the production of motion by the exertion of force. We are conscious of a power to move our limbs, and, by their intervention, other bodies. We are also conscious that this effect is the result of a certain inexplicable process by which we exert *force*. And even when such exertion produces no visible effect, as when we press our two hands violently together in such a way as just to oppose each other's effect, we still perceive, by the fatigue and exhaustion, that something is going on within us, of which the will is the determining cause. The impression which we receive of the nature of force from our own effort and the sense of fatigue is quite different from that which we obtain of it from seeing the

[1] i.e. those qualities of bodies by which they directly *affect* the senses.

effect of force exerted by others in producing *motion*. Were there no such thing as motion, had we, for instance, been from infancy shut up in a dark dungeon and every limb encrusted with plaster, the internal consciousness would give us a complete idea of *force*; but when set at liberty, habit alone would enable us to recognize its exertion by its *signal*, motion, and *that* only by finding that the same action of the mind which in our confined state enables us to fatigue and exhaust ourselves by the tension of our muscles, puts it in our power, when at liberty, to *move* ourselves and other bodies. But how obscure is our knowledge of the process going on within us in the exercise of this movement (by virtue of which alone we act as direct *causes*), we may judge from the fact that when we put any limb in motion, the seat of the exertion seems to us to be *in* the limb, a conclusion which is demonstrably wrong.

This one instance of the obscurity which hangs about the only act of direct *causation* of which we have an immediate consciousness, will suffice to show how little prospect there is that, in the investigation of nature, we can ever arrive at a knowledge of ultimate causes. We must be satisfied with a knowledge of *laws*, and to obtain these we begin by analysing every particular complex phenomenon presented to us, and continuing the process until the resolved constituents are the most simple and elementary obtainable.

§ 4. His Analysis of the Phenomenon of Sound

Herschel now proceeds to give an instance of the analysis of a complex phenomenon. Let us, he says, take the phenomenon of sound, and, by considering the various cases in which sounds of all kinds are produced, we shall find that they all agree in these points:—

1. The excitement of a *motion* in the sounding body.
2. The communication of this motion to the air or other medium which is interposed between the sounding body and our ears.
3. The propagation of such motion from particle to particle of such medium in due succession.
4. Its communication, from the particles of the medium adjacent to the ear, to the ear itself.
5. Its conveyance in the ear, by a certain mechanism to the auditory nerve.
6. The excitement of *sensation*.

Now in this analysis we notice that two principal matters must be understood before we can have a true and complete knowledge of sound:—

1. The excitement and propagation of *motion.*
2. The production of *sensation.*

These, therefore, appear to be the elementary phenomena into which the complex phenomenon of sound resolves itself.

But, again, if we consider the communication of *motion* from body to body, or from one part to another of the same body, we shall perceive that it is again resolvable into several other phenomena:—

1. The original setting in motion of a material body, or any part of one.
2. The behaviour of a particle set in motion, when it meets another lying in its way, or is otherwise impeded or influenced by its connection with surrounding particles.
3. The behaviour of the particles so impeding or influencing it in such circumstances.

   The last two suggest another phenomenon which it is necessary also to consider, viz.:—
4. The phenomenon of the connection of the parts of material bodies in masses, by which they form aggregates, and are enabled to influence each other's motions.

Thus we see that an analysis of the phenomenon of sound leads to the enquiry—

1. Into two *causes*, viz.,

   (*a*) The cause of *motion*,

   (*b*) The cause of *sensation*,

   these being phenomena which we seem to be unable to analyse further, and we therefore set them down as simple, elementary, and referable, for anything we can see to the contrary, to the immediate action of their causes.
2. Into several questions relating to the connection between the motion of material bodies and its cause; for example,

   (*a*) *What will happen* when a moving body is surrounded on all sides by others not in motion?

   (*b*) *What will happen* when a body not in motion is advanced upon by a moving one?

   It is evident that the answers to such questions as these can be no others than *laws of motion.*

Lastly, we are led, by pursuing the analysis and considering the phenomenon of the aggregation of the parts of material bodies, and the way in which they influence each other, to two other general phenomena, namely, the *cohesion* and *elasticity* of matter; and these we have no means of analysing further, and must therefore regard them (until we see reasons to the contrary) as *ultimate phenomena*, and referable to the direct action of causes, namely, an attractive and a repulsive *force*.[1]

§ 5. The Limits of such an Analysis

Of *force*, as counterbalanced by opposing force, we have, as already said, an internal consciousness; and though it may seem surprising that matter should appear capable of exerting on matter the same kind of effort, yet we feel bound to accept the direct evidence of our senses; and this seems to show us that when we keep a spring stretched with one hand, we feel our effort opposed exactly in the same way as if we had ourselves opposed it with the other hand, or as it would be by that of another person. The inquiry, therefore, into the aggregation of matter resolves itself into the general question,—What will be the behaviour of material particles under the mutual action of opposing forces capable of counterbalancing each other? and the answer to this question can be no other than the announcement of the *law of equilibrium*, whatever law that may be.

With respect to the *cause of sensation*, it must be regarded as much more obscure even than that of motion, inasmuch as we have no conscious knowledge of it.[2]

Dismissing, then, as beyond our reach, the inquiry into *causes*, we must be content, at present, to concentrate our attention on the *laws* which prevail among phenomena, and which seem to be their immediate results. From the instance just given, it is evident that every inquiry into the intimate nature of a complex phenomenon, branches out into as many different and distinct inquiries as there are elementary phenomena into which it may be analysed; and that, therefore, it would greatly assist us in our study of nature, if we could, by any means, ascertain what *are* the ultimate phenomena into which all the composite ones presented by it may be

[1] *ib.* pp. 88-90

[2] The chasm between the physical and the psychical is still unbridged. Subjective Psychology can throw no light whatever on the cause of sensation, and Physiology—so far—very little.

resolved. Clearly this can be ascertained—if at all—only by going to Nature itself; and just as the analytical chemist accounts every ingredient an *element* until it can be resolved into others, so, in investigation generally, we must account every phenomenon as an elementary or simple one till we can analyse it and show that it is the result of others, which in their turn become elementary. Thus, in a modified and relative sense, we may still continue to speak of causes, not intending thereby *ultimate* causes, but those *proximate* causes which connect phenomena with others of a simpler, higher, more general, or elementary kind. For example, we may regard the vibration of a musical string as the proximate cause of the sound it yields, receiving it so far as an ultimate fact, and deferring the inquiry into the cause of vibrations, which is of a higher and more general nature. It may, however, often happen that although we become fully aware of the complexity of a phenomenon, we are entirely unable to analyse it.[1]

In pursuing the analysis of any phenomenon, the moment we find ourselves stopped at a point beyond which further analysis seems impossible, we are forced to refer, at least provisionally, the constituent element we have now reached to the class of ultimate facts; and the study of this elementary constituent and of its laws becomes a separate branch of Science. On those phenomena which are most frequently encountered in an analysis of nature, and which most decidedly resist further decomposition, it is evident that the greatest pains and attention ought to be bestowed. They furnish the key to the greatest number of inquiries, and in them we must look for the direct action of causes. Now by far the most general phenomenon with which we are acquainted, and that which occurs most constantly, in every inquiry upon which we enter, is *motion*. Dynamics, then, is a branch of knowledge, a thorough study of which is imperative. Happily, it is one in which a very high degree of certainty is attainable, a certainty in no way inferior to mathematical demonstration.

Unfortunately, no general rules of procedure can be laid down for the analysis of a complex phenomenon into simpler ones.[2] Success comes from experience, patience, insight, and a careful study of the work of successful investigators.

[1] *op. cit.* pp. 90-3. [2] *ib.* pp. 93-6.

CHAPTER XX

Generalization and Empirical Laws

§ 1. The Meaning of Generalization

The Scholastic Logicians "generalized" by merely omitting all those qualities which distinguished the observed particulars from one another. But the term *generalization* as now commonly used, seems to include two distinct processes, though these are often closely associated together.

In the first place, the term is used when, even in two objects, there is a recognition of something in common. The slightest similarity seems almost of necessity to lead to an inference of some kind from one case to another.[1]

In the second place the term *generalization* is often used to denote the process of passing from a limited number of facts, or from a partial law, to a multitude of unexamined cases, which we believe to be subject to the same conditions. In the cases actually examined, we have done more than merely recognize similarity; we feel that we have been able to detect the conditions which invariably accompany and determine those cases. Hence, in generalizations of this kind, we may be said to endow ourselves with a power of prediction of more or less probability. Having observed, for example, that many substances assume, like water and mercury, the three states of solid, liquid, and gas, and having assured ourselves by frequent trial that the greater the means we possess of heating and cooling, the more substances we can vaporize and freeze, we pass confidently in advance of fact, and assume that all substances are capable of these three forms.[2]

It is generalization in this second sense that enters so largely into the work of Science. The essence of the process consists in the discovery, in a group of observed phenomena, of those invariable conditions which determine the common nature of the phenomena. We do not generalize from number of instances as such; validity in no way depends on the mere number of instances examined. Complete knowledge may, in favourable cases, be attained by the careful analysis of a single instance. The validity of the generalization rests upon the fundamental assumption that every elementary fact

[1] Cf. Welton, *Logic*, vol. ii, pp. 191-2.

[2] Cf. Jevons, *Prin. of Sci.*, pp. 597-8.

of Nature is always definitely determined in precisely the same way, so that the relation between a phenomenon and its conditions cannot vary.[1]

§ 2. Generalizations Vary in Degree

In the earlier stages of scientific inquiry, generalizations are necessarily more or less empirical. We have first to be satisfied with "facts", which we check and correct, label and classify, and thus do our best to ensure accuracy and orderliness. All this must necessarily precede any discovery of determinate conditions. That which has to be "explained" must be clearly and distinctly set out before we can attempt to explain it. In other words, empirical generalizations must precede generalizations of strict determination; we must first ascertain the *Laws* of phenomena; the consideration of *Causes* must come later.[2] Obviously, then, our generalizations vary greatly in finality; but as our knowledge grows, so they increase in comprehensiveness and certainty.[3]

Certain established natural laws are held to be true of all matter in the universe absolutely, without exception, no instance to the contrary ever having been noticed. This is conspicuously true of the law of universal gravitation, and of Newton's laws of motion. These, therefore, we may regard as ultimate laws, at all events for the present. But by far the greater number of properties of matter vary in degree; substances are more or less dense, more or less transparent, more or less magnetic; and so on. One common result of the progress of Science is to show that qualities once supposed to be entirely absent from many substances are really present, though in so low a degree of intensity that the means of detection were insufficient. Newton, for instance, believed that most bodies were quite unaffected by the magnet. Faraday and Tyndall have rendered it very doubtful whether any substance whatever is wholly devoid of magnetism.[4] Thus are generalizations, once commonly accepted, shown to be incorrect.

Then phenomena which are in reality of a closely similar or even identical nature may present to the senses very different appearances, and so, again, we may be led to make false generalizations. Without a careful analysis of the changes which take place, we may

[1] Cf. Welton, *Logic*, ii, p. 194; also ch. xv, §§ 1 and 9.
[2] See Welton, ii, p. 196; and Whewell, *Nov. Org. Ren.*, p. 118.
[3] Cf. Jevons, *Prin. of Sci.*, p. 600.
[4] *ib.* pp. 603-7. Magnetism here includes "diamagnetism

often be in danger of widely separating facts and processes, which are actually instances of the same law. Extreme difference of degree or magnitude is a frequent cause of error. It is, for instance, difficult, for the moment, to recognize any similarity between the gradual rusting of a piece of iron and the rapid combustion of a heap of straw. Yet Lavoisier's chemical theory was founded upon the similarity of the oxidizing process in the two cases. We have only to take iron in a finely divided state to show that it is the more combustible of the two.[1]

§ 3. Empirical Laws

Scientific investigators give the name of *Empirical Laws* to those uniformities which observation or experiment has shown to exist, but on which they hesitate to rely in cases varying much from those which have been actually observed, because they have not yet been able to see any reason *why* such a law should exist. It will be seen, therefore, that the very notion of an empirical law implies that it is not an *ultimate* law; that, if true at all, there must be an explanation, which should be sought and found. It is a *derivative* law, the derivation of which is not yet known. To state the explanation, the *why*, of the empirical law, would be to state *the laws from which it is derived.* And if we know these, we should also know under what conditions it would cease to be fulfilled. But these higher laws may not yet be identical with the *ultimate* laws of causation; they may be of an intermediate class, requiring still further derivation.

1. That snow is always to be found on high mountains was at one time an empirical law. The law was based on observation, but was not susceptible of being explained or referred to any higher generalizations. We can now resolve it into the laws connected with radiant heat passing through the atmosphere, and we may therefore regard it as being derived from such laws. These may not themselves be the highest attainable generalities; still they are much more general than the stated uniformity connecting snow and height.

2. The fact that water always rises in pumps was an empirical law previous to the discovery of the pressure of the atmosphere. The application of the Method of Agreement, in different countries, and with pumps of different bores, proved that no pump could draw water beyond about 33 ft. The law could be relied on within the wide limits of place and circumstances where it had been tried.

[1] Cf. Jevons, *op. cit.* p. 611.

It could not have been extended to other planets, but it might be extended with apparent safety to any part of the earth. But on the discovery of the pressure of the atmosphere, the empirical law passed into a law of higher generalization; its limits of operation were precisely defined. The new discovery explained why the water could not rise much beyond 33 ft.; why the height varied at different times; why, on a high mountain, the rise was considerably less than at the sea level; and so on. It is conceivable that, some time or other, these explanations might have been empirically discovered by sufficiently wide and careful experiments, but it is very unlikely. In any case the *derivation* superseded such a laborious task.

3. That our breathing animals are hot-blooded is a law formerly empirical, but now *derived* from the general law of the dependence of temperature on the oxygenation of the blood.

4. The periodical return of eclipses, as originally ascertained by the observation of the early astronomers, was an empirical law until the general laws of the celestial motions had explained it.

The following are empirical laws still waiting to be resolved into the simpler laws from which they are derived.—The local laws of the flow and ebb of the tides in different places; the succession of certain kinds of weather to certain appearances of the sky; the apparent exceptions to the almost universal truth that bodies expand by increase of temperature; the law that gases have a strong tendency to permeate animal membranes; the law that when different metals are fused together the alloy is harder than the various elements; the law that substances containing a very high proportion of nitrogen (such as morphia and hydrocyanic acid) are powerful poisons.

An empirical law, then, is an observed uniformity, presumed to be resolvable into simpler laws, but not yet resolved into them. It is a law which awaits explanation.[1]

§ 4. The "Joint Action" of Causes

All possible empirical laws, and all possible uniformities of less generality, seem to be resolvable into a very limited total number of ultimate laws of causation. Conversely, from this limited number of ultimate laws of causation, a vast number of uniformities, both of succession and coexistence, must necessarily be derived. Now the order of succession or coexistence which obtains among effects neces-

[1] Cf. Mill, *Logic*, Book III. ch. xvi, §§ 1, 2; Bain, *Inductive Logic*, pp. 104-8.

sarily depends on their causes. "If they are effects of the same cause, it depends on the laws of that cause; if on different causes, it depends on the laws of those causes severally, *and on the circumstances which determine their coexistence.*" If we trace the coexistence of these causes back, the different effects may meet at a point, and the whole is thus shown to depend ultimately on some common cause; or they may terminate in different points, and the order of succession and coexistence of the effects is thus proved to have arisen from the "collocation"[1] or joint action of some of the primeval causes or natural agents.

Derivative laws, therefore, do not depend solely on the ultimate laws into which they are resolvable; they mostly depend on those ultimate laws *and an ultimate fact*, namely, the mode of coexistence or joint action of some of the component elements of the universe. For example, the order of succession and coexistence among the heavenly motions, which is expressed by Kepler's Laws, is derived from the coexistence or joint action of two primeval causes,—the sun's attractive force, and the original projectile force belonging to each planet. But the sun's attraction and the original projectile force coexist and act together *in a certain ratio*,—a ratio which is productive of regular elliptical motions. But this ratio *might* have been entirely different, in which case the motions would have been different, though still regular.

Now *why* the sun's attraction and the force in the direction of the tangent coexist and act together in the exact proportion they do, we do not know, and we cannot trace any coincidence between it and the proportions in which any other elementary powers in the universe thus coexist or act together. The utmost disorder is, in fact, apparent in the combination of causes generally. In the resolution of derivative laws, then, we have to consider not only the ultimate laws from which they are derived, but also an element which is not a law of causation, an element in which there is no uniformity, no principle, no rule. It is an elusive element of joint action, and it defies explanation.

We now see why investigators place only a limited degree of reliance upon derivative laws. A derivative law which results wholly from the operation of some one cause will be as universally true as the laws of the cause itself, but where the law results from

[1] This term of Mill's is not a very happy one; "co-action" would be better, except that as commonly used it implies a certain amount of compulsion; "joint action" seems most nearly to suggest the correct idea.

effects of several causes, there may be a variation in the mode of coexistence or joint action of these causes; and as we are necessarily ignorant of the nature of this coexistence or joint action, we are not safe in extending the law beyond the limits of time, place, and circumstances, in which we have actual experience of its truth.[1] The elliptic motion of the planets, for example, would be fundamentally modified if some great disturbing body were sufficiently near to counteract solar attraction, or if the tangential force were made different from what it is. Hence we cannot extend the law of the ellipse to every body that may now or at any future time revolve about the sun.[2]

We have seen that, by the Method of Agreement alone, we can never arrive at causes. Hence, no generalization can be more than an empirical law when the only proof rests on that method. It therefore follows that almost all results obtained by simple observation without experiment must be considered empirical only,—derivative laws, the derivation of which has not been traced.[3]

§ 5. The Detection of Derivative Laws

Suppose we have determined, in an observed uniformity, some law of causation. By what signs are we to judge that it is not an ultimate law but an unresolved derivative law?

One sign would be any evidence, between the antecedent *a*, and the consequent *b*, of some intermediate link, some phenomenon of which we can surmise the existence, though from the imperfection of our senses or of our instruments, we are unable to ascertain its precise nature and laws. If we denote the link by *x*, it follows that, even if *a* be the cause of *b*, it is but the remote cause, and that the law *a* causes *b* is resolvable into at least two laws, *a* causes *x*, and *x* causes *b*. This is frequently the case, since the operations of nature mostly take place on so minute a scale that many of the successive steps are either imperceptible or very indistinctly perceived.

Take the case, for instance, of the "explosion" of a mixture of oxygen and hydrogen to form water.[4] All that we see of the process is, that the two gases are mixed in certain proportions, that an electric spark is passed, an explosion takes place, the gases disappear, and water vapour comes into being, and that there is diminution of volume. There is no doubt about the law, no doubt about

[1] Cf. Mill, *Logic*, Book III, ch. xvi, §§ 2, 3, 4.
[2] Cf. Bain, p. 107.
[3] Mill, *ib.*
[4] Cf. ch. xii, § 8.

the causation. But between the antecedent (the mechanical mixture of gases to which the electric spark is applied) and the consequent (the production of water), there must be an intermediate process which we do not see. We find that every portion of the water vapour, even the smallest portion our instruments are capable of appreciating, contains oxygen and hydrogen in the ratio of 1:2, and we feel no doubt that the minutest perceptible portion contains the same elements in the same ratio. Hence, we are driven to the conclusion that still more minute portions of oxygen and hydrogen must have come together in every such minute portion of space; and, since the volume of water vapour is less than the original mixture, they must have come closer together. If we grant the truth of the atomic theory, there must have been a breaking asunder of the combined atoms in each oxygen and hydrogen molecule, a rearrangement of all these atoms, and a recombination into water molecules. If the whole process could be slowed down from the fraction of a second to, say, an hour, and rendered visible, can we doubt that a succession of wonderfully interesting phenomena would be presented to us? There is probably a chain of innumerable intervening links between *a* and *b*, the antecedent and consequent with which alone we are familiar. And so it must be generally, in all chemical processes both in the inorganic and organic worlds.

A second sign from which a law of causation, though hitherto unresolved, may be inferred to be a derivative law, is when the antecedent is a complex phenomenon. Take the case of a diminution of the pressure of the atmosphere (indicated by the fall of the barometer) followed by rain. The antecedent is a complex phenomenon: the column of atmosphere consists of a column of air and a column of aqueous vapour mixed with it; and the change in the two together, shown by a fall in the barometer and followed by rain, must be either a change in one of these, or in the other, or in both. We might, then, even in the absence of any other evidence, form a reasonable presumption, from the invariable presence of both these elements in the antecedent, that the sequence is probably not an ultimate law but a result of the laws of the two different agents, a presumption only to be destroyed when we had made ourselves so well acquainted with the laws of both as to be able to affirm that those laws could not by themselves produce the observed result.[1]

[1] Cf. Mill, *Logic*, III, xvi, § 6.

§ 6. The Meaning of "Law".

A great deal has been written about the precise significance of the term "Law", and as objection is often raised to its use in scientific investigation, a few remarks on the subject will not be out of place.

According to Sidgwick, "Laws" may be defined as Rules of Conduct which we are morally bound to obey; or, more briefly, commands imposed by Rightful Authority.[1]

"It is essential to the idea of *law* that it be attended with a sanction; or, in other words, a penalty or punishment for disobedience."[2]

Our human laws, says Froude, are but the copies, more or less imperfect, of the eternal laws, so far as we can read them, and either succeed and promote our welfare, or fail and bring confusion and disaster, according as the legislator's insight has detected the true principle, or has been distorted by ignorance or selfishness.

The eminent jurist, Blackstone, regarded "the law of gravitation, the law of nature, and the law of England", as different examples of the same principle,—as rules of action or conduct imposed by a superior power on its subjects. The Creator "endued matter with a principle of mobility, and established certain rules for the perpetual direction of that motion; so, when he created man, he laid down certain immutable laws of human nature".[3]

But Austin, the great writer on jurisprudence, insisted, with much energy, on the necessity for a distinction; he pointed out that some of these laws are commands, while others are not commands. The so-called laws of nature are not commands; they are uniformities which resemble commands only in so far as they may be supposed to have been ordered by some intelligent being. But they are not commands in the only proper sense of the word,—they are not addressed to reasonable beings who may or may not will obedience to them. Laws of nature are not addressed to anybody, and there is no possible question of obedience or disobedience to them. Austin pronounces these as laws improperly so called.

But law in the scientific sense has acquired a position of its own from which it is impossible to dislodge it; and it involves none of the ambiguities and confusions against which Austin protested. The conceptions of law by the jurist and by the man of science are

[1] *Method of Ethics*, p. 269.
[2] A. Hamilton, *Federalist*, pp. 210–5.
[3] Blackstone, *Extracts*, p. 10.

now entirely distinct.[1] We may regard the word law in its scientific sense to be a borrowed metaphor; a law in the judicial sense has the characteristic of *uniformity*, and it is from this characteristic alone that "law" can be employed to signify order in nature.[2]

CHAPTER XXI

Hypotheses

§ 1. What an Hypothesis is

In the earlier years of the last century, a Manchester schoolmaster named John Dalton, who had undertaken an investigation into the chemical composition of various substances, analysed two gases, olefiant gas and marsh gas, both of which consist of carbon and hydrogen, and obtained the following results:—

> Olefiant gas, 85·7 % of carbon and 14·3 % of hydrogen.
> Marsh gas, 75 % of carbon and 25 % of hydrogen.

On comparing these numbers, he found that the ratio of carbon to hydrogen in olefiant gas is 6:1, whereas in marsh gas it is 3:1 or 6:2. The mass of hydrogen, combined with a given mass of carbon, is therefore exactly twice as great in the one case as in the other. This research was followed by those on the composition of the oxides of carbon and nitrogen. In all these compounds a regularity of composition was found, and the uniformity led Dalton to formulate the *empirical law* of "Multiple Proportions".

Dalton now cast about for an *explanation* of the composition of matter, an explanation which would entail this formulated law as a mere consequence. He therefore made the assumption that all elements consist of very minute indivisible particles, termed atoms, having a definite weight; that the atoms of each elementary substance are alike among themselves and are different from the atoms of every other element; that the atoms of a chemical com-

[1] Ed. Robertson, *Ency. Brit*, xiv, p 335. As Dr. Robertson points out, the opposed conceptions are still entangled in the field of political economy. Some people speak of certain economical principles as if they were laws of nature, and any measures that would violate these principles are regarded as particularly heinous.

[2] Cf. Bain, *Logic*, vol. ii, p. 9. See also Welton, *Logic*, vol. ii, p. 200. Professor Welton objects to the term "empirical *laws*" altogether; he thinks that the generalizations are too loose, and express no necessity, and that therefore the term "law" is out of place.

nothing, or it may be a perfect and conclusive induction. In the example given, the essence of the supposed relation is the management of a business by a few persons specially chosen from amongst the much larger number actually interested. Now some may contend that this circumstance, with its various consequences, is the chief factor in determining all the effects which make up what we call good or bad administration. If they can establish this, their argument has the force of a rigorous induction; if they cannot, they have failed in proving the analogy between the two cases.

But it is more usual to extend the name of analogical evidence to arguments from any sort of resemblance, provided they do not amount to a complete induction.—"Two things resemble each other in one or more respects; a certain proposition is true of the one; therefore it is probably true of the other."—The distinction between analogy and induction is, then, that in the case of a complete induction, by due comparison of instances it is established that there is an *invariable conjunction* between the former points of resemblance and the latter; but in what is called analogical reasoning, no such conjunction has been made out. An argument from analogy really amounts to this: a fact m, known to be true of A, is more likely to be true of B if B agrees with A in some of its properties (even though no connection is known to exist between m and those properties), than if no resemblance at all could be traced between B and any other thing known to possess the attribute m.[1]

Suppose, for instance, we infer that there are probably inhabitants (m) in the moon (B) because there are inhabitants on the earth (A). Now the moon resembles the earth in being a solid, opaque, nearly spherical body; appearing to contain, or to have contained, active volcanoes; receiving heat and light from the sun relatively in about the same quantity as our earth; revolving on its axis; and obeying the laws of gravitation. If this were all that was known of the moon, the existence of inhabitants in that luminary would derive from these various resemblances to the earth a fairly high degree of probability.

But any dissimilarity between A and B furnishes a counter-probability. There will be a competition between the known points of agreement and the known points of difference. The moon, for instance, differs from the earth in being smaller, in having its surface more unequal and apparently volcanic throughout, in having (at least on that side next the earth) no atmosphere sufficient to

[1] See Mill, Book III, ch. xx, §§ 1, 2.

refract light, no clouds, and—presumably—no water. These differences might perhaps balance the resemblances, so that the analogy would afford no presumption either way. But considering that some of the circumstances which are wanting on the moon are among those which, on the earth, are indispensable to animal life, we are forced to conclude that if there *is* animal life in the moon, it must be an effect of causes totally different from those on which it depends here. But the important point to notice is the competition between analogy and diversity. A probability arising out of points of agreement may be cancelled, or at all events greatly affected, by known differences; and it is often enormously reduced by the large element of the unknown.[1]

§ 2. Points of Resemblance must be Weighed, not Counted

It is, however, necessary to bear in mind that, in analogy, as indeed in inference generally, the mere *number* of points of resemblance may be of little importance; such points ought to be *weighed* not *counted*.[2] The points of resemblance contain no common factor which admits of any form of mathematical treatment. As Professor Welton says, properties are not isolated or separate individualities which we can count and enumerate as we can balls and books.[3] There is, in fact, no popular error from which the student of Science must more resolutely shake himself free than the notion that resemblance and difference can be estimated as an "amount". A resemblance or difference is great or small not according either to its power of striking the observer's notice, or to the *number* of "points" (or details) into which it may be analysed, but according to the importance of its details in regard to the matter in hand.[4]

§ 3. "Essential" Resemblance

There is a danger in all analogical argument that the resemblance between the cases supposed to be analogous is only a superficial one; or even perhaps that the resemblance, though on the whole real and deep, is *not essential* for the purpose intended. So far as an argument professes to rest on analogy, we must first ascertain, if possible, the exact points of resemblance and difference, and inquire whether the resemblance has any right to be considered *essential*.[5]

[1] Cf. Mill, *ib.* §§ 2, 3; Bain, *Logic*, vol. ii, p. 147.
[2] Cf. Bosanquet, *Logic*, vol. ii, p. 90.
[3] Cf. Welton, *Logic*, vol. ii, pp. 76-80
[4] See Sidgwick, *Process of Argument*, p. 194.
[5] Cf. Sidgwick, *Fallacies*, p. 253.

To illustrate this point, Mr. Sidgwick makes use of the argument sometimes employed against Sunday closing, that, since the upper classes have their clubs open on that day, it would be unfair to deprive the poor of their only places of resort and refreshment. It is clear at once that we have here a case of double analogy, "essential resemblance" being considered to exist (1) between clubs and public-houses, and (2) between the upper classes and the lower. Now "essential resemblance" means that, for the purposes immediately in view, we may neglect points of difference; but if we do this we begin to generalize. If we neglect points of difference between clubs and public-houses, or between one class of men and another, we speak of them as members of some wider class. In the case of clubs and public-houses, it is the fact of their being places of "resort and refreshment" that is considered essential. And although the key to the analogy between the upper and lower classes is not expressly given, no doubt some such maxim as that "the law is no respecter of persons" is implied. If, then, this account of the analogy is a correct one, if it is *only* as being places of resort and refreshment that public-houses are to be kept open for the benefit of the poor in their sole capacity of citizens of the State, we imply the generalization, "all citizens of the State are equally entitled to their places of resort and refreshment". By means of this, *if true*, the original proposition may now be deductively proved.[1]

Although analogy rarely gives more than a slight presumption of Proof, it is more widely used in common discourse than any other form of argument; and even for purposes of Proof as well as for purposes of Inference. This seems to be due to the slackness with which our examination of evidence is commonly carried on. It is so much less trouble to see that two things bear a "striking resemblance" than to discriminate accurately how far the resemblance really goes, and the points wherein they differ. There is probably nothing that is more characteristic of the higher intellect, as contrasted with the lower, than its greater power of discriminating, that is, of seeking points of difference. Knowledge begins with a vague blur, which gradually becomes distinct. The trained eye, the analytic mind, is always able to detect finer shades of difference than are visible to the multitude; it is, in fact, neglect of differences that marks the ruder nature.[2] Of course, any neglect of

[1] See Sidgwick, *Fallacies*, pp. 254–6.

[2] This, perhaps, requires some qualification. For instance, the remarkable forest-lore of the Red Indian, or the equally remarkable knowledge of animal spoors shown by the Zulu, must be attributed to an intelligent observation and an appreciation of minute differences

real resemblance would result in serious error, but the inducements to over-generalize are usually much stronger than those to indulge in excessive hair-splitting. And it is always less trouble to avoid distinguishing, even when we have attained the power. Anything that appeals to our idleness, anything that flatters our sense of "breadth of view", will invariably meet with a friendly reception.[1]

We see, then, that, in strength, an analogical argument may approach, but not reach, a valid induction. The value of such an argument increases with the extent of ascertained *essential resemblance*, and this is determined by a careful comparison of points of resemblance and points of difference. We cannot "neglect" points of difference until they have been carefully scrutinized; should there be *essential difference*, no analogical inference is likely to be acceptable. And if there is a large "unexplored region of unascertained properties", in other words, if our knowledge of the subject-matter is only slight, the extent of our ascertained essential resemblance is, in any case, likely to be so small as to lead to an inference of doubtful or no value.[2]

§ 4. Instances of Analogical Inference

Analogy naturally plays a great part in discovery, often giving hints that are followed up in a most fruitful way. There are analogies which connect whole branches of Science in a parallel manner, and enable us to infer of one class of phenomena what we know of another. It has thus happened on several occasions that the discovery of an unsuspected analogy between two branches of knowledge has been the starting-point for a rapid course of discovery.

No two branches of Science might seem at first sight more different in their subject-matter than Geometry and Algebra, and, prior to the time of Descartes, they were developed slowly and painfully in almost entire independence of each other. But that great philosopher showed that every algebraic equation may be represented by a geometric curve; and this discovered analogy be-

that would put an ordinary civilized white man to shame. We are apt to gauge the intelligence of primitive races by their conception of abstract ideas, coupled with their adaptability when confronted with civilizing influences. No doubt, the nobler conceptions of human life are totally incomprehensible to the average aboriginal, who, however, is a much more intelligent person, and possesses far more real knowledge, than some of those who affect to despise him.

[1] See Sidgwick, *Fallacies*, pp. 256–8.

[2] Cf. Mill, Book III, ch. xx, §§ 2, 3; and Bosanquet, *Logic*, vol. ii, pp. 98–9.

tween Geometry and Algebra soon led to many new developments in mathematical methods.

Then, again, the different forms of wave-motion provide us with striking instances of analogy. All waves, whatsoever be the matter through which they pass, obey the principles of rhythmical or harmonic motion, and the subject presents a fine field for mathematical generalization. Each kind of medium will, however, probably allow of waves with specific characteristics, so that it is a good exercise in analogical reasoning to decide how, in making inferences from one kind of medium to another, we must make allowance for difference of circumstances. The waves of the ocean are large and visible, and there are the yet greater tidal waves which extend round the globe. From these we pass to waves of sound, varying in length from about 32 ft. to a small fraction of an inch. If, now, we can imagine the fortieth octave of the middle C of a piano, we reach the undulations of yellow light, the ultra-violet being about the forty-first octave. Thus we pass from the obvious to the obscure. Yet the same phenomena of reflection, interference, and refraction, which we find in some kinds of waves, may be expected to occur, *mutatis mutandis*, in other kinds. But while light travels 186,000 miles a second, sound in air travels only 1100 ft. in the same time, that is, nearly a million times as slowly. We are, therefore, prepared to find great differences of some kind, both in the form and in the character of the vibrations.

In Astronomy, too, analogy has played an important part. When the scientific world was divided in opinion between the Copernican and Ptolemaic systems, Galileo discovered, by the use of the telescope, four small satellites revolving round Jupiter. The analogy from this miniature planetary world was irresistible. Then our speculations concerning the physical conditions of the planets and their satellites depend largely upon analogies. We do not hesitate to infer that the moon has mountains and valleys, and we infer with considerable probability that Mars has Polar seas. These are comparatively safe inferences, but speculations have also been made on the existence of life in other planets. Huyghens even went so far as to enter into an inquiry whether the inhabitants of other planets would possess reason and knowledge of the same sort as ours; and he concluded that, although their intellectual power might be different, they would at least have the same Geometry if they had any at all. Laplace entertained a strong belief in the existence of inhabitants in other planets, considering that the benign

influence of the sun would tend to be the same in the case of other planets as in the case of the earth. Even if it be objected that, in certain planets, the extreme heat or the extreme cold would render life, as we know it, impossible, it has to be remembered that many metals and other elements never found in organic structures are yet capable of forming compounds with substances of vegetable or animal origin; and it is therefore quite within the bounds of possibility that creatures formed of different yet analogous compounds (compared with those of earth creatures) might exist in temperatures vastly different from ours. Still, we must admit that all such speculations rest on weak analogies, and are hardly admissible within the portals of true Science. Nevertheless, as Jevons points out, such speculations are far more reasonable and acceptable than dogmas which assert that the thousand million of persons upon the earth, or rather a small fraction of them, are the sole object of care of the Power which designed this limitless Universe.[1]

§ 5. Illegitimate Analogy

In public speaking, analogy is often put forward to raise a vague presumption; and it may be done in such a manner that, if objections should be raised, it remains easy to claim that only an illustration was intended, and to grant with much candour that possibly as an illustration it fails to fit the case exactly; a process which closely resembles the parliamentary practice of first using and then withdrawing an offensive expression. The words "because", or "for", or "since" are, as a rule, omitted by the speaker; the connection will be readily supplied, as every experienced rhetorician knows, by any average audience, and being thus voluntarily supplied, will probably be less exposed to immediate criticism. Whately, for example, did not write "Inductive Logic can never be a rival to the Aristotelian Logic, *since* a plough can never be substituted for a flail"; but he wrote that Inductive Logic "would not have the same object proposed with the Aristotelian Logic; nor be in any respect a rival to that system. A plough may be a much more ingenious and valuable instrument than a flail, but it can never be substituted for it."[2]—Such a rhetorical device often comes perilously near the border-line of intentional deception.

[1] See Jevons, *Prin. of Sci.*, pp. 627–41.

[2] It should be noticed that this is not given as an instance of necessarily *false* analogy. It is given as an instance of the usual method of getting an analogy (true or false) accepted by an audience. It is usually difficult to decide whether the analogy is really relied on as

Proverbs, again, are frequently employed in arguing from a shadowy resemblance. Any "striking" analogy will so easily pass muster that proverbs can always be freely and safely used. To assume that some case comes under some well-known proverb, without a shadow of evidence to show that it does so beyond what may be gathered from the crudest superficial inspection, is, with some people, quite a favourite practice.[1]

§ 6. Hypotheses Suggested by Analogy

Hypotheses are very frequently suggested by analogy. Even the simplest phenomenon may present so many points that suggest comparison, that we often have a choice from among many hypotheses. Analogy has been aptly compared to a guide-post.[2]

Although the cases in which analogical evidence affords in itself any very high degree of probability are only those in which the resemblance is essential and extensive, yet there is no analogy, however faint, which may not be of the utmost value in suggesting experiments or observations that may lead to more positive conclusions. We may feel that it is impossible to accept as positive truths any hypotheses which are unsusceptible of being ultimately brought to the test of actual induction. Yet "any hypothesis which has so much plausibility as to explain a considerable number of facts, helps us to digest these facts in proper order, to bring new ones to light, and make *experimenta crucis* for the sake of future inquiries". If an hypothesis both explains known facts and has led to the prediction of others previously unknown, and since verified by experience, the analogy which extends so far may probably extend further, and nothing is more likely to suggest experiments tending to throw light upon the real properties of the phenomenon than the following out of such a hypothesis. But to this end it is by no means necessary that the hypothesis be mistaken for a scientific truth. That illusion would be an impediment to the progress of real knowledge. Yet analogy should readily be allowed to play its part in bringing up new phenomena for comparison with the old; an hypothesis may thus receive additional strength, and be one step nearer

evidence, or genuinely and legitimately put forward as an illustration merely, or to point a quaint and semi-serious fallacy. A still more difficult question arises when we attempt to fix the line between the metaphorical and the direct use of words. See Sidgwick, *Fallacies*, pp. 263–4.—The worthy Archbishop was rather unfortunate in his choice of a figure, and could hardly have foreseen how soon the flail was to be relegated, with Aristotelian Logic, to a museum

[1] Cf. Sidgwick, *Fallacies*, p. 266.

[2] Cf. Jevons, p. 630; and Mill, III, xx, § 3.

scientific truth. On the other hand, it may be weakened, and perhaps even destroyed.[1]

CHAPTER XXIII

Probability

§ 1. General Notions

Let us suppose[2] that there are three perfectly sincere persons, A, B, and C, and that, on some particular subject, A holds one opinion, B another, and C has no opinion at all. One of them, say A, proceeds to burn B and C, or to hang them, or imprison them, or, at the least, to libel them in the newspapers, according to what the feelings of the age will allow; the pretext being that A, B, and C are morally inexcusable for not believing what is true. If A is shown the absurdity of his own arguments, he promptly contends for a sort of absolute truth external to himself, which B or C, he declares, might attain if they pleased. Now let it be granted for a moment that the intellectual constitution of A, B, and C is precisely the same, and that there is ground for declaring that any difference of opinion resulting from the same arguments must be one of moral character. If, then, it were quite certain A is right, and if it be granted that State punishments are reformative of immoral habits as well as repressive of immoral acts, A might be justified in using, with B and C, methods which are reformative of moral character, even if these methods amounted to direct persecution. But, as De Morgan says, anyone who is able to see with the eyes of his body that the same weight will stretch different strings differently, and with the eyes of his mind that the same arguments will affect different minds differently—by difference not of moral but of intellectual construction—will also see that the only legitimate process of effecting a change of conviction must be that of argument and discussion.[3]

Intolerance arises, as a rule, from inability to see how differently different persons are affected by real *probabilities*. It therefore becomes interesting to ask what it is that mathematicians, in their

[1] Cf. Mill, *ib.*

[2] See De Morgan, *Essay on Probabilities*, pp. 7, 8.

[3] The common assertion that opinions dangerous to the existence of public order must not be promulgated, is hardly germane to our subject.

theory of Probabilities, actually number, measure, and calculate. Is it belief, or opinion, or doubt, or knowledge, or chance, or necessity, or what? Does probability exist in the *things* which are probable, or in the mind which regards them as such?

Now it must be clear, at the outset, that the subject of the theory cannot be "chance". Chance does not exist in Nature. The exact form of every pebble on the seashore is the resultant effect of a succession of definitely acting antecedents. Chance is merely an expression, as Laplace remarked, for our ignorance of the causes in action, and our consequent inability to predict the result or to bring it about infallibly. In Nature, "all is causal, nothing is casual";[1] in her laws there can never be any uncertainty. Such deficiency as there may be must lie wholly in our *knowledge.*

Clearly, then, probability is no inherent property of any set of circumstances, it is a feeling of the mind.[2] This may be seen from the fact that different minds may regard the very same event at the same time, with widely different degrees of probability. A steam vessel, for instance, is missing, and some persons believe that she has sunk in mid-ocean; others think differently, though all have the same scanty information concerning her last-known movements But in the event itself there can be no uncertainty; the vessel has either sunk or not sunk, and no amount of subsequent discussion of the probable nature of the event can alter the fact. Yet the probability of the event will really vary from day to day and from mind to mind, according as the slightest information is gained regarding the vessels met at sea, the weather prevailing there, wreckage found, and so on. Probability, then, belongs wholly to our minds, to the light in which we regard events, the occurrence or non-occurrence of which is, in themselves, absolutely certain.[3]

§ 2. The Theory of Probability deals with Quantity of Knowledge

Jevons disagrees with De Morgan, who says that "by degree of probability we really mean or ought to mean degree of belief"; and with Donkin's opinion that probability is "quantity of belief"; for, says Jevons, "the nature of *belief* is not more clear to my mind than the notion which it is used to define. The theory of probability

[1] See Welton, *Logic*, vol. ii, p. 165. [2] De Morgan, *op. cit.* p. 7.
[3] Cf. Jevons, *Prin. of Sci.*, pp. 197-8; Lotze, *Logic*, p. 367; and Welton, *Logic*, vol. ii, pp. 165-70.

does not measure what the belief is but what it ought to be."[1] Venn also refers to the difficulty of obtaining any measure of the amount of our belief. In the first place, there is the disturbing influence produced on the quantity of belief by any strong emotion or passion; and in the second place, there is the extreme complexity and variety of the evidence on which our belief of any proposition depends. It follows, therefore, that our actual belief at any given moment is one of the most fugitive and variable things possible, so that we can scarcely ever get sufficiently clear hold of it to measure it. Directly we begin to think of the amount of our belief, we have to think of the arguments by which it is produced,—in fact, these arguments will intrude themselves without our choice. As each in turn flashes through the mind, it modifies the strength of our conviction.[2]

Jevons prefers to avoid the term "belief" as being obscure, and he regards the theory of probability as dealing with *quantity of knowledge*. An event is only probable, he says, when our knowledge of it is diluted with ignorance, and exact calculation is needed to discriminate how much we do and do not know. The theory of probability measures the comparative amounts of our knowledge and ignorance.[3]

§ 3. Quantitative Aspects of the Theory

"When we say that the probability that an event will happen in a certain way is 1/n, what we mean is that the relative amounts of knowledge and ignorance we possess as to the conditions of the event justify the amount of expectation. The event itself will happen in some one definite way, exactly determined by causation; the probability does not determine that, but only our subjective expectation of it." "It is from this combination of knowledge and ignorance that the calculation of probability starts."[4]

Fundamentally, the theory of probability consists in putting similar cases on an equality, and distributing equally among them whatever knowledge we possess. Throw a penny into the air, and consider what we know in regard to its way of falling. We know that it will certainly fall upon a side, so that either head or tail will be uppermost; but as to whether it will be head or tail, our knowledge is equally divided. Whatever we know concerning head, we know also concerning tail, so that we have no reason for expecting

[1] Jevons, *Prin. of Sci.*, p. 199.
[2] Cf. Venn, *Logic of Chance*, pp. 125-7.
[3] Jevons, *ib.* pp. 199-200.
[4] Cf. Welton, *Logic*, vol. ii, pp. 165-70.

one more than the other. The least predominance of belief to either side would be irrational; it would consist in treating unequally things of which our knowledge is equal. *We must treat equals equally.*[1]

The theory does not require that we should first ascertain by experiment the equal facility of the events we are considering. The more completely we could ascertain and measure the causes in operation, the more would the events be removed from the sphere of probability. The theory comes into play where ignorance begins, and the knowledge we possess requires to be distributed over many cases. Nor does the theory show that the coin will fall as often on the one side as the other. It is almost impossible that this should happen, because some inequality in the form of the coin, or some uniform manner in throwing it up, is almost sure to occasion a slight preponderance in one direction. But as we do not previously know in which way a preponderance will exist, we have no reason for expecting head more than tail.[2]

Suppose that, of certain events, we know that some one will certainly happen, and that nothing in the constitution of things determines one rather than another; in that case, each will recur, in the long run, with a frequency in the proportion of one to the whole. Every second throw of a coin, for example, will, in the long run, give heads. Every sixth throw of a die will, in the long run, give ace.[3]

The method which we employ in the theory consists in calculating the number of all the cases or events concerning which our knowledge is equal.[4]

Let us suppose that an event may happen in three ways and fail in two ways, and that all these ways are equally likely to occur. Clearly, in the long run, the event must happen three times and fail two times out of every five cases. The probability of its happening is therefore $\frac{3}{5}$, and of its failing, $\frac{2}{5}$. Thus the probability of an event is the ratio of the number of times in which the event occurs, in the long run, to the sum of the number of times in which the events of that description occur and in which they fail to occur.

An event must either happen or fail. Hence the sum of the probabilities of its happening or failing is certainty. We therefore represent certainty by unity.

[1] Cf. Jevons, *Prin. of Sci.*, p. 200.
[2] Cf. Jevons, *op. cit.* p. 201.
[3] Cf. Bain, *Inductive Logic*, p. 91.
[4] Jevons, *op. cit.* p. 201.

§ 4. Simple Mathematical Considerations

The usual algebraic definition of probability is as follows. If an event may happen in a ways and fail in b ways, and all these ways are equally likely to occur, the probability of its happening is $\frac{a}{a+b}$, and the probability of its failing is $\frac{b}{a+b}$. (In mathematical works, the word "chance" is often used as synonymous with probability.)

It should be noticed that $\frac{a}{a+b} + \frac{b}{a+b} = 1$; also that $1 - \frac{a}{a+b} = \frac{b}{a+b}$. Thus, if p be the probability of the happening of an event, the probability of its not happening is $1 - p$.

When the probability of the happening of an event is to the probability of its failure as a is to b, the *odds* are said to be a to b *for* the event, or b to a against it, according as a is greater or less than b.

Suppose that 2 white, 3 black, and 4 red balls are thrown promiscuously into a bag, and a person draws out one of them, the probability that this will be a white ball is $\frac{2}{9}$, a black ball, $\frac{3}{9}$, and a red ball, $\frac{4}{9}$.

A few simple problems will help to illustrate the principles involved.

1. What is the probability of throwing 2 with an ordinary die?—Any one face is as likely to be exposed as any other face; there are therefore one favourable and five unfavourable cases, all equally likely. The required probability is therefore $\frac{1}{6}$.

2. What is the probability of throwing a number greater than two with an ordinary die?—Obviously there are 4 possible favourable cases out of a total of 6. The probability is therefore $\frac{4}{6}$ or $\frac{2}{3}$.

3. A bag contains 5 white, 7 black, and 4 red balls. What is the probability that 3 balls drawn at random are all white?—We have 16 balls altogether. The total number of ways[1] in which 3 balls can be drawn is therefore ${}^{16}C_3$, and the total number of ways in which 3 white balls can be drawn is ${}^{5}C_3$. Therefore, by definition, the probability is ${}^{5}C_3/{}^{16}C_3$, that is $\frac{1}{56}$.

By a *compound event*, we mean an event which may be decomposed into two or more simpler events. Thus, the firing of a gun may be

[1] It is assumed that the reader is acquainted with the elementary theory of Combinations and Permutations.

decomposed into pulling the trigger, the fall of the hammer, the explosion of the cartridge, &c. In this example, the simple events are *not independent*, because, if the trigger is pulled, the other events will, under proper conditions, necessarily follow, and their probabilities are therefore the same as that of the first event. Events are *independent* when the happening of the one does not render the other either more or less probable than before. Thus the death of a person is neither more nor less probable because the planet Mars happens to be visible. When the component events are independent, a simple rule can be given for calculating the probability of the compound event, thus: *Multiply together the fractions expressing the probabilities of the independent component events.*[1]

If, for instance, A occur once in 6 times, its probability is $\frac{1}{6}$, or 1 for and 5 against; if B occur once in 10 times, its probability is $\frac{1}{10}$, or 1 for and 9 against. The probability, or relative frequency in the long run, of the concurrence of the two is $\frac{1}{60}$, that is, 1 for and 59 against.[2]

The justification of the rule may be shown thus.[3]—If two dice are thrown, the side which the one shows uppermost has nothing to do with the side which the other shows uppermost; but each die has 6 sides, each of which may fall uppermost, and each of these may with equal possibility coincide with any one of the 6 sides of the other; there are thus 36 possible cases, and the probability of each single one of them is $\frac{1}{36}$ $(= \frac{1}{6} \times \frac{1}{6})$.

We may add one or two more problems.

1. What is the probability of throwing an ace in the first only of two successive throws of a single die?—Here we require a compound event to happen, namely, at the first throw the ace is to appear, at the second throw the ace is not to appear. The probability of the first simple event is $\frac{1}{6}$, and of the second $\frac{5}{6}$. Hence the required probability is $\frac{5}{36}$ $(= \frac{1}{6} \times \frac{5}{6})$.

2. A party of 23 persons take their seats at a round table. Show that it is 10 to 1 against two specified individuals sitting next to each other.—The probability that a given person A is on one side of a given person B is $\frac{1}{22}$; the probability that A is on the other side of B is also $\frac{1}{22}$; hence, the probability of A being next to B is $\frac{2}{22} = \frac{1}{11}$. Thus the odds are 10 to 1 against A and B sitting together.

3. Find the probability of throwing 8 with two dice. — With two dice, 8 can be made up of 2 and 6, 3 and 5, 4 and 4, 5 and 3,

[1] Cf Jevons, *op. cit.* p. 204. [2] Cf. Bain, *Logic*, vol. ii, p. 92. [3] Cf. Lotze, *Logic*, p. 368.

and 6 and 2, that is 5 ways. The total number of ways is 36. The probability is therefore $\frac{5}{36}$, and the odds 31 to 5 against.

§ 5. Experience and Theory Compared

"The Laws of Probability rest upon the fundamental principles of reasoning, and cannot be really negatived by any possible experience. It might happen that a person should always throw a coin head uppermost, and appear incapable of getting tail by chance. The theory would not be falsified because it contemplates the possibility of the most extreme runs of luck."[1] But the probability of the occurrence of extreme runs of luck is excessively slight. Whenever we make any extensive series of trials, as in throwing a die or coin, the probability is great that the results will agree pretty nearly with the predictions yielded by theory. Precise agreement must not, of course, be expected, for that, as the theory shows, is highly improbable. Buffon caused a child to throw a coin many times in succession, and he obtained 1992 tails and 2048 heads. The same experiment performed by a pupil of De Morgan's resulted in 2044 tails to 2048 heads. In both cases the coincidence with theory is as close as could be expected. Jevons himself made an extensive series of experiments. He took 10 coins, and made 2048 throws in two sets of 1024 throws each. Obviously, the probability of obtaining 10, 9, 8, 7, &c., heads is proportional to the number of combinations of 10, 9, 8, 7, &c., things chosen from 10 things. The results may therefore be thus conveniently tabulated:—

Character of Throw.	Theoretical Numbers.	First Series.	Second Series	Average.	Divergence.
10 Heads, 0 Tails	$^{10}C_0 = 1$	3	1	2	+ 1
9 „ 1 „	$^{10}C_1 = 10$	12	23	$17\frac{1}{2}$	+ $7\frac{1}{2}$
8 „ 2 „	$^{10}C_2 = 45$	57	73	65	+20
7 „ 3 „	$^{10}C_3 = 120$	129	123	126	+ 6
6 „ 4 „	$^{10}C_4 = 210$	181	190	$185\frac{1}{2}$	− $24\frac{1}{2}$
5 „ 5 „	$^{10}C_5 = 252$	257	232	$244\frac{1}{2}$	− $7\frac{1}{2}$
4 „ 6 „	$^{10}C_6 = 210$	201	197	199	−11
3 „ 7 „	$^{10}C_7 = 120$	111	119	115	− 5
2 „ 8 „	$^{10}C_8 = 45$	52	50	51	+ 6
1 „ 9 „	$^{10}C_9 = 10$	21	15	18	+ 8
0 „ 10 „	$^{10}C_{10} = 1$	0	1	$\frac{1}{2}$	− $\frac{1}{2}$
	1024	1024	1024	1024	0

[1] Jevons, *Prin. of Sci.*, p. 206.

The present writer repeated the same series of experiments, with the following results:—

Character of Throw.	Theoretical Numbers.	First Series.	Second Series.	Average.	Divergence.
10 Heads, 0 Tails	$^{10}C_0 = 1$	4	0	2	+ 1
9 „ 1 „	$^{10}C_1 = 10$	20	6	13	+ 3
8 „ 2 „	$^{10}C_2 = 45$	40	40	40	− 5
7 „ 3 „	$^{10}C_3 = 120$	83	150	116½	− 3½
6 „ 4 „	$^{10}C_4 = 210$	224	222	223	+13
5 „ 5 „	$^{10}C_5 = 252$	250	209	229½	−22½
4 „ 6 „	$^{10}C_6 = 210$	242	222	232	+22
3 „ 7 „	$^{10}C_7 = 120$	115	107	111	− 9
2 „ 8 „	$^{10}C_8 = 45$	28	60	44	− 1
1 „ 9 „	$^{10}C_9 = 10$	14	6	10	0
0 „ 10 „	$^{10}C_{10} = 1$	4	2	3	+ 2
	1024	1024	1024	1024	0

The whole number of single throws of coins amounted to 2048 × 10, or 20,480 in all, one half of which, or 10,240, should theoretically give heads. The total number of heads obtained by Jevons was 10,352 (5130 in the first series, and 5222 in the second). The number obtained by the present writer was 10,234 (5098 in the first series, and 5136 in the second). The coincidence with theory is in each case fairly close.[1]

§ 6. Inverse Probability

From the known character of certain events, we may argue backwards to the probability of a certain law or condition governing those events. When it is known that an event has happened, and that it must have followed from one of a certain number of causes, the determination of the probabilities of the different possible causes is said to be a problem of *inverse* probability.

If, for instance, it was known that a black ball was drawn from one or other of two bags, one of which was known to contain 2 black and 7 white balls, and the other 5 black and 4 white balls, what was the probability that the ball was drawn from the first bag?

Let us suppose a great number, 2N, of drawings to be made; there will in the long run be N from each bag. But in N draw-

[1] See Jevons, *Prin. of Sci.*, pp. 206–9. It was the writer's intention to perform several experiments of this kind; but if the reader will undertake one such experiment for himself, he will probably conclude that one is sufficient to satisfy one person.

ings from the first bag there are, on the average, $\frac{2}{9}$N which give a black ball; and in N drawings from the second bag there are $\frac{5}{9}$N which give a black ball. Hence, in the long run, $\frac{2}{9}$N out of a total of $\frac{2}{9}$N + $\frac{5}{9}$N black balls are due to drawings from the first bag. Thus, the probability that the ball was drawn from the first bag is $\frac{2}{9}$N/($\frac{2}{9}$N + $\frac{5}{9}$N) = $\frac{2}{7}$.[1]

The general problem may be thus stated: *If it is certain that one or other of the supposed causes exists, the probability that any one does exist is the probability that if it exists the event happens, divided by the sum of all the similar probabilities.*

If, for instance, there be three boxes, each containing 10 balls in all, and respectively containing 7, 4, and 3 white balls, then on mixing all the balls together we have 14 white ones; and if we draw a white ball, that is, if the event happens, the probability that it came out of the first box is $\frac{7}{14}$; which is exactly equal to $\frac{7}{10}$/($\frac{7}{10}$ + $\frac{4}{10}$ + $\frac{3}{10}$), the fraction given by the rule.[2]

The inverse problem is complex and difficult, but the principle of the method is frequently used in cases of scientific investigation. If only two, or at the most a few, hypotheses may be made as to the origin of certain phenomena, we may sometimes easily calculate the respective probabilities. It was thus, as Jevons points out, that Bunsen and Kirchhoff established with a probability almost equal to certainty that iron exists in the sun.[3] Then, again, the probability has been calculated as to whether the six brightest stars of the Pleiades came by accident into such close proximity. Michell's estimate is that the odds are nearly 500,000 to 1 against casual conjunction.[4] Then extremely interesting cases have been worked out in regard to the similarity of direction of the orbital motions and axial rotations of the planets, and in regard to the near approximation of the orbits of the planets to a common plane. The numbers in these cases representing the odds are so great as to be altogether beyond the comprehension of the non-mathematical mind. Suffice it to say that the enormous probability that the constitution of the planetary system arose from some common cause, amounts to practical certainty.[5]

[1] See C. Smith's *Algebra*, p. 521. Cf. Chrystal's *Algebra*, vol. ii, ch. xxxvi.

[2] See Jevons, *Prin. of Sci.*, pp. 243-4.

[3] See Kirchhoff, *Researches on the Solar System*, i, pp. 18, 19.

[4] *Phil Trans*, vol. lvii, p. 431.

[5] Cf. Laplace, *Essai Philosophique*, p. 55; and Todhunter, *History of Theory of Probability*, p. 543. For details of these and other interesting cases, see Jevons, pp. 244-50.

§ 7. Simple Rules of the Inverse Method

Although the general solution of the inverse problem is outside the scope of this work, two useful rules may be mentioned.

1. *To find the probability that an event which has not hitherto been observed to fail will happen once more, divide the number of times the event has been observed increased by one, by the same number increased by two.*

Continued recurrence of an event testifies to the persistence of the conditions which produce the event; and even though we do not know what these conditions are, yet, as evidence of their existence increases, we are justified in expecting more and more strongly their continued existence. If, for example, an event has happened once, that is one reason for expecting its recurrence. But let us put that on one side for a moment. We may now argue that the chance of the event not occurring is in itself just as likely as that it will. There are therefore two reasons for expecting the event to recur, and only one for expecting it not to recur. The odds for its recurrence are consequently 2 to 1, and the probability of the event happening again is, by definition, $\frac{2}{3}$. So generally, if an event has happened m times, and we consider the possibility that it may occur again, we have a total number of alternatives of $m + 2$, of which $m + 1$ are favourable. The probability that the event will occur once more is therefore $(m + 1)/(m + 2)$. If, for instance, we suppose the sun to have risen one thousand million times, the probability that it will rise again, on the ground of this knowledge merely, is 1,000,000,001/1,000,000,002—a probability extremely close to certainty.[1]

2. *To find the probability that an event which has not hitherto failed will not fail for a certain number of new occasions, divide the number of times the event has happened increased by one, by the same number increased by one and the number of times it is to happen.*[2]

If, for instance, we suppose the sun to have risen one thousand million times, the probability that it will continue to rise for another thousand million times is only 1,000,000,001/2,000,000,001, or almost exactly $\frac{1}{2}$. The probability that it will continue so rising a thousand times as long is only about 1/1001—a very low degree of probability.

We thus see that with wide and uncontradicted experience, the probability that an empirical law which summarizes that experience

[1] Cf. Lotze, *Logic*, § 282 (5); Welton, *Logic*, vol. ii, pp. 180-1; and Jevons, pp. 257-9

[2] Cf. Jevons, *Prin. of Sci.*, p. 257.

will hold good in one more case is very high. But we also see that extension of the law beyond the realm of actual experience becomes increasingly uncertain with increase in the width of that extension.[1]

Of course, without demonstration or decisive experiment, probability can never reach absolute certainty. Fermat, for example, sought a formula in connection with prime numbers. An examination of several instances led him to think that 2 raised to a power which was itself a power of 2 would, if increased by unity, result in a prime number. Thus, $2^2 + 1 = 5$; $2^4 + 1 = 17$; $2^8 + 1 = 57$; $2^{16} + 1 = 65537$; all prime numbers. Fermat thereupon concluded that there was a great probability of the result being general. Absolute certainty, however, could only come with demonstration. In point of fact, there is a breakdown at the very next step, as Euler showed; $2^{32} + 1 = 4{,}294{,}967{,}297$, which is the product of 6,700,417 and 641, and is therefore not prime.[2]

§ 8. The Transmission of Historical Evidence

Laplace points out that, since the successive powers of a fraction less than unity continue to diminish, an event which depends upon a series of very great probabilities may at last become extremely improbable. "Suppose", he says,[3] "an incident to be transmitted to us by twenty witnesses in such manner that the first has transmitted it to the second, the second to the third, and so on. Suppose, again, the probability of each testimony to be equal to the fraction $\frac{9}{10}$. The probability of the incident resulting from all the testimonies will be $(\frac{9}{10})^{20}$, or less than $\frac{1}{8}$",—an enormous diminution in the probability. Now we all know that the farther news travels the more distorted it becomes, but it is extremely doubtful if calculation will really help us to decide the degree of trust we may repose in a transmitted statement. In the first place we make the large assumption that the probability of each testimony is (to take the above case) equal to $\frac{9}{10}$, that is, that a particular person speaks the truth 9 times out of 10. Then, again, any given statement is either right, or it deviates more or less from the truth; and we might assign to it a greater or less degree of credibility according as it deviates more or less, supposing it to be possible to measure against one another the different amounts of those deviations. But this we can seldom do. As Lotze points out,[4] the falsification of a state-

[1] Cf. Welton, *Logic*, ii, pp. 180–2.
[2] Cf. Laplace, *Theory of Probabilities*, p. 177.
[3] Laplace, *op. cit.* p. 13.
[4] See Lotze, *Logic*, pp. 370–1.

ment depends not on the number of times it has been passed on, but on the size and sort of errors made in it each time it has been passed on. The eyewitness A may or may not have wished to communicate aright what he has rightly observed; his hearer B has or has not understood him aright, or he may have understood him and yet desire to hand it on himself in a distorted form; a third person C, who intended to distort afresh what he already misunderstood, may chance to hit upon the actual truth in what he communicates. It is hardly conceivable that the trustworthiness of a communication depends, in any regular manner, merely on the number of times it has passed from mouth to mouth.

§ 9. Coincidences which are Casual

It will not, of course, be thought that the theory of probability is ever likely to furnish us with an infallible guide. All that it can give is *the result in the long run*, as it is called, and this virtually means an infinity of cases. During any finite experience, however long, chances *may* be against us. Yet the theory is the best guide we can have, and, if we follow it, we shall have the best chance of escaping error.

But no rule can be given for discriminating between coincidences which are casual and those which are the effects of law. Facts casually conjoined are separately the effects of causes, but of different causes, and causes not connected by any law. While, therefore, it is incorrect to say that any phenomenon is produced by chance, we may add that two or more phenomena are conjoined by chance, that they coexist, or that the one succeeds the other only by chance.[1]

It is, for example, only by chance that the ratio of the sun's diameter to the earth's diameter, the ratio of the mean distance of the earth from the sun to the sun's diameter, and the ratio of the mean distance of the moon from the earth to the moon's diameter, are all very approximately 110 to 1. Curious coincidences often occur in historical matters. If, for example, to 1794, the number of the year in which Robespierre fell, we add the sum of its digits, the result is 1815, the year in which Napoleon fell; the repetition of the process gives 1830, the year in which Charles X abdicated. Then the Bacon-Shakespeare controversy arose from a number of curious coincidences; the Baconian advocates, lacking insight into

[1] Cf. Jevons, *Prin. of Sci.*, pp. 261-2; and Mill, *Logic*, Book III, ch. xvii. (Cf. also ch. xx, § 4, on Joint Action of Causes.)

scientific method, mistook the casual for the causal.[1] The subject of Phrenology affords another instance of the manner in which numerous coincidences may mislead the unscientific mind.—The assumption is made that the outside of the skull is so finely and accurately modelled to the surface of the brain that it is an exact copy of that surface, an assumption which has been absolutely disproved. The great majority of phrenologists have had no serious scientific training at all; they are mere collectors of casual facts. Many of these facts when tabulated suggest, it is true, a rather high degree of probability, but this is the only solid claim that can be put forward. The work of the phrenologists is of the nature of a series of guesses, followed by deduction; it has no scientific basis whatever. Another assumption they make is that the grey matter of the brain is divided into a number of regions corresponding to certain universal habits, propensities, passions, and so on. This is a pure guess on their part; it is not based on scientific knowledge. The greatest living physiologists are at the present time laboriously accumulating facts in regard to cerebral localization, but an enormous amount of work yet remains to be done before we have anything like complete knowledge. Well may the physiologist regard with amused contempt the phrenologists and their absurd pretensions.

§ 10. Uncertainty almost Inevitable

If, in estimating the probability of events, the only data we have are the mere frequency of events in the past, our inferences are necessarily much more precarious than they would be if they could be deduced from an accurate knowledge of the frequency of the occurrence of the *causes* of the events. But it is a fact that, in almost all cases in which chances admit of estimation sufficiently precise to render their numerical appreciation of any practical value, the numerical data are not drawn from knowledge of the causes, but from experience of the events themselves. The probabilities of life at different ages; the probabilities of recovery from a particular disease; the chances of the destruction of property by fire; the chances of the loss of a ship on a particular voyage;—are derived from statistics on mortality, returns from hospitals, registers of fires,

[1] For instance, the word *honorificabilitudinitatibus* (Love's Labour's Lost, V, 1), which yields the anagram, "Hi ludi orbi tuiti F. Baconis nati", has given rise to much misplaced ingenuity of computation. (See *The Associated Accountants' Journal.*)

of shipwrecks, &c.; that is, from the observed frequency not of the causes but of the effects. In all these classes of facts, the causes are not amenable to precise observation, and whatever inferences we draw are necessarily drawn from frequency of effects.[1]

The element of uncertainty should always be borne in mind. If, for example, we are considering the prospect of a given particular man living another year, and we know from statistics that 9 out of 10 of his age do survive, it does not necessarily follow that *he* will; we say that our belief in his surviving is diminished from certainty to $\frac{9}{10}$. Not only is there an element of uncertainty in the empirical law that has resulted from generalization, but there is the further element of uncertainty in the inference we draw from such a law.[2]

A curious prejudice exists in some quarters against the study of the theory of probability, it being considered that such study is likely to foster a love of gaming and gambling. There may or may not be some truth in this, but the very great practical value of the theory must not be lost sight of. The whole business of insurance of all kinds is based upon it. And as regards Science, not only is the exactness of our present knowledge of Astronomy largely owing to the applications of the theory, but the theory acts as a fine corrective to false impressions and doubtful hypotheses in all branches of Science. Although its direct results may not be very obvious, its final importance cannot be denied.[3]

CHAPTER XXIV

Measurement

§ 1. Precise Measurement fundamental in Science

The ultimate aim of Science is to reduce the complexities of nature to their fundamental elements, and to express in an exact and quantitative form the relations amongst those elements. The more that exact measurement enters into any branch of Science, the

[1] Cf. Mill, *Logic*, Book III, ch. xviii, § 3.

[2] Cf. Venn, *The Logic of Chance*, pp. 192–3

[3] See De Morgan, *op. cit.* pp. 18–19. The reader who feels interested in the subject should turn to the works of De Morgan, Venn, Laplace, and Quetelet. See also Sir J. Herschel's paper on "Estimation of Skill in Target-shooting".

more highly is that branch developed. It is for this reason that Chemistry and Physics are so far in advance of Botany and Geology. And the reason why we can obtain so much clearer notions of, for instance, an area or a weight, than of, say, wisdom or chivalry, is because the former are *measurable*, the latter not. It is of the first importance in Science that we should, whenever possible, obtain precise quantitative statements of phenomena,[1] and thus we see why it is that the introduction of a new scientific instrument so often leads to a marked advance in our knowledge.[2] There is no doubt that with the instruments now available we are able to take note of quantities at least a million times as small as in the time of the Chaldeans.[3]

The great majority of questions in physical Science are complex, and an investigation often includes a number of steps, any one of which may suddenly lead to one or more subsidiary investigations. The kinds of questions likely to arise are so varied that no general rules of guidance can be laid down, though occasionally the sequence best followed will be fairly obvious. Suppose, for instance, we have to make an investigation into the question of the solution of common salt in water. The first point that would, perhaps, suggest itself would be, Does the solubility vary with the temperature? Then we should probably go on: Does the quantity of salt dissolved increase or diminish with the temperature? What is the amount of variation? Is there a law of variation, and, if so, what is it? Do different salts show different results? Does solubility vary with the pressure? Does the presence of other salts affect the result? Will different solvents lead to the same or to different results? And so one question leads to another, exact measurement being necessary at nearly every stage.[4]

§ 2. Standards and Units

The immediate result of every actual measurement is to give us a purely *numerical ratio*, namely, that between the magnitude to be measured, and a certain other magnitude, which should, when possible, be a fixed standard unit or magnitude.

When the standard unit is greater than the magnitude to be measured, we often *divide the unit*, until we get a magnitude equal to that measured. To measure minute objects, for instance, we use

[1] Cf. Herschel, *Phil.*, pp. 122-3; De Morgan, *Logic*, p. 175 ff.; Welton, *Logic*, ii, pp. 160-3.
[2] Cf. Jevons, *Prin. of Sci.*, p. 270. [3] *ib.* p. 271. [4] *ib.* pp. 278-282.

the micrometer screw, and so divide the inch or centimetre. But frequently we have to *multiply the unit* until we get a magnitude equal to that to be measured; for instance, in ordinary measurements with the footrule or chain.

In other cases we multiply or divide a *magnitude* until we get what is equal to the unit or easily comparable with it. To measure the velocity of a falling body, for example, we diminish the velocity by letting the body roll down an inclined plane.

A third method consists in multiplying both magnitudes to be compared until some multiple of the first is found to coincide very nearly with some multiple of the second. This method of repetition is naturally employed whenever quantities can be repeated or correctly repeat themselves. The oscillation of the pendulum, for example, admits of almost endless repetition, and since the force of gravity never ceases, there is no interval between the oscillations. It is thus possible, by comparing the oscillations of two exactly similar pendulums, to compare the force of gravity at the top and at the bottom of a mine; and by the aid of electric-clock signals this can be done with remarkable precision. In an experiment at Harton Colliery, Airy was able to measure a total difference in the vibrations at the top and bottom of the shaft of only 2·24 seconds in 24 hours, with an error of less than one-hundredth of a second, or one part in 8,640,000 of the whole day.[1]

Exact quantitative laws can occasionally be obtained without instrumental measurements. For instance, we learn that sounds of different pitch have exactly equal velocities, by observing that a peal of bells is heard harmoniously at any distance to which the sound penetrates. This could not be the case if one sound overtook the other.

Experiments are sometimes devised for the purpose of indirectly measuring quantities, which, in their extreme greatness or smallness, are beyond the powers of our senses. Thus Faraday measured the thickness of gold-leaf by weighing 2000 leaves $3\frac{3}{8}$ inches square; the weight was 384 grammes. From the known specific gravity of gold, it was easy to calculate that the average thickness of the leaves was less than a quarter of a millionth of an inch.[2]

Systematic measuring on an extensive scale often involves a large number of separate determinations, and care therefore has to

[1] Jevons, *Prin. of Sci.*, p. 291; and cf. *Phil. Trans.*, vol. clxvi.

[2] Faraday, *Chem. Res.*, p. 293. Gold leaf may be beaten out so fine that the thickness of a single sheet is only $\frac{1}{300000}$ part of an inch, or about one-sixth part of the length of a wave length of green light

be taken to adopt a system whereby any initial error is not only not increased in subsequent measurements, but actually detected, if possible. For instance, in the trigonometrical survey of a country, the most scrupulous care is taken to ensure the accuracy of the base-line; few laymen can appreciate the enormous trouble taken over this particular measurement. A principal triangulation now fixes, with the utmost possible accuracy, the relative positions and distances of a few points. A minor triangulation refers every prominent hill or village to one of the principal points, and thus the details are filled in by reference to the secondary points. Again, in ascertaining the specific gravities of substances, all gases are referred to atmospheric air at a given temperature and pressure; all liquids and solids are referred to water. We therefore require to compare, with the greatest accuracy, the densities of water and air, and the comparative densities of any two substances whatever can then be ascertained.

Few measurements of any kind are exact to more than six significant figures, and it is very seldom that even this degree of accuracy can be hoped for. *Time* is the magnitude which until recent years seemed capable of the most exact estimation. Astronomers have been able to ascertain the ratio of the mean solar to the sidereal day to the 8th place of decimals, or to one part in 100,000,000. Thirty years ago, this was probably the most accurate result of measurement in the whole range of Science. But determinations of *weight* seem now to come first in exactness, and balances have been constructed to detect one part in at least 250,000,000.[1] Determinations of *length* are open to much error in the junction of the measuring bars. Even in measuring the base-line of a trigonometrical survey, where extraordinary care is taken, the accuracy generally attained is only that of about one part in 60,000, or one inch in the mile. But Sir J. Whitworth was able to detect (by the use of a remarkably well turned screw) a change of dimension in a bar, amounting to no more than one-millionth of an inch.[2] Electrical measurements, too, are now carried out with an extraordinary degree of exactness

[1] For instance, the balance constructed by Rueprecht for the International Bureau of Weights and Measures.

[2] See Jevons, *Prin. of Sci.*, pp. 270-304. Cf. also *Phil. Trans.*, vol cxlvi, pp 330-1; *First Annual Report of the Mint*, p. 106; Watts, *Dictionary of Chemistry*, vol. i, p. 483; *Brit. Assocn. Report*, 1856, Address of President of Mech. Section; *Pro. Roy Soc*, vol. lxxxiii, p. 81, on Dr Tutton's reference to Mr. Grayson's fine rulings of 40,000 to the inch.

§ 3. Empirical Formulæ

In quantitative experiments we endeavour to obtain the relation between the different values of one quantity which is varied at will, and another quantity which is caused thereby to vary. The former may be called the *variable*, and the latter the *variant*. The variable is that one of the two measured quantities which is an antecedent condition of the other. When we are examining the effect of heat in expanding bodies, heat[1] is the variable, length the variant. If we compress a body to observe how much it is thereby heated, pressure, or it may be the dimensions of the body, is the variable, heat the variant.[2] Having once obtained, from a series of experiments, a number of values of a variable, and a corresponding number of values of the variant, we endeavour to determine what mathematical function the variant is as regards the variable. It is usual, therefore, first to discover whether there is any constant relation between the variable and the variant, and then to determine the *empirical formula* which expresses the relation between them. This empirical formula may or may not lead to the discovery of the *rational formula* expressing the law of nature involved.[3]

It is, of course, characteristic of quantitative investigations in physical Science that they are approximate only. As a general rule, a function can be developed or expressed as the sum of quantities, the values of which depend upon the successive powers of the variable quantity. If y be a function of x, then we may say that

$$y = A + Bx + Cx^2 + Dx^3 + Ex^4 + \ldots$$

In this equation, the terms may be infinite in number, or, after a time, may cease to have any value. The coefficients A, B, C, D, &c., are fixed quantities, of different values in different cases, and may happen to be zero or negative. The quantity x, on the other hand, is, of course, variable. Let us suppose a particular instance in which x and y are both lengths; and let us assume that $\frac{1}{10000}$ part of an inch is the least that we can take note of. Thus, when x is $\frac{1}{100}$ of an inch, $x^2 = \frac{1}{10000}$ of an inch, and if C is less than unity, the term Cx^2 is inappreciable, being less than we can measure. Unless any of the quantities D, E, &c., should happen to be very great, it is evident that all the succeeding terms will also be inappreciable, because the powers of x become rapidly smaller in geo-

[1] Or, one of its dimensions, temperature.
[2] Jevons, *op. cit.* p. 440. [3] *ib.* pp. 483-7.

metrical ratio. Thus when x is made small enough, the quantity y seems to obey the equation $y = A + Bx$. If x should be still less, if, for example, it should become as small as $\frac{1}{1000000}$ of an inch, and B should not be very great, then y would appear to be the fixed quantity A, and would not seem to vary with x at all. On the other hand, were x to grow greater, say equal to $\frac{1}{10}$ of an inch, and C not be very small, the term Cx^2 would become appreciable, and the law would be more complicated.[1]

Or, if we take any curve and consider a portion of it to be free from any kind of discontinuity, we may represent the character of such portion by an equation of the form

$$y = A + Bx + Cx^2 + Dx^3 + \ldots$$

If, now, we restrict our attention to a very small portion of the curve, the eye will be unable to distinguish its difference from a straight line; in other words, the term Cx^2 in the portion examined has no value appreciable to the eye. In this case $y = A + Bx$.

If we take a larger portion of the curve, curvature will become obvious, but it may be possible to draw a parabola or ellipse so that the curve shall apparently coincide with a portion of that parabola or ellipse. Similarly, if we take larger and larger arcs of the curve, it may assume the character successively of a curve of the third, fourth, or perhaps higher degrees; that is, it corresponds to equations involving the third, fourth, and higher powers of the variable quantity.[2]

In abstract mathematical theorems, the approximation to absolute truth is perfect, because we can treat of infinitesimals. In physical Science, on the contrary, the least quantities we can treat of are those which are perceptible to the senses; but if the measured effects are really small, any *joint* effect is likely to be altogether imperceptible. For instance, in the expansion of a solid body, we regard the cubic expansion as three times as great as the linear expansion. The coefficients of expansion are so small, and so imperfectly determined, that when we expand $(1 + \alpha)^3$, we neglect the minute quantities specified by the second and third powers of α. For α being a very small fraction, its square becomes an entirely negligible quantity, and still more so its higher powers.[3]

In order to establish an empirical formula, we may generally

[1] Jevons, *op. cit.* pp. 471–2.
[2] Cf. Jevons, *ib.* p. 473.
[3] *ib.* p. 478.

assume, in actual practice, that the quantities involved will approximately conform to a law of the form

$$y = A + Bx + Cx^2,$$

in which x is the variable and y the variant. From the experimentally determined series of corresponding values, which should be arranged in a table, of the variable x and the variant y, we select three pairs, and, substituting them in the general equation, we solve the three resulting equations, and so obtain the values of the constants A, B, and C. We can now write down the empirical formula. It will usually be found that the formula thus obtained will yield the other numbers of the table to a considerable degree of approximation.

As an example, we may take one of Perot's determinations of the densities of saturated vapours.[1] Perot's method depended, in principle, on the isolation and weighing of a certain volume of the particular saturated vapour. The results for ether are given in the following table:—

SPECIFIC VOLUME OF ETHER VAPOUR IN CUBIC CENTIMETRES

Experiment ...	(*a*)	(*b*)	(*c*)	(*d*)	(*e*)	(*f*)	(*g*)
Temperature ...	28·4	30·0	31·7	31·9	57·9	85·5	110·5
Specific Volume	426·2	400	375·1	373	168	77·77	43·94

We now select any three of these results, say, (*b*), (*d*), and (*e*),[2] and, substituting their respective values in the general equation $A + Bx + Cx^2 = y$, we have

$$\left.\begin{aligned} A + 30B + 900C &= 400 \\ A + 31{\cdot}9B + 1017{\cdot}61C &= 373 \\ A + 57{\cdot}9B + 3352{\cdot}41C &= 168 \end{aligned}\right\}$$

Solving in the usual way, we find that A = 1043·27, B = − 28·24, and C = ·227. Hence our empirical formula is

$$v = 1043{\cdot}27 - 28{\cdot}24t + {\cdot}227t^2.$$

The next step is to see if the empirical formula thus found agrees with the remaining experimental results. We find that it does

[1] See Perot, *Journal de Physique*, tom. vii, p. 129. Cf. Preston, *Heat*, pp. 344-5.

[2] Such a selection is not very likely to produce an acceptable formula. The results selected should always be far apart, and as far as possible equidistant.

agree with (*a*) and (*c*). It therefore covers the first five cases, (*a*), (*b*), (*c*), (*d*), and (*e*). But it fails in the case of both (*f*) and (*g*), and in these instances the approximation is so slight that we are driven to the conclusion either that the experimental results are wrong, or that the underlying law is more complex than would appear from the formula established. The best plan now is to take a new group of three cases, say (*a*), (*e*), and (*g*),[1] and see how nearly the formula derived therefrom, viz.,

$$v = 802{\cdot}62 - 15{\cdot}47t + {\cdot}079t^2,$$

covers the remaining cases. This new formula will be found hardly more satisfactory than the other. It thus becomes necessary to formulate an equation involving higher powers of the variable, though, of course, to solve an equation with as many as six or seven unknowns, especially with such numbers as those given, is a somewhat formidable task.[2] It is always possible that some of the experimental results may be wrong, owing to experimental errors which are either beyond control or perhaps unsuspected. For instance, if in the series of experiments just mentioned, water vapour had been under examination, temperatures much above 100° could not be employed, on account of the solvent action of water vapour on glass, at high temperatures.

It will often happen that even the second power of the variable will be unnecessary. Regnault found that the results of his inquiry into the latent heat of steam at different pressures were represented with sufficient accuracy by the formula

$$Q = 606{\cdot}5 + 0{\cdot}305t,$$

where Q is the total heat of the steam and t the temperature.[3]

On the other hand, it is sometimes necessary to include the third power of the variable. In the expansion of liquids, for instance, physicists assume the law to be of the form

$$\delta = at + bt^2 + ct^3,$$

and they calculate from the results of observation the value of the

[1] These are the best three. See the last footnote.

[2] The reader would do well to face this task. To work out a series of results, to compare them, and to discover exactly why they differ, will throw much light upon the principles underlying empirical formulæ.

[3] See Preston, *Heat*, p. 313. The pressures varied from ·22 to 13·625 atmospheres. In thirty-eight experiments made under the ordinary atmospheric pressure, the mean value of the total heat was found to be 637·67, the extreme values in the series being 635·6 and 638·4.

three constants a, b, and c, which are usually very small quantities. In the case of water, Kopp used the formula

$$V = 1 - at + bt^2 - ct^3$$

for the volume at any temperature t of a mass occupying unit volume at zero. For liquids at temperatures above the normal boiling-point, Hirn expressed the dilatation Δ by means of formulæ of the type[1]

$$\Delta = at + bt^2 + ct^3 + dt^4.$$

Thus, in the case of water, the volume being taken equal to unity at zero, the volume at any temperature θ between 100°C, and 200°C, was given by the formula

$$v = 1 + 0{\cdot}00010867875\theta + 0{\cdot}00000300736 53\theta^2 + 0{\cdot}00000000287304 22\theta^3 - 0{\cdot}00000000000664 57031\theta^4.$$

Theoretically speaking, the process of empirical representation might be applied with any degree of accuracy; we might include still higher powers in the formula. In a similar manner, periodic variations may be represented, to any required degree of accuracy, by formulæ involving the sines and cosines of angles and their multiples.[2]

§ 4. Rational Formulæ

It must be clearly understood that all these empirical formulæ do not coincide with natural laws. They are only approximations to the results of natural laws, and it is upon the general principles of approximation that they are founded. *We do not learn what function the variant is of the variable*, but we obtain another function, which, within the bounds of observation, gives nearly the same values.

Let us consider the case of a stone which is projected vertically downwards. Five observations are made, and the results are as follows:—

Number of seconds after the start ...	2	3	3¾	5¼	6
Number of feet covered after the start	88	180	270	504	648

Taking the general formula $s = a + bt + ct^2$, and substituting the

[1] For details, see Preston, *Heat*, p. 185.

[2] See, for example, *On Tides and Waves*, by Sir G. B. Airy.

first, third, and fifth of the pairs of results (the best selection possible), we have

$$\left.\begin{aligned} a + 2b + 4c &= 88 \\ 16a + 60b + 225c &= 4320 \\ a + 6b + 36c &= 648 \end{aligned}\right\}$$

which gives $a = 0$, $b = 12$, $c = 16$. The formula therefore is

$$s = 12t + 16t^2.$$

This will be recognized at once as the ordinary formula connecting space and time in the case of falling bodies. (The value of g in this case is 32; and 12 represents, of course, the initial velocity of projection.)

But it need hardly be said that the above numbers were not obtained from actual experiments. They were made up from previous knowledge, and specially for purposes of illustration. Actual experiments, no matter how carefully performed, would have yielded results only approximately accurate, and the consequent complex empirical formula might or might not have given a clue to the rational formula $s = \frac{1}{2}gt^2$. But this particular relation has long passed the empirical stage, and our knowledge of the action of gravity not only enables us to establish the relation in a different way, but clearly to see the *reason* underlying the relation.

The graph of an empirical formula will be a curve approximating the true curve, but will give us no information concerning the precise nature of the true curve. Indeed, the curve obtained may be such a fragment, so to speak, of the whole curve, that it gives us scarcely the slightest clue to the relation between the quantity of the cause and the quantity of the effect.

What we are seeking is the *rational* formula or function,[1] which will exhibit the exact nature and origin of the law connecting the phenomena. Given the quantities, we want the function of which they are the values. The discovery of this function is often extremely difficult, and not infrequently it seems absolutely impossible to make any headway whatever beyond the empirical law.

We *may*, it is true, discover the rational function by purely haphazard trial, for we are always at liberty to invent any mathematical formula we like, and then try whether, by the suitable

[1] Any quantity which depends upon and varies with another may be called a *function* of it, and either may be considered a function of the other Literally, a rational formula shows the "reason" of the law connecting the phenomena.

selection of values for the unknown constant quantities, we can make it give the required results. But the chance of succeeding in this manner is very small, for the number of possible functions is practically without limit, and even the number of comparatively simple functions is so large that the possibility of falling on the correct one by mere chance is only slight. We do, however, usually obtain the law by a deductive process of some kind, not by showing that the numbers give the law, but that the law gives the numbers.[1]

The better plan is to note the general character of the variation of the quantities, trying, by preference, functions which give a similar form of variation. A survey of the numbers will often give us a general notion of the kind of law they are likely to obey, and we may gain much assistance by drawing their graph. We can in this way ascertain with some probability whether the curve is likely to return into itself, or whether it has infinite branches; whether such branches are asymptotic; whether it is logarithmic in character, or trigonometric. This indeed we can only do if we remember the results of previous investigations, and a complete familiarity with different classes of curves is indispensable. Once we can discover the *class* of functions to which the required law belongs, our chances of success are much increased because our work, whether by haphazard trial or otherwise, is then brought within much narrower limits. But unless we have the greater part of the curve before us, the identification of its character must be a matter of great uncertainty; for limited portions of curves of almost any character can be made to approximate to one another. Clearly, then, both insight and mathematical knowledge are needed to obtain the correct *form* of the function; but its form once obtained, the remaining work is mere computation, the unknown constants being determined, in the manner already explained, by making selections from our experimental results. We thus get the function itself, and now try, as before, whether it gives with sufficient accuracy the remainder of our experimental results.

It is obvious, then, that to discover the form of function most likely to suit, we shall almost always have to draw freely upon our previous knowledge and to depend upon analogical reasoning. The general nature of the phenomenon will often show at once whether the law is one of direct simple proportion, or of an exponential form; and so on. Any influence which spreads freely through tridimen-

[1] Cf. Jevons, pp. 489-90.

sional space will, of course, be subject to the law of the inverse square of the distance. But no general rules can be given. Knowledge and insight are alone likely to ensure success.

Success, however, is by no means always certain. In many important branches of Science it seems almost impossible to detect the precise laws, and the rational formulæ are, therefore, necessarily unknown. The pressure of saturated vapours at different temperatures, for instance, has been determined by experiments conducted with extraordinary care, but no incontestable general law has been established. All sorts of formulæ have been suggested,[1] but none can be said to correspond very closely with the actual experimental results. Then, again, some of the greatest men of Science[2] have spent much labour in trying to discover some general law of atmospheric refraction, but all to no purpose.

§ 5. Variation in Simple Proportion

In quantitative investigations our first impression is often likely to be that one quantity *varies directly* as another, thus obeying the law $y = mx + n$, and this is often actually the case. For instance, the heat produced by friction is exactly proportional to the mechanical energy absorbed; and if electricity is converted into heat we have again simple proportion. Wherever, in fact, one thing is but another thing with a new aspect, we may expect to find this law. But it is necessary to distinguish between the cases where this proportionality is really true and where it is only apparently true. A small portion of any curve, for instance, will appear to be a straight line, and when our modes of measurement are comparatively rude, we must expect to be unable to detect the curvature. Kepler made many attempts to discover the law of refraction of light, and he approximated to it when he observed that the angles of incidence and refraction, *if small*, bear a constant ratio to each other; for angles, when small, do, of course, vary nearly as their sines. It would be well to look upon every law of simple proportion as only provisionally true until reason to the contrary is shown.[3]

[1] For instance, Young suggested $p = (a+b\theta)^m$, in which a, b, and m are constants to be determined by experiments. Biot suggested log. $p = a + ba^\theta + c\beta\theta$. Roche used a formula of the type $p = aa^{\frac{\theta}{m+n\theta}}$. (See Jamin, *Cours de Physique*, vol. ii., p. 138; Young, *Nat. Phil.*, vol. ii, p. 440; Biot, *Connaissance des Temps*, 1844; Dulong and Arago's Memoir, *Mém. de l'Institut*, tom x, p. 227; Preston, *Heat*, pp. 330-1; Jevons, *Prin. of Sci.*, pp. 499-501.)

[2] For instance, Kepler, Newton, and Laplace.

[3] Cf. Jevons, *Prin. of Sci.*, pp. 483-503.

§ 6. Theory and Experimental Results

It should be noticed that the great bulk of quantitative facts recorded by scientific investigators, have not been brought under any theoretical system. The results are empirical only. A phenomenon may be measured, but no explanation may be forthcoming as to why it should possess any particular quantity, or to connect it by theory with other quantities. The tables of numerical results which abound in books on Chemistry and Physics, the records of observations of public Observatories, the numerous tables of meteorological observations,—are, for the most part, results of a merely empirical character; either theory is defective, or the labour of calculation and comparison is too formidable. Of course, purely empirical measurements may have a direct practical value, as when tables of specific gravities, or strengths of materials, assist the engineer; or when a knowledge of the refractive index of various kinds of glass enables the optician to make achromatic lenses; but, in such cases, the use made of the measurements is not scientific but practical.[1]

If, by means of a theory, we can not only predict the nature of a phenomenon, but also assign the precise quantity of a phenomenon, we have an excellent test of the probable truth of the theory. It was in this manner that Newton first attempted to verify his theory of gravitation. He knew approximately the velocity produced in falling bodies at the earth's surface; and if the law of the inverse square of the distance held true, and the reputed distance of the moon was correct, he could infer that the moon would fall towards the earth at the rate of 15 ft. in one minute. Now the actual divergence of the moon from the tangent of its orbit appeared to amount only to 13 ft. in one minute. This discrepancy of 2 ft. caused Newton to "lay aside at that time any further thoughts on this matter". Many years afterwards, he obtained more precise data from which he could calculate the size of the moon's orbit, and he then found the discrepancy to be inappreciable. His theory of gravitation was thus verified as far as the moon was concerned. This was to him only the beginning of a long course of deductive calculations, each ending in a verification.[2]

It may happen that we are able from certain quantitative experiments and a correct theory, to determine the amount of a phenomenon which we either cannot measure at all, or cannot measure with sufficient accuracy to verify the prediction which the

[1] Jevons, *op. cit.* pp. 551-3. [2] *ib.* pp. 555-6.

theory enables us to make. For instance, the specific heat of air[1] was believed, on the grounds of direct experiment, to amount to 0·2669, but the methods of experiment were open to sources of error. Rankine calculated in 1850, from the mechanical equivalent of heat and other thermodynamic data, that the number ought to be 0·2378. This determination was then accepted, though not verified. Subsequently Regnault obtained by direct experiment the number 0·2377, proving that Rankine's estimate was well grounded.

It is evident that, in quantitative questions, verification is a matter of degree and probability. Many quantities are assigned on theoretical grounds which we are quite unable to verify with corresponding accuracy. The thickness of gold leaf, the average depths of the oceans, the velocity of a star's approach to the earth, are cases in point. Physicists have measured light-undulations, and we also know the velocity with which light travels; from these data we can estimate that about 600,000,000,000 undulations must strike the retina of the eye in one second. But how by direct counting could we verify such a number?

§ 7. Discordance between Theory and Direct Measurement

It frequently happens that there is a serious want of accordance between the theory adopted and the results of direct measurement. There are several possible causes of this: the direct measurements may be erroneous; theory may be correct as far as regards the general form of the supposed laws, but some of the constant numbers or other quantitative data employed in the theoretical calculations may be inaccurate; the theory may be false, in the sense that the forms of the equations assumed to express the laws of nature are incorrect; the theory and the quantities concerned may be approximately correct, but some regular unknown cause may have interfered so that the divergence may be regarded as a *residual effect* representing possibly a new phenomenon.

No precise rules can be laid down whereby the investigator can overcome such difficulties. He must depend on his own insight and knowledge, though certain points will always suggest themselves to him. He will, for instance, increase the number of his experiments; he may find it necessary to devise other apparatus or to modify his materials; or he may approach the subject in an entirely new way.

[1] That of water being taken as unity.

He must continue to vary the circumstances, in the hope that the source of the inconsistency will at last reveal itself. Of course he may, finally, have to abandon his original hypothesis, but not until he can form another which yields a more accurate accordance, in which case the new one will have first claim upon his attention.[1]

CHAPTER XXV

Error and its Correction

§ 1. Exact Measurement is virtually Impossible

It will have become evident that the knowledge we acquire in experimental investigation is only of an approximate character; it is, in fact, a rare thing to reach laws which are absolutely true, and exact to the last degree. Some people, for example, consider it *proved* that planets move in ellipses; but to "prove", that is to demonstrate with certainty, that the orbits are elliptical, is beyond our resources; all that we can do is to show that the orbit of an unperturbed planet approaches *very nearly* to the form of an ellipse, and more nearly the more accurately our observations are made. But to assert that the orbit *is* an ellipse is to pass beyond our data and to make an assumption which cannot be verified by observation. And, as a matter of fact, no planet does move in a perfect ellipse; the mutual perturbations of the planets distort the elliptical paths.

We could never prove the existence of perfectly circular or parabolic movement, even if it existed. The circle is a particular case of the ellipse, for which the eccentricity is zero; but if the orbit of a planet were a circle we could never prove the entire absence of eccentricity; we could not do more than declare that the divergence from the circular form was inappreciable. Again, we can conceive the existence of a comet moving in a parabolic orbit; but owing to the particular limit which the parabola occupies between the ellipse and hyperbola,[2] we could never prove that the comet so moved.[3]

[1] Cf. Jevons, *op. cit.* pp. 558–60. The whole of ch. xiii, xiv, xxii, and xxv of Jevons will repay careful reading.

[2] The hyperbola should be considered in its most general sense,—the curve formed by the intersection of a plane and a double cone,—and not confined to the case where the plane cuts the cone parallel to its axis The plane must make a greater angle with the base of the cone than the side of the cone makes. (With an equal angle we have a parabola, with a smaller angle an ellipse.)

[3] See Jevons, *Prin. of Sci.*, pp. 456–8.

§ 2. The Assumptions made by Science

We seldom realize, perhaps, what great assumptions we make in scientific investigation, and how our knowledge must therefore be largely of a hypothetical and merely approximate character. We base calculations upon the assumed existence of inflexible bars, inextensible lines, heavy points, homogeneous substances, perfect fluids and gases; but as probably none of these things have any real existence, we cannot say that our problems are ever finally solved. And even the very best of the instruments with which we perform our measurements are imperfect. We assume a plumb line gives a vertical line, but this can never be true in the absolute sense, owing to the attraction of mountains and other inequalities in the surface of the earth. We assume the surface of mercury to be a perfect plane, but even in a breadth of five inches there is a divergence from a true plane of about one ten-millionth part of an inch; we assume that in the torsion balance the force of torsion of a wire is proportional to the angle of torsion, but this is true only for infinitely small angles. Even the pendulum—our most perfect instrument—is not theoretically perfect, except for infinitely small vibrations.[1]

There is not, of course, any inexactness in the laws of nature; the inexactness is in our data. And so far as assumption enters in, so far want of certainty will attach to our conclusions. Yet there are occasions when we seem warranted by our data in assuming the existence of an exact law, and using it in preference to the numerical results which are at best only approximate. Dalton's laws of definite combining proportions never have been exactly proved; but chemists having shown, to a considerable degree of approximation, that the elements combine together as if each element had atoms of an invariable mass, assume that this is exactly true. Chemists thus step beyond their data; they throw aside their actual experimental numbers and boldly assume that the discrepancies are due to experimental errors.[2]

§ 3. Interfering Causes

When we wish to attain rigid accuracy, it is surprising how many possible causes of error may enter into even the simplest experiments. We cannot, for instance, perform the common experiment of testing the truth of Boyle's Law, without paying regard to (1)

[1] Jevons, *op. cit.* pp. 456-61. [2] Cf. Jevons, *ib.* pp. 462-5.

the variations of atmospheric pressure which are communicated to the gas through the mercury; (2) the compressibility of mercury, which causes the column of mercury to vary in density; (3) the temperature of the mercury throughout the column; (4) the temperature of the gas, which is with difficulty maintained invariable; (5) the expansion of the glass tube containing the gas. Although Regnault took all these circumstances into account in his examination of the law, there is no reason to suppose that he exhausted the sources of inaccuracy.[1]

A measurement which aims at any considerable degree of exactness is a very delicate and usually complex operation, and much of the difficulty arises from the fact that it is scarcely ever possible to measure a single effect at a time. Thus, if we wish to measure the expansion of a liquid by heat, we observe the rise of a column of liquid in a narrow glass tube. But we cannot heat the liquid without heating the glass, so that the change observed is really the difference between two expansions. Careful investigation will show the necessity of allowing for further effects, for example, the compression of the liquid, and the expansion of the bulb due to the increased pressure of the column as it becomes lengthened. The variation in the height of the barometer is another complex effect, being partly due to the real variation of the atmospheric pressure and partly to the expansion of the mercurial column by heat.[2]

As, however, our object in an experiment is to measure a single effect only, we always endeavour to obtain that effect free from interfering effects; and if we cannot get rid of the interfering effects altogether, we reduce them to a minimum. We try, then, to adopt some means of counteracting interfering causes. It should, however, be noted that those quantities which are called *errors* in one case may become important phenomena in another investigation. When we speak of "eliminating error", we really mean isolating a particular phenomenon and freeing it, as far as possible, from interfering causes. Several methods of eliminating error are recognized.

§ 4. Elimination of Error

In the *first* place we may devise an experiment, or opportunity of observation, in which error is *avoided*, or at all events rendered

[1] See Jevons, *op. cit.* p. 468; and Jamin, *Cours de Physique*, I, pp. 282–3.

[2] Cf. Jevons, *ib.* pp. 336–8. The term "expansion" is here used in its general sense, contraction being regarded as negative expansion.

inappreciable. An astronomer, for example, is unable to assign any satisfactory law to atmospheric refraction; he therefore avoids, as far as possible, making observations of an object when near the horizon, and waits till it reaches the highest point of its daily course. An astronomer also places his principal controlling clock in a cellar or other place where the changes of temperature, being very slight, will not affect the length of the pendulum.[1] Dulong and Petit's method of measuring the expansion of mercury enabled them to avoid the difficulty arising from change of dimension in the containing tubes.

Sometimes an experiment may be rendered valueless owing to the existence of error which cannot be avoided. Foucault's experiment for demonstrating the rotation of the earth, for instance, is of no use for purposes of *exact* measurement; it is practically impossible to avoid giving the pendulum a slight lateral motion; the consequence is an elliptic path with a progressive motion of the axis of the ellipse, a motion which disguises that due to the rotation of the earth.[2]

In the *second* place, we may sometimes measure phenomena in such circumstances that the error remains very nearly the same in all the observations. This method is available when we want a *difference* between quantities, and not the absolute quantity of either. In Leslie's Differential Thermometer, for instance, any alterations of the temperature of the air will affect the equal bulbs equally, and produce no change in the indications of the instrument. Only that radiant heat which is purposely thrown upon one of the bulbs will produce any effect.[3]

A *third* method is known as the method of *corrections*. Whenever the result of an experiment is affected by an interfering cause to a calculable amount, it is sufficient to add or subtract this amount. We are said to "correct observations" when we thus eliminate what is due to extraneous causes. The variation in the height of the barometer, for instance, is partly due to the change of temperature, but since the coefficient of absolute expansion of mercury is known, the necessary correction for temperature is a simple matter.

When we come to use instruments of great accuracy, there are many minute sources of error which must be guarded against. If a thermometer, for example, has been graduated when vertical, it will

[1] This is an additional check to the compensatory arrangement for the change of temperature

[2] Cf. Jevons, *op. cit.* pp. 340–4. See also *Phil. Mag.*, 1851, fourth series, vol. ii.

[3] Cf. Jevons, *ib.* p. 345, and Leslie, *Inquiry into the nature of Heat*, p. 10.

read somewnat differently when laid flat, since the pressure of a column of mercury is removed from the bulb. The reading may also be somewhat altered if it has recently been raised to a higher temperature than usual, if it be placed under an exhausted receiver, or if the tube be unequally heated as compared with the bulb. For these minute causes of error we may have to introduce troublesome corrections. Again, the measurement of quantities of heat is a matter of great difficulty because there is no known substance impervious to heat, and the correction of the consequent experimental errors often taxes the resources of our ablest physicists; it is very much like trying to measure liquids in porous vessels.[1]

A *fourth* method is that of *compensation.* Here we adopt some means of neutralizing the interfering cause by balancing against it an exactly equal and opposite cause of unknown amount. We cannot, for instance, weigh an object with great accuracy unless we make a correction for the weight of the air displaced by the object. When a chemist wishes to weigh gas in a large glass globe, he avoids the error and the labour of correcting it by attaching to the opposite scale of the balance a dummy sealed glass globe of equal capacity to that containing the gas to be weighed. In the astatic galvanometer, we have another illustration of the principle of the method.

A *fifth* method of eliminating error may be adopted when we can so *reverse* our mode of procedure as to make the interfering cause act alternately in opposite directions. If we can get two experimental results, one of which is as much too great as the other is too small, the error is equal to half the difference, and the true result is the *mean* of the two apparent results. It is by this method that we are able to ensure accuracy in, for example, the use of the dip-needle.[2]—This leads us to the consideration of the "Method of Means".

§ 5. The Method of Means

Any person who uses a scientific instrument of great precision and registers successive observations in an unbiased manner, will invariably find that the results differ. Only the careless investigator will think that his observations agree. The more accurate our modes of observation are rendered, the more numerous are the sources of minute error which become apparent. We may, in fact, look upon the existence of error in all measurements as the normal

[1] Cf. Jevons, *op. cit.* pp. 346–50.

[2] See Jevons, *ib.* pp. 354–6.

state of things. Experimental results which agree too closely should raise our suspicions. If, then, we cannot get exactly the same result twice over, the question arises, how can we ever attain the truth, or select the result which may be supposed to approach most nearly to it? It is clear that if the quantity of a certain phenomenon is expressed in several differing numbers, only one at most can be true, and very likely all are false. Common sense suggests that we must take the *mean*, and mathematical reasoning shows that the mean is very likely to bring us near the truth.

There are several kinds of means,[1] the commonest of which is the *arithmetic mean*. This is often referred to as "the mean". The arithmetic mean of a series of quantities is, of course, the sum of the series divided by their number. If a and b be two numbers, their arithmetic mean is $\frac{1}{2}(a + b)$. The *geometric mean* is $\sqrt{ab}$.

The geometric mean is necessarily adopted in certain cases. When we estimate the work done against a force which varies inversely as the square of the distance from a fixed point, the mean force is the geometric mean between the forces at the beginning and end of the path. When in an unperfect balance we eliminate error by adopting the method of Gauss, weighing first in one pan and then in the other, the true weight is the geometrical mean of the two apparent weights.[2]

A *mean result* sometimes signifies a merely representative number, expressing the general magnitude of a series of quantities. Such a number is sometimes called the *fictitious mean* or the *average result*. In popular usage, however, the terms *mean* and *average* are synonymous; and even in Science they are sometimes indifferently used. But although the term *average*, when employed in the sense of a fictitious mean, represents no really existing quantity, it is yet of great scientific importance, as enabling us to imagine a number of particular details generalized in a single result. The weight of a body, for example, is the sum of the weights of a number of infinitely small particles, each acting at a different place, but we may regard the weight of all the particles as concentrated in a particular point,—the Centre of Gravity,—and the behaviour of the whole

[1] The old mathematicians recognized ten.

[2] Thus, if a body of true weight W weighs A when placed in the right-hand pan, and B when placed in the left-hand pan, then calling R and L the lengths of the respective arms, we have $WR = AL$; $WL = BR$; $\therefore W^2 = AB$; $\therefore W = \sqrt{AB}$. Since A and B are as a rule very nearly equal, we may in most cases use the arithmetic mean instead. Thus the arithmetic mean of 1·000 and 1·001 is 1·0005, whilst the geometrical mean is 1·0004998. It would be impossible to detect the difference between the two by the balance.—See Stewart and Gee, *Practical Physics*, vol. i, p. 89, or Glazebrook and Shaw, *Practical Physics*, pp. 99–118.

body will be exactly represented by the behaviour of this imaginary heavy point. Terrestrial gravity is a case of approximately parallel forces, and the centre of gravity is but a special case of the more general Centre of Parallel Forces. Wherever a number of forces of whatever amount act in parallel lines, it is possible to discover a point at which the algebraic sum of the forces may be imagined to act with exactly the same effect. Thus we have the Centre of Pressure, the Centre of Percussion, and so on.

But we ought to distinguish between those cases in which an *invariable* centre can be assigned, and those in which it cannot. Strictly speaking, there is no such thing even as an invariable centre of gravity. As a general rule, a body is capable of possessing an invariable centre only for perfectly parallel forces, and gravity never does act in absolutely parallel lines. Again, we familiarly speak of the poles of a magnet. But, strictly, the poles are not the ends of the magnet, nor any fixed points within, but the variable points from which the resultants of all the forces exerted by the particles in the bar upon exterior magnetic particles may be considered as acting. The poles are, in short, Centres of Magnetic Forces; but as these forces are never really parallel, the centres will vary in position according to the relative position of the object attracted.[1]

One mode of employing the mean result is analogous to the method of reversal, a method which is extensively practised in some branches of physical Science. We have a simple instance in the determination of the latitude of a place by observation of the Pole Star. If the elevation of any circumpolar star be observed at its higher and lower passages across the meridian, half the sum of the elevations gives the height of the pole, which, of course, is equal to the latitude of the place. Such a star is as much above the pole at its highest passage as it is below at its lowest, so that the mean must give the height of the pole free from doubt, except as regards incidental errors.[2]

Sometimes we are able to eliminate fluctuations and take a mean result by purely mechanical arrangements. The daily variations of temperature, for instance, become imperceptible one or two feet below the surface of the earth, so that a thermometer placed with its bulb at that depth gives very nearly the true daily mean temperature.[3]

[1] Cf. Jevons, *op. cit.* pp. 363–5. Cf. also Venn, *Logic of Chance*, ch. xviii (on averages). The distinction between "average" and "mean" is perhaps a little artificial.

[2] Cf. Jevons, *ib.* p. 366. [3] *ib.* p. 368.

It is frequently very difficult to determine exactly the zero point from which we desire to measure, and in some cases it is actually better to determine it by the average of equally diverging quantities, than by direct observation. In delicate weighings with a chemical balance, for instance, it is requisite to ascertain exactly the point at which the beam comes to rest, but it is often better to let the beam vibrate and observe the terminal points of the vibrations. The mean between two successive extreme positions will nearly, but not quite, indicate the position of rest; for the swings gradually decrease, owing to friction and to resistance of the air. We therefore observe a third terminal point, on the same side as the first, and then reason thus: the single swing to the right is, say, 125; the two swings to the left, 63 and 69; we may therefore assume that the mean of 63 and 69, that is 66, would have been the left-hand turning-point at the moment at which it was 125 on the other, had the pointer been swinging in the opposite direction. The mean of 125 and 66 is 95·5, which may therefore be regarded as the resting-point. We may, if we wish, observe another turning-point to the right, say, 120; then we have another such series. Proceeding thus, we get a set of determinations of the resting-point, the mean of which will give us the true position with great accuracy.[1]

§ 6. The Law of Error

It will have been understood that the term *error*, as here used, merely means *discordance*, of which the cause is unknown. The error may arise from some law of nature not known to the observer; it may arise from the imperfection of the observer's senses; it may arise from the personal constitution of the observer, that is, his particular habit or temperament which causes him to differ from other persons in his method of observing; it may arise from some imperfection peculiar to the apparatus employed,—a graduated metal scale, for instance, undergoes daily expansion and contraction by variations of temperature. Now before any trials are made, that is, before anything is known of the character of the observer or of the apparatus he uses, we can have no reason to suppose that any one observation is more likely to exceed the truth than to fall short of it. When any observation is *greater* than reality, the error

[1] That is, the resting-point when the pans are empty. See Glazebrook and Shaw, *Practical Physics*, pp. 110, 111.

is called *positive*; when less, *negative*. The hypothesis, therefore, of an equal presumption for positive and negative errors, is one with which we must commence; and it follows from the supposition that the mean is the most probable result of a number of discordant observations. The sum of all the observations will be without error itself if the amount of the positive errors be equal to that of the negative ones. This last supposition, though not probable in itself, is nevertheless more probable than any other, and the odds are very much in favour of its being very nearly true. Now whatever may be the error of the sum of observations, say 100 in number, the average, or the hundredth part of the sum, contains only the hundredth part of that error; and the presumption that such an average is very close indeed to the truth greatly exceeds the probability in favour of any one of the observations.[1]— All this seems to suggest the possibility of reducing error to Law.

In point of fact, mathematicians have established a "Law of Error", a law which not only enables us, among discordant results, to approximate to the truth, but to assign the degree of probability which fairly attaches to this conclusion. Mathematicians agree, however, far better as to the *form* of the Law than they do as to the manner in which it can be deduced and proved. They agree that, among a number of discordant results of observation, *that mean quantity is probably the best approximation to the truth, which makes the sum of the squares of the errors as small as possible.* But the whole subject is much too difficult for general treatment here, and can be touched upon only in some of its more elementary aspects.[2]

§ 7. How the Law has been Arrived at

Mathematicians have arrived at the Law in different ways. Gauss proceeds much upon assumption; Herschel depends upon geometrical considerations; Laplace and Quetelet regard the Law as a development of the doctrine of combinations. The last-mentioned method is happily illustrated by Jevons. The illustration, simplified and shortened, is as follows.

Let us assume that a particular observation is subject to six

[1] Cf. De Morgan, *Probability*, pp. 128–34.

[2] The standard Law of Error, commonly called the Exponential Law, is expressed in the formula $y = Ye^{-cx^2}$, in which x is the amount of the error, Y the maximum ordinate of the curve of error, and c a number constant for each series of observations and expressing the amount of the tendency to error, varying between one series of observations and another; e is the mathematical constant. See Jevons, pp. 374–82.

chances of error, each of which will increase the result 1 inch if it happens. Each of these errors is to be regarded as an event independent of the rest, and we can therefore assign, by the theory of probability, the comparative probability and frequency of each conjunction of errors. By giving x in ${}^{6}C_{x}$ all values from 0 to 6, we see that no error at all can happen in only 1 way; an error of 1 inch can happen in 6 ways; and the ways of happening of errors of 2, 3, 4, 5, and 6 inches will be 15, 20, 15, 6, and 1 in number, as in the following table:—

Amount of Error, in inches	0	1	2	3	4	5	6
Number of Errors	1	6	15	20	15	6	1

Obviously the error *most likely* to occur is that of 3 inches, and will occur in the long run in 20 cases out of 64. Errors of 2 and 4 inches will be equally likely, but will occur less frequently; errors of 1 and 5 inches still less frequently; while no error at all and one of 6 inches will be a comparatively rare occurrence.

Let us now suppose the errors to act as often in one direction as in the other. There will thus be three positive causes of error and three negative causes, and we may tabulate the numbers of errors of various amounts thus:—

	Positive Error.				Negative Error.		
Amount of Error, in inches	3	2	1	0	1	2	3
Number of Errors	1	6	15	20	15	6	1

From this table we easily ascertain the probability of any particular amount of error under the conditions supposed. The probability of a positive error of exactly 1 inch is $\frac{15}{64}$, in which fraction the numerator is the number of combinations giving 1 inch positive error, and the denominator the whole number of possible errors of all magnitudes. By adding together the appropriate numbers we can get the probability of an error not exceeding a certain amount. Thus the probability of an error of 2 inches or less is (6 + 15 + 20 + 15 + 6)/64 or 62/64. Evidently, the probability of small errors is far greater than of large ones; for example, the odds are 62 to 2 or 31 to 1 that the error will not exceed 2 inches.

But to assume any special number of causes of error is an arbi-

trary proceeding, and mathematicians have chosen the least arbitrary course by imagining the existence of an infinite number of infinitely small errors.[1] Upon this basis they have proceeded to establish the Law of Error already mentioned. It should be noticed that the Law allows of the possible existence of errors of every assignable amount, a fact in itself sufficient to show that it is only approximately true. Although we may fairly say that in measuring a mile it would be impossible to commit an error of a thousand miles, yet the general Law of Error would assign a probability for an error of even that amount, and more, but such a probability would be almost inconceivably small. All that the Law claims to do is to represent the errors in any special case to a very close approximation.[2]

One important fact following immediately from the Law of Error is that *the mean result is the most probable one*; and when there is only a single variable, this mean is found by the familiar arithmetic process. The "Method of Means" may be regarded merely as a special application of the general case, that is, of the "Method of Least Squares".[3]

§ 8. The Probable Error of Results

When, however, we are dealing with cases of importance, we must not be content with finding the simple mean and treating it as true. We must also ascertain *the degree of confidence we may place in this mean.* In some cases the mean may be approximately certain and accurate; in other cases it may be worth little or nothing. The Law of Error enables us to ascertain the degree of confidence proper in any instance, for it shows how to calculate the probability in the case of a divergence of any amount from the mean, and we can thence ascertain the probability that the mean in question is within a certain distance of the true number. By *probable error* mathematicians mean the limits within which it is *as likely as not* that the truth will fall. Thus, if 5·45 be the mean of all the determinations of the density of the earth, and ·20 be approximately the probable error, the meaning is that the probability of the real density of the earth falling between 5·25 and 5·65 is $\frac{1}{2}$. Any other limits might, of course, have been

[1] This course is quite justifiable, considering that "there may exist infinitely numerous causes of error in any act of observation".

[2] Cf Jevons, pp. 374-85.

[3] Whewell stated that the Method of Least Squares is a Method of Means, but, as Jevons pointed out, this is incorrect. Cf. Whewell, *Phil. of Ind. Sci.*, ii, pp. 408-9; and Jevons, *Prin. of Sci.*, p. 386.

selected; for instance, we might calculate the limits within which it was 10 to 1 or 100 to 1 that the truth would fall; but there is a convention to take the even odds of 1 to 1, as the quantity of probability of which the limits are to be estimated.[1]

For making the necessary calculations, works on probability give the following rules:—

1. Find the mean of the observed results.
2. Find the difference, that is the error, between the mean and each observed result.
3. Find the sum of the squares of these errors.
4. Divide by one less than the number of observations. This gives *the square of the mean error.*
5. Take the square root of this last result; this is *the mean error of a single observation.*
6. Divide by the square root of the number of observations; this gives *the mean error of the mean result.*
7. Multiply by the natural constant 0·6745; this gives *the probable error of the mean result.*

For purposes of illustration Jevons gives the following example: Suppose the five measured heights of a hill to be 293, 301, 306, 307, and 313 ft. We require to know the probable error of the mean.

1. The mean is 304.
2. The differences ("errors") between this mean and the measured heights are 11, 3, 2, 3, and 9.
3. The squares of these errors are 121, 9, 4, 9, and 81, the sum of which is 224.
4. Divide by 1 less than 5, that is 4, and we get 56, the square of the mean error.
5. The mean error of a single observation is thus $\sqrt{56} = 7{\cdot}48$.
6. Divide by $\sqrt{5}$, that is by 2·236, and we have 3·35, the mean error of the mean result.
7. Multiply by 0·6745, and we get 2·259, the probable error of the mean result. (Approximately $2\frac{1}{4}$.)

The meaning of this is that the probability is $\frac{1}{2}$, or the odds are even, that the true height of the hill lies between $301\frac{3}{4}$ and $306\frac{1}{4}$ ft. We thus have an exact measure of the degree of credibility of our mean result.[2]

In these calculations, the object is only to give a notion of the *degree of confidence* with which we view the mean; it is therefore of little use to carry them to any great degree of precision. And it

[1] Cf. Jevons, *op. cit.* p. 387.

[2] Cf. Jevons, *ib.* p. 388.

should be remembered that the probable error has regard only to those causes of errors which, in the long run, act as much in one direction as another; it takes no account of constant errors. The true result accordingly may often fall far beyond the limits of probable error, owing to some unknown constant error or errors. It is always necessary to bear in mind that the mean of any series of observations is the best, that is, the most probable approximation to the truth, only in the absence of knowledge to the contrary. The selection of the mean rests entirely on the probability that unknown causes of error will, in the long run, act as often in one direction as the opposite, and will therefore balance one another.[1]

§ 9. The Method of Least Squares

When two or more unknown quantities are so involved that they cannot be separately determined by the simple Method of Means, we can yet obtain their most probable values by the Method of Least Squares. A simple example of Herschel's[2] illustrates the principle admirably, and the same example is mentioned by Venn.

Suppose that a man had been firing for some time with a pistol at a small mark, say a wafer on a wall. We may take it for granted that the shot-marks would tend to group themselves about the wafer as a centre, with a density varying in some way inversely with the distance from the centre. But now suppose that the wafer which marked the centre was removed, so that we could see nothing but the surface of the wall spotted with the shot-marks; and that we were asked to guess the position of the wafer. Had there been only one shot, common sense would suggest our assuming (of course very precariously) that this marked the real centre. Had there been two, common sense would suggest our taking the mid-point between them. But if three or more were involved, common sense would be at a loss. It would feel that some intermediate point ought to be selected, but would not see its way to a more precise determination, because that on which it is accustomed to rely,—the arithmetical average,—does not seem at hand here. The rule of Least Squares tells us how to proceed. It directs us to select that point which will render the sum of the squares of all the distances of the shot-marks from it the least possible.

In practice such a problem would reduce itself to taking what may be conveniently called the "centre of gravity" of the shot-

[1] Jevons, *op. cit.* pp. 388–9.

[2] *Nat. Phil.*, pp. 217–8.

marks, all being regarded as of equal weight. Such a centre is, in reality, the "average" of all the marks, as the elementary geometrical construction for obtaining the centre of gravity of a system of points will show.[1]

The Method of Least Squares is the most general mode of finding the true magnitude from a number of divergent measurements, but when these measurements involve one magnitude only, the simplest mode of applying the method is to take the arithmetical mean.[2]

The Law of Error and the Method of Least Squares are things of an entirely distinct kind and must not be confused. The Law of Error is the formulated statement of a precisely ascertained fact; it assigns, with more or less of accuracy, the relative frequency with which errors or deviations of any kind are found in practice to present themselves. It belongs therefore to what may be termed the physical foundations of the science of Probability. The Method of Least Squares,—or its simplified application, the arithmetical average,—is no law whatever in the scientific sense. It is rather a precept or rule for our guidance. It directs us how to treat the errors which tend to occur, when any number of them are presented to us. There is, of course, a relation between the Law and the Method; nevertheless, they are absolutely distinct.[3]

§ 10. The Method of Curves

A further brief reference may be made to the Method of Curves. —Every equation involving two variable quantities corresponds to some kind of plane curve, and every plane curve may be represented symbolically in an equation. In an experimental research, as we saw in the last chapter, we obtain a number of values of the variant corresponding to an equal number of values of the variable. The variant, or quantity whose change we would consider, is made the *ordinate* of the curve; and the variable, or the quantity which we vary at will and on which the changes depend, we make the *abscissa*. If a curved line be drawn through all the points, or ends of the ordinates, it will probably exhibit irregular inflections, owing to the

[1] The reader should take such a series of marks, draw convenient axes, and find, by the usual methods, what for convenience' sake is here called the Centre of Gravity. He should then join each of the marks to this centre, and prove that the sum of the squares of these distances is the least possible. The geometrical construction and algebraic solution are exceedingly simple. See Venn, *Logic of Chance*, pp. 466–8; Welton, pp. 185–7; Whewell, *Nov. Org. Ren.*, pp. 215–6 [2] Cf. Welton, p. 187. [3] Cf. Venn, p. 41.

errors which affect the numbers. But when the results are numerous, it becomes apparent which results are more divergent than others; and, guided by a so-called "sense of continuity", it is possible to trace a line among the points which will approximate to the true law more than the points themselves. We draw the curve "not *through* the points given by observation, but *among* them".[1]

The value of the Method of Curves depends upon the fact that order and regularity are more readily and clearly recognized when pictorially exhibited to the eye than they are when presented to the mind in any other manner. And the curve often enables us to infer numerical results more free from accidental errors than any of the numbers obtained directly from experiment. Further, the form of the curve sometimes indicates the class of function to which our results belong.[2]

[1] See Jevons, *op. cit.* pp. 492–4; Whewell, *Nov. Org. Ren.*, pp. 204–10 —Most readers will probably be quite familiar with ordinary graphic algebra. Those who are not should read through Professor Gibson's *Treatise on Graphs*, which not only deals with the subject in a very lucid manner, but provides a large variety of examples of physical applications. Ch. xi of Earl's *Physical Measurements* will prove helpful to the beginner

[2] Jevons, *op. cit.* p. 494. On the danger of *interpolation*, see Jevons, pp. 495–9.

The subject-matter of this chapter is difficult, and the reader who desires to follow it up must be prepared for a serious task. The nature and theory of average is well dealt with by Venn (*Logic of Chance*, ch. xviii and xix), and several chapters in Jevons's *Principles of Science* are suggestive. Compare also Whewell, *Novum Organum Renovatum*, ch. vii. The *Law of Error* is admirably dealt with in *Ency. Brit.*, vol. xxviii, pp. 280–91, and the same work has an exhaustive article on "Probability" (vol. xix, pp. 768–88). For some instructive elementary remarks on the Exponential Curve, see Venn, *Logic of Chance*, pp. 29 *seq.*; Jevons, *Principles of Science*, ch xvii; and De Morgan, *Probability*, ch. vii Reference may also be made to Quetelet, *Letters on the Theory of Probabilities* (translation by Downes). On the Method of Least Squares, the works of Gauss and Encke are amongst the best known, but Todhunter's contribution to this subject is considered by mathematicians to rank high; it will be found in the *Transactions* of the Camb. Phil Soc , vol. xi, Part II; a reprint in pamphlet form can sometimes be purchased for four or five shillings. Comstock's *Method of Least Squares* is more recent; some typical Error Curves are shown in the frontispiece, and the book contains numerous typical examples. There is a very readable translation of Laplace's *Essay on Probabilities* by Truscott and Emory. Todhunter's *History of Probability* is, of course, a standard work.

BOOK III

FAMOUS MEN OF SCIENCE AND THEIR METHODS

EXTRACTS FROM THE WORKS OF FAMOUS MEN OF SCIENCE

If the reader desires to become acquainted with the practice of scientific discovery, he must do more than make himself familiar with the principles of method. He must acquire a first-hand knowledge of the work of men who have been famous for success in the field of research. From the records of the researches of not a few of these men, it is easily possible to gather clear notions of the methods they adopted. But however careful be his study of the work and methods of a great master, the ordinary man is never likely to rival him. In the first place, there are, of course, enormous differences in degree, between one man and another, of intellectual endowment. In the second place, the successful discoverer always seems able to detect a difference where the ordinary man sees none, and to detect a resemblance where the ordinary man sees only a difference. But let the reader judge for himself. We have room for only a few extracts, but all the works from which the extracts are taken should be read right through. Newton, Faraday, and Darwin should be read again and again.

CHAPTER XXVI

White of Selborne

(1720–1793)

[Gilbert White was a Hampshire curate who wrote a Natural History of his own parish of Selborne. The History consists of miscellaneous jottings of all kinds, written in letter form to brother naturalists. White possessed remarkably keen powers of observation, as the following passages, selected at random, will show.]

From Letter XXVII.[1]—Hedgehogs abound in my gardens and fields. The manner in which they eat their roots of the plaintain in my grass-walks is very curious: with their upper mandible, which is much longer than their lower, they bore under the plant, and so eat the roots off upwards, leaving the tuft of leaves untouched. In this respect they are serviceable, as they destroy a very troublesome weed; but they deface the walks in some measure by digging little round holes. It appears that beetles are no inconsiderable part of their food. In June last I procured a litter of four or five young hedgehogs, which appeared to be about five or six days old; they, I find, like puppies, are born blind, and could not see when they came to my hand. No doubt their spines are soft and flexible at the time of their birth, but it is plain they soon harden; for these little pigs had such stiff prickles on their backs and sides as would easily have fetched blood, had they not been handled with caution. Their spines are quite white at this age; and they have little hanging ears, which I do not remember to be discernible in the old ones. They can, in part, at this age, draw their skin down over their faces; but are not able to contract themselves into a ball as they do, for the sake of defence, when full grown. The reason, I suppose, is because the curious muscle that enables the creature to roll itself up into a ball was not then arrived at its full tone and firmness. Hedgehogs make a deep and warm hybernaculum with leaves and moss, in

[1] To Thomas Pennant.

which they conceal themselves for the winter: but I never could find that they stored in any winter provision, as some quadrupeds certainly do.

From Letter XXXV.[1]—Happening to make a visit to my neighbour's peacocks, I could not help observing that the trains of those magnificent birds appear by no means to be their tails; those long feathers growing not from their uropygium, but all up their backs. A range of short brown stiff feathers, about six inches long, fixed in the uropygium, is the real tail, and serves as the fulcrum to prop the train, which is long and top-heavy, when set on end. When the train is up, nothing appears of the bird before but its head and neck, but this would not be the case were those long feathers fixed only in the uropygium, as may be seen by the turkey-cock when in a strutting attitude. By a strong muscular vibration these birds can make the shafts of their long feathers clatter like the swords of a sword-dancer.

From Letter XVI.[2]—About the middle of May, if the weather be fine, the house-martin begins to think in earnest of providing a mansion for its family. The crust or shell of this nest seems to be formed of such dirt or loam as comes most readily to hand, and is tempered and wrought together with little bits of broken straws to render it tough and tenacious. As this bird often builds against a perpendicular wall without any projecting ledge under, it requires its utmost efforts to get the first foundation firmly fixed, so that it may safely carry the superstructure. On this occasion the bird not only clings with its claws, but partly supports itself by strongly inclining its tail against the wall, making that a fulcrum; and thus steadied, it works and plasters the materials into the face of the brick or stone. But then, that this work may not, while it is soft and green, pull itself down by its own weight, the provident architect has prudence and forbearance enough not to advance her work too fast; but by building only in the morning, and by dedicating the rest of the day to food and amusement, gives it sufficient time to dry and harden. About half an inch seems to be a sufficient layer for a day. Thus careful workmen when they build mud walls (informed at first perhaps by this little bird) raise but a moderate layer at a time, and then desist; lest the work should become top-heavy, and so be ruined by its own weight. By this method in about ten or twelve days is formed an hemispheric nest with a small aperture towards the top, strong, compact, and warm; and perfectly fitted

[1] To Thomas Pennant. [2] To the Hon. Daines Barrington.

for all the purposes for which it was intended. But then nothing is more common than for the house sparrow, as soon as the shell is finished, to seize on it as its own, and to line it after its own manner. The shell or crust of the nest is a sort of rustic work full of knobs or protuberances on the outside: nor is the inside of those that I have examined smoothed with any exactness at all; but is rendered soft and warm, and fit for incubation, by a lining of small straws, grasses, and feathers, and sometimes by a bed of moss interwoven with wool.

CHAPTER XXVII

Alfred Russel Wallace, O.M., F.R.S.

(Born 1823)

[Dr. Wallace, who enjoyed the friendship of Darwin for twenty-five years, is our greatest living naturalist. He is the author of *Man's Place in the Universe*, *Darwinism*, and several other standard works. A good many people dislike his views on Vaccination and on "Spiritualism", but they readily admit the soundness of his reasoning from the facts adduced in support of these views. The following extract is from his latest work, *The World of Life*, written at the age of eighty-seven. Note how he insists upon the distinction of *fact* and *inference*.]

Pp. 148–152.—Before quitting the subject of migration, on which Mr. Seebohm's observations throw so much light, I will shortly describe the most wonderful exhibition of migration phenomena in the world—that of the small island of Heligoland, 40 miles off the mouth of the Elbe in about the same latitude as Scarborough. Most of the migratory birds from Scandinavia and Arctic Europe pass along the coasts of the German Ocean, and the lighthouse on Heligoland serves as a guide, and the island itself as a resting-place, during bad weather. Mr. Seebohm's account of what he witnessed in the island, during nearly a month spent there in September to October, 1875, is most interesting; and I refer to it here chiefly for the sake of pointing out a very important error as to the cause of a very singular fact recorded there by Herr Gatke, who for fifty years observed and registered the migrations both in spring and autumn with great accuracy, and formed a collection of birds there, perhaps more extensive than could be made at any other station in Europe.

The fact observed was, that, during the autumn migration, as regards many of the most abundant species, the young birds of the year, that is, those that had been hatched in the far north in the preceding June or July, and who were, therefore, only about three or four months old, arrived in Heligoland earliest and alone, the parent birds appearing a week or two later. This is the fact. It has been observed on Heligoland for half a century; every resident on the island knows it, and Mr. Seebohm declares that there can be no doubt whatever about it. The inference from this fact (drawn by Herr Gatke and all the Heligolanders, and apparently accepted by almost all European ornithologists) is, that these young birds start on their migration alone, and before their parents, and this not rarely or accidentally, but every year—and they believe also that this is a *fact*, one of the most mysterious of the facts of migration. Neither Mr. Seebohm nor Professor Lloyd Morgan (in his *Habit and Instinct*) expresses any doubts about the *inference* any more than about the *fact*. Yet the two things are totally distinct; and while I also admit the *fact* observed, I totally reject the *inference* (assumed to be also a *fact*) as being absolutely without any direct *evidence* supporting it. I do not think any English observer has stated that the young of our summer migrants all gather together in autumn and leave the country before the old birds; the American observers state that *their* migrating birds do not do so; while many facts observed at Heligoland show that no such inference is required to explain the admitted fact. Let us see what these additional facts are.

The enormous rushes of migratory birds which rest at Heligoland always occur at night and are very intermittent. They usually take place on dark nights, sometimes in millions; at other times a week will sometimes pass with only a few stragglers. Of one such pitch-dark night, Mr. Seebohm writes:—

"Arrived at the lighthouse, an intensely interesting scene presented itself. The whole of the zone of light within range of the mirrors was alive with birds coming and going. Nothing else was visible in the darkness of the night, but the lanthorn of the lighthouse vignetted in a drifting sea of birds. From the darkness in the east, clouds of birds were continually emerging in an uninterrupted stream; a few swerved from their course, fluttered for a moment as if dazzled by the light, and then gradually vanished with the rest in the western gloom. . . . I should be afraid to hazard a guess as to the hundreds of thousands that must have passed in

a couple of hours; but the stray birds that the lighthouse man succeeded in capturing amounted to nearly 300."

He also tells us that 15,000 skylarks have been caught on Heligoland in one night; and all agree that the countless myriads that are seen passing over Heligoland are but a minute fraction of those that really pass, high up and quite out of sight. This is shown by the fact, that if, on a dark night, it suddenly clears and the moon comes out, the swarms of birds immediately cease. Another fact is, that, on what the islanders call "good nights", the birds that come to rest seem to drop down suddenly out of the sky. One other fact is mentioned by Mr. Seebohm. It is that every year the regular migration season is preceded by a week or two during which a few stragglers appear; and these are all old birds and many of them slightly crippled, or partially moulted, or without some of their toes, or only half a tail, or some other defect. These are supposed to be mostly unmated birds, or those whose young have been destroyed. It is also supposed that, during favourable weather (for the birds), migration goes on continuously during the season of about six weeks, though for the most part invisible at Heligoland, but often audible when quite invisible.

Now, the fact of the young birds only appearing on Heligoland for the first week or so of the season of each species is easily explicable. Remembering that the autumnal migration includes most of the parent birds and such of their broods as have survived, it is probable that the latter will form at least half or, more often, two-thirds of each migrating flock. But the young birds, not having yet acquired the full strength of the adults, and having had little, if any, experience in long and continuous flights, a considerable proportion of them on the occasion of their first long flight over the sea, on seeing the lighthouse and knowing already that lights imply land and food-crops below them, and being also much fatigued, will simply drop down to rest just as they are described as doing. The old birds and the stronger young ones, however, pass high overhead, till they reach the north coast of Holland, or, in some cases, pass over to our eastern coasts. We must also remember that the longer the birds are in making the journey overland, the more young birds are lost by the attacks of birds of prey and other enemies. Hence the earliest flocks will have a larger proportion of young birds than the later ones. The earlier flocks also, being less pressed for time, will be able to choose fine weather for the crossing, and thus it will be only the young and quickly fatigued birds that will probably

fly low and come down to rest. Later on, every recurrence of bad weather will drive down old and young alike for temporary shelter and rest. Thus, all the facts are explained without having recourse to the wildly improbable hypothesis of flocks of immature birds migrating over land and sea quite alone, and a week in advance of their parents or guides.

CHAPTER XXVIII

Darwin

(1809–1882)

[Charles Robert Darwin, the famous English naturalist, and in some ways the greatest Englishman of the nineteenth century, did much to mould the form of modern thought. Of his many works, the *Origin of Species* is the best known. "He had a marvellous faculty of observation, and collected masses of facts to an extent that is almost incredible." "His calm unbiased mind and his love of truth enabled him immediately to abandon his own hypotheses when they ceased to be supported by observation." The following extract is taken from his book on *Vegetable Mould and Earthworms* (pp. 19–26). It will be noticed that, in this case, observation is aided by simple experiment.]

The Sensitiveness of Worms to Light

Worms are destitute of eyes, and at first I thought that they were quite insensible to light; for those kept in confinement were repeatedly observed by the aid of a candle, and others out of doors by the aid of a lantern, yet they were rarely alarmed, although extremely timid animals. Other persons have found no difficulty in observing worms at night by the same means.

Hoffmeister, however, states that worms, with the exception of a few individuals, are extremely sensitive to light; but he admits that in most cases a certain time is requisite for its action. These statements led me to watch on many successive nights worms kept in pots, which were protected from currents of air by means of glass plates. The pots were approached very gently, in order that no vibration of the floor should be caused. When, in these circumstances, worms were illuminated by a bull's-eye lantern having slides of dark red and blue glass, which intercepted so much light that they could be seen only with some difficulty, they were not at all affected by this amount of light, however long they were exposed

to it. The light, as far as I could judge, was brighter than that from the full moon. Its colour apparently made no difference in the result. When they were illuminated by a candle, or even by a bright paraffin lamp, they were not usually affected at first. Nor were they when the light was alternately admitted and shut off. Sometimes, however, they behaved very differently, for as soon as the light fell on them, they withdrew into their burrows with almost instantaneous rapidity. This occurred perhaps once out of a dozen times. When they did not withdraw instantly, they often raised the anterior tapering ends of their bodies from the ground, as if their attention was aroused or as if surprise was felt; or they moved their bodies from side to side as if feeling for some object. They appeared distressed by the light; but I doubt whether this was really the case, for on two occasions after withdrawing slowly, they remained for a long time with their anterior extremities protruding a little from the mouths of their burrows, in which position they were ready for instant and complete withdrawal.

When the light from a candle was concentrated by means of a large lens on the anterior extremity, they generally withdrew instantly; but this concentrated light failed to act perhaps once out of half a dozen trials. The light was on one occasion concentrated on a worm lying beneath water in a saucer, and it instantly withdrew into its burrow. In all cases the duration of the light, unless extremely feeble, made a great difference in the result; for worms left exposed before a paraffin lamp or candle, invariably retreated into their burrows within from five to fifteen minutes; and if in the evening the pots were illuminated before the worms had come out of their burrows, they failed to appear.

From the foregoing facts it is evident that light affects worms by its intensity and by its duration. It is only the anterior extremity of the body, where the cerebral ganglia lie, which is affected by light, as Hoffmeister asserts, and as I observed on many occasions. If this part is shaded, other parts of the body may be fully illuminated, and no effect will be produced. As these animals have no eyes, we must suppose that the light passes through their skins, and in some manner excites their cerebral ganglia. It appeared at first probable that the different manner in which they were affected on different occasions might be explained, either by the degree of extension of their skin and its consequent transparency, or by some particular incidence of the light; but I could discover no such relation. One thing was manifest, namely, that when worms were em-

ployed in dragging leaves into their burrows or in eating them, and even during the short intervals whilst they rested from their work, they either did not perceive the light or were regardless of it; and this occurred even when the light was concentrated on them through a large lens.

When a worm is suddenly illuminated and dashes like a rabbit into its burrow—to use the expression employed by a friend—we are at first led to look at the action as a reflex one. The irritation of the cerebral ganglia appears to cause certain muscles to contract in an inevitable manner, independently of the will or consciousness of the animal, as if it were an automaton. But the different effect which a light produced on different occasions, and especially the fact that a worm when in any way employed and in the intervals of such employment, whatever set of muscles and ganglia may then have been brought into play, is often regardless of light, are opposed to the view of the sudden withdrawal being a simple reflex action. With the higher animals, when close attention to some object leads to the disregard of the impressions which other objects must be producing on them, we attribute this to their attention being then absorbed; and attention implies the presence of a mind. Every sportsman knows that he can approach animals whilst they are grazing, fighting, or courting, much more easily than at other times. The state, also, of the nervous system of the higher animals differs much at different times; for instance, a horse is much more readily startled at one time than another. The comparison here implied between the actions of one of the higher animals and of one so low in the scale as an earthworm, may appear far-fetched; for we thus attribute to the worm attention and some mental power, nevertheless I can see no reason to doubt the justice of the comparison.

Although worms cannot be said to possess the power of vision, their sensitiveness to light enables them to distinguish between day and night; and they thus escape extreme danger from the many diurnal animals which prey on them. Their withdrawal into their burrows during the day appears, however, to have become an habitual action; for worms kept in pots covered by glass plates, over which sheets of black paper were spread, and placed before a north-east window, remained during the daytime in their burrows and came out every night; and they continued thus to act for a week. No doubt a little light may have entered between the sheets of glass and the blackened paper; but we know from the trials with coloured glass, that worms are indifferent to a small amount of light.

Worms appear to be less sensitive to moderate radiant heat than to a bright light. I judge of this from having held at different times a poker heated to dull redness near some worms, at a distance which caused a very sensible degree of warmth in my hand. One of them took no notice; a second withdrew into its burrow, but not quickly; the third and fourth much more quickly, and the fifth as quickly as possible. The light from a candle, concentrated by a lens, and passing through a sheet of glass which would intercept most of the heat-rays, generally caused a much more rapid retreat than did the heated poker. Worms are sensitive to a low temperature, as may be inferred from their not coming out of their burrows during a frost.[1]

CHAPTER XXIX

Lord Avebury, F.R.S.

(1834–1913)

[Lord Avebury—the Sir John Lubbock of former years—was a well-known naturalist, banker, politician, and man of affairs. His numerous works include *British Wild Flowers*; *Ants, Bees, and Wasps*; and *The Senses, Instincts, and Intelligence of Animals*; all of which abound with suggestions for teachers of Nature Study. The following extract (*Ants, Bees, and Wasps*, pp. 176–81) is a good illustration of the method of the naturalist who calls in the aid of simple experiments. This chapter should be compared with the last.]

The Power of Communication amongst Ants

One rather cold day, when but few ants were out, I selected a specimen of *Atta testaceopilosa*, belonging to a nest which I had brought back with me from Algeria. She was out hunting about six feet from home, and I placed before her a large dead bluebottle fly, which she at once began to drag to the nest. I then pinned the fly to a piece of cork, in a small box, so that no ant could see the fly until she had climbed up the side of the box. The ant struggled, of course in vain, to move the fly. She pulled, first in one direction,

[1] Other interesting examples of Darwin's methods may be found on almost any page of any of his books. One or two may be mentioned: (1) Intelligence shown by worms in their manner of plugging their burrows (*Earthworms*, pp. 64–98); (2) The struggle of animals and plants for existence (*Origin of Species*, pp. 48–61); (3) Special expressions of animals (*Expression of the Emotions*, pp. 116–46); (4) Comparison of mental powers of man and the lower animals (*Descent of Man*, pp. 98–147).

and then in another, but, finding her efforts fruitless, she at length started off back to the nest empty-handed. At this time there were no ants coming out of the nest. Probably there were some few others out hunting, but for at least a quarter of an hour no ant had left the nest. My ant entered the nest, but did not remain there; in less than a minute she emerged[1] accompanied by seven friends. I never saw so many come out of that nest together before. In her excitement the first ant soon distanced her companions, who took the matter with much more *sang-froid*, and had all the appearance of having come out reluctantly, or as if they had been asleep and were only half awake. The first ant ran on ahead, going straight to the fly. The others followed slowly and with many meanderings; so slowly, indeed, that for twenty minutes the first ant was alone at the fly, trying in every way to move it. Finding this still impossible, she again returned to the nest, not chancing to meet any of her friends by the way. Again she emerged in less than a minute with eight friends, and hurried on to the fly. They were even less energetic than the first party; and when they found they had lost sight of their guide, they one and all returned to the nest. In the meantime several of the first detachment had found the fly, and one of them succeeded in detaching a leg, with which she returned in triumph to the nest, coming out again directly with four or five companions. These latter, with one exception, soon gave up the chase and returned to the nest. I do not think so much of this last case, because as the ant carried in a substantial piece of booty in the shape of the fly's leg, it is not surprising that her friends should some of them accompany her on her return; but surely the other two cases indicate a distinct power of communication.

Lest, however, it should be supposed that the result was accidental, I determined to try it again. Accordingly, on the following day I put another large dead fly before an ant belonging to the same nest, pinning it to a piece of cork as before. After trying in vain for ten minutes to move the fly, my ant started off home. At that time I could only see two other ants of that species outside the nest. Yet in a few seconds she emerged with no less than twelve friends. As in the previous case, she ran on ahead, and they followed very slowly and by no means directly, taking, in fact, nearly half an hour to reach the fly. The first ant, after vainly labouring for about a quarter of an hour to move the fly, started off

[1] Lord Avebury marked his ants, for purposes of identification, by means of a small dab of paint on the back (*op. cit.* p. 5).

again to the nest. Meeting one of her friends on the way, she conversed with her a little, then continued towards the nest, but, after going about a foot, changed her mind, and returned with her friend to the fly. After some minutes, during which two or three other ants came up, one of them detached a leg, which she carried off to the nest, coming out again almost immediately with six friends, one of whom, curiously enough, seemed to lead the way, tracing it, I presume, by scent. I then removed the pin, and they carried off the fly in triumph.

Again, on June 15, 1878, another ant belonging to the same nest had found a dead spider, about the same distance from the nest. I pinned down the spider as before. The ant did all in her power to move it; but after trying for twelve minutes, she went off to the nest. Although for a quarter of an hour no other ant had left the nest, yet in a few seconds she came out again with ten companions. As in the preceding case, they followed very leisurely. She ran on ahead and worked at the spider for ten minutes; when, as none of her friends had arrived to her assistance, though they were wandering about, evidently in search of something, she started back home again. In three-quarters of an hour after entering the nest she reappeared, this time with fifteen friends, who came on somewhat more rapidly than the preceding batch, though still but slowly. By degrees, however, they all came up, and after most persevering efforts carried off the spider piecemeal. On July 7, I tried the same experiment with a soldier of *Pheidole megacephala*. She pulled at the fly for no less than fifty minutes, after which she went to the nest and brought five friends exactly as the *Atta* had done.

On a subsequent day at three o'clock I again put a dead fly pinned on a bit of cork before a *Formica fusca*, who was out hunting. She tried in vain to carry it off, ran round and round, tugged in every direction, and at length at ten minutes to four she returned to the nest. Very soon after she reappeared, preceded by one and followed by two friends; these, however, failed to discover the fly, and, after wandering about a little, returned to the nest. She then set again to work alone, and in about forty minutes succeeded in cutting off the head of the fly, which she at once carried into the nest. In a little while she came out again, this time accompanied by five friends, all of whom found their way to the fly; one of these, having cut off the abdomen of the fly, took it into the nest, leaving three of her companions to bring in the remainder of their prey.

These experiments certainly seem to indicate the possession by

ants of something approaching to language. It is impossible to doubt that the friends were brought out by the first ant; and as she returned empty-handed to the nest, the others cannot have been induced to follow her merely by observing her proceedings. In face of such facts as these, it is impossible not to ask ourselves how far are ants mere exquisite automatons; how far are they conscious beings? When we see an ant-hill, tenanted by thousands of industrious inhabitants, excavating chambers, forming tunnels, making roads, guarding their home, gathering food, feeding the young, tending their domestic animals,—each one fulfilling its duties industriously, and without confusion,—it is difficult altogether to deny to them the gift of reason; and the preceding observations tend to confirm the opinion that their mental powers differ from those of men, not so much in kind as in degree.[1]

CHAPTER XXX

William Harvey

(1578–1657)

[We now pass to a subject where experiment plays a more important part.—William Harvey, after graduating at Cambridge, studied medicine at Padua. He took his M.D. at the age of twenty-four, and soon afterwards obtained the post of physician to St. Bartholomew's Hospital. Later on, he became physician to both James I and Charles I. In 1628 he published his famous treatise on the circulation of the blood,—*Exercitatio de motu Cordis et Sanguinis*. The method by which Harvey arrived at his complete and almost faultless solution of what is perhaps the most fundamental and difficult problem in Physiology, is deserving of the closest examination. We append two short extracts, but the whole volume, a cheap reprint of which can now be obtained, should be read.]

The Circulation of the Blood

From Chapter IX.—Let us assume either arbitrarily or from experiment, the quantity of blood which the left ventricle of the heart will contain when distended, to be, say two ounces, three ounces, one ounce and a half,—in the dead body I have found it to hold upwards of two ounces. Let us assume further, how much less

[1] The whole of the volume forms most instructive reading to the student of scientific method. The same remark applies to the other volumes mentioned at the head of the chapter.

the heart will hold in the contracted than in the dilated state; and how much blood it will project into the aorta upon each contraction;—and all the world allows that with the systole something is always projected, a necessary consequence obvious from the structure of the valves; and let us suppose as approaching the truth that the fourth, or fifth, or sixth, or even but the eighth part of its charge is thrown into the artery at each contraction; this would give either half an ounce, or three drachms, or one drachm of blood as propelled by the heart at each pulse into the aorta; which quantity, by reason of the valves at the root of the vessel, can by no means return into the ventricle. Now, in the course of half an hour, the heart will have made more than one thousand beats, in some as many as two, three, or even four thousand. Multiplying the number of drachms propelled by the number of pulses, we shall have either one thousand half-ounces, or one thousand times three drachms, or a like proportional quantity of blood, according to the amount which we assume as propelled with each stroke of the heart, sent from this organ into the artery; a larger quantity in every case than is contained in the whole body! In the same way, in the sheep or dog, say that but a single scruple of blood passes with each stroke of the heart, in one half-hour we should have one thousand scruples, or about three pounds and a half of blood injected into the aorta; but the body of neither animal contains above four pounds of blood, a fact which I have myself ascertained in the case of the sheep.

Upon this supposition, therefore, assumed merely as a ground for reasoning, we see the whole mass of blood passing through the heart.

From Chapter X.—If a live snake be laid open, the heart will be seen pulsating quietly, distinctly, for more than an hour, moving like a worm, contracting in its longitudinal dimensions, (for it is of an oblong shape,) and propelling its contents; becoming of a paler colour in the systole, of a deeper tint in the diastole; and almost all things else by which I have already said that the truth I contend for is established, only that here everything takes place more slowly, and is more distinct. This point in particular may be observed more clearly than the noon-day sun: the vena cava enters the heart at its lower part, the artery quits it at the superior part; the vein being now seized either with forceps or between the finger and thumb, and the course of the blood for some space below the heart interrupted, you will perceive the part that intervenes between the

fingers and the heart almost immediately to become empty, the blood being exhausted by the action of the heart; at the same time the heart will become of a much paler colour, even in its state of dilatation, than it was before; it is also smaller than at first, from wanting blood; and then it begins to beat more slowly, so that it seems at length as if it were about to die. But the impediment to the flow of blood being removed, instantly the colour and the size of the heart are restored.

If, on the contrary, the artery instead of the vein be compressed or tied, you will observe the part between the obstacle and the heart, and the heart itself, to become inordinately distended, to assume a deep purple or even livid colour, and at length to be so much oppressed with blood, that you will believe it about to be choked; but the obstacle removed, all things immediately return to their pristine state—the heart to its colour, size, stroke, &c.

Here then we have evidence of two kinds of death: extinction from deficiency, and suffocation from excess. Examples of both have now been set before you, and you have had opportunity of viewing the truth contended for with your own eyes in the heart.

[The experiments with ligatures in ch. xi, and the demonstration of the function of the valves in the veins in ch. xiii, are excellent examples of methodical investigation.]

CHAPTER XXXI

William Charles Wells

(1757–1818)

[Dr. Wells was a London physician to whom we are indebted for the true theory of Dew. In 1814 he published his admirable Essay on this subject. A long series of experiments, happily conceived and skilfully executed, enabled him to propound his theory, which has stood the test of all subsequent criticism. The Essay is a model of wise enquiry, lucid exposition, and scientific method. The success of the investigation was largely due to the very careful use of the Method of Agreement and the Method of Difference, combined with a well-thought-out logical plan of continually varying the circumstances. The Essay extends over 160 pp. A copy can occasionally be bought for three or four shillings.]

Wells' observations and experiments were made in a Surrey garden "not more than 1¼ miles from a densely built part of the

suburbs" of South London. The garden was level and half an acre in extent. "At one end was a dwelling house of moderate size, at the other a range of low buildings; on one side a row of high trees, on the other a low fence, dividing it from another garden." Within it were some small fruit trees. Towards one end there was a grass-plat, 62 feet by 16 feet, the herbage of which was kept short. The rest of the garden was given up to the growing of vegetables. "All these circumstances, however trifling they may appear, had influence on my experiments."

In one part of his investigation Wells used, for collecting the dew, little bundles of wool, which, when dry, weighed 10 grains each.

Influence of Situation on the Production of Dew

I now proceed to relate the influence which several *differences in the situation* have upon the production of dew.

One general fact relative to situation is, that whatever diminishes the view of the sky, as seen from the exposed body, occasions the quantity of dew, which is formed upon it, to be less than would have occurred if the exposure to the sky had been complete.

Experiment with elevated board.—I placed, on several clear and still nights, 10 grains of wool upon the middle of a painted board, 4½ feet long, 2 feet wide, and 1 inch thick, elevated 4 feet above the grass-plat, by means of four slender wooden props of equal height; and, at the same time I attached, loosely, 10 grains of wool to the middle of its under side. The two parcels were, consequently, only an inch asunder, and were equally exposed to the action of the air. Upon one night, however, I found, that the upper parcel had gained 14 grains in weight, but the lower only 4. On a second night the quantities of moisture, acquired by like parcels of wool, in the same situations as in the first experiment, were 19 and 6 grains; on a third, 11 and 2; on a fourth, 20 and 4; the smaller quantity being always that which was gained by the wool attached to the lower side of the board.

Experiment with bent pasteboard.—I bent a sheet of pasteboard into the shape of a house roof, making the angle of flexure 90 degrees, and leaving both ends open. This was placed one evening, with its ridge uppermost, upon the same grass plat, in the direction of the wind, as well as this could be ascertained. I then laid 10 grains of wool on the middle of that part of the grass, which was sheltered by the roof, and the same quantity on another part of the grass-

plat fully exposed to the sky. In the morning the sheltered wool was found to have increased in weight only 2 grains, but that which had been exposed to the sky, 16 grains.

In these experiments, the view of the sky was almost entirely cut off from the situations in which little dew was formed. In others, where it was less so, the quantity gained was greater. Thus, 10 grains of wool, placed upon the spot of the grass-plat which was directly under the middle of the raised board, and which enjoyed, therefore, a considerable oblique view of the sky, acquired during one night 7, during a second 9, and during a third 12 grains of moisture, while the quantities gained, during the same times, by equal parcels of wool, laid upon another part of the grass-plat, which was entirely exposed to the heavens, were 10, 16, and 20 grains.

As no moisture, falling like rain from the atmosphere, could, on a calm night, have reached the wool in any of the situations, where little dew was formed, it may be thought that the board and the pasteboard under which the wool was placed, prevented, mechanically, the access of that fluid. But on this supposition it cannot be explained why some dew was always found in the most sheltered places, and why a considerable quantity occurred upon the grass under the middle of the raised board. A still stronger proof of the want of justness in this supposition is afforded by the following experiment:—

Experiment with hollow cylinder.—I placed, upright, on the grass-plat, a hollow cylinder of baked clay, the height of which was $2\frac{1}{2}$ feet, and diameter 1 foot. On the grass, surrounded by the cylinder, were laid 10 grains of wool, which, in this situation, as there was not the least wind, would have received as much rain as a like quantity of wool fully exposed to the sky. But the quantity of moisture obtained by the wool surrounded by the cylinder, was only a little more than 2 grains, while that acquired by 10 grains of fully exposed wool was 16. This occurred on the night, during which the wool under the bent pasteboard gained only 2 grains of moisture.

Other varieties of situation.—Dew, however, will, in consequence of other varieties of situation, form in very different quantities, upon substances of the same kind, although these should be similarly exposed to the sky.

(1) In the first place; it is requisite, for the most abundant formation of dew, that the substance attracting it *should rest on a stable horizontal body of some extent.* Thus, upon one night, while

10 grains of wool, laid upon the raised board, increased 20 grains in weight, an equal quantity, suspended in the open air, 5½ feet above the ground, increased only 11 grains, notwithstanding that it presented a greater surface to the air than the other parcel. On another night, 10 grains of wool gained on the raised board 19 grains, but the same quantity suspended in the air, on a level with the board, only 13; and on a third, 10 grains of wool acquired, on the same board, 2½ grains of weight, during the time in which other 10 grains, hung in the air, at the same height, acquired only half a grain.

(2) In the second place; the quantities of dew attracted by equal masses of wool, similarly exposed to the sky, and resting on equally stable and extended bodies, oftentimes vary considerably, in consequence of *some difference in the other circumstances of these bodies.* Ten grains of wool, for instance, having been placed (*a*) *on the grass-plat,* on a dewy evening; 10 grains upon (*b*) *the gravel walk* which bounded the grass-plat; and 10 grains upon (*c*) *a bed of bare garden mould,* immediately adjoining the gravel walk; in the morning the wool on the grass was found to have increased 16 grains in weight, but that on the gravel walk only 9, and that on the garden mould only 8. On another night, during the time that 10 grains of wool laid upon grass, acquired 2½ grains of moisture, the same quantity gained only half a grain upon the bed of garden mould, and a like quantity, placed upon the gravel walk, received no accession of weight whatever.

Two objections will probably be made against the accuracy of these, as well as my other experiments with wool. (*a*) One is that wool placed on grass may, by a kind of capillary attraction, receive dew previously formed on the grass, in addition to its own. To this I answer, that wool in a china saucer, placed on the grass, acquired very nearly as much weight, as an equal parcel immediately touching the grass. (*b*) The second objection is, that a part of the increased weight in the wool might arise from its imbibing moisture, as a hygroscopic substance. I do not deny that some weight was given to the wool in this way; but it may be safely affirmed that this quantity must have been very small. For, on very cloudy nights, apparently best fitted to increase the weight of hygroscopic substances, wool upon the raised board would, in the course of many hours, acquire little or no weight; and in London I have never found 10 grains of wool, exposed to the air on the outside of one of my chamber windows, to increase, during

a whole night, more than half a grain in weight. When this weight was gained, the weather was clear and still; if the weather was cloudy and windy, the wool received either less or no weight. This window is so situated as to be, in great measure, deprived of the aspect of the sky.

It being shown that wool, though highly attractive of dew, was prevented, by the mere vicinity of a gravel walk, or a bed of garden mould, for only a small part of it actually touched those bodies, from acquiring nearly as much dew, as an equal parcel laid upon grass, it may be readily inferred, that little was formed upon themselves. In confirmation of this conclusion, I shall mention, that I never saw dew on either of them. Another fact of the same kind is that, while returning to London from the scene of my experiments about sunrise, I never observed, if the atmosphere was clear, the public road, or any stone pavement on the side of it, to be moistened with dew, though grass within a few feet of it, and painted doors and windows of houses not far from it, were frequently very wet. If, indeed, there was a foggy morning, after a clear and calm night, even the streets of London would sometimes be moist, though they had been dry the day before, and no rain had in the meanwhile fallen. This entire, or almost entire, freedom of certain situations from dew depends, however, much more upon extraneous circumstances, than upon the nature of the substances found there; for river-sand, though of the same nature as gravel, when placed upon the raised board, or upon grass, attracted dew copiously.

(3) A third difference, from situation, in the quantity of dew collected by similar bodies, similarly exposed to the sky, depends upon *their position with respect to the ground.* Thus, a substance placed several feet above the ground, though in this situation later dewed, than if it touched the earth, would, notwithstanding, if it lay upon a stable body of some extent, such as the raised board lately mentioned, acquire more dew during a very still night, than a similar substance lying on grass.

(4) A fourth difference of this kind occurred among bodies placed *on different parts of the same board.* For one, that was placed at the leeward end of it, generally acquired more dew than a similar body at the windward extremity.[1]

Space cannot be spared for further extracts, but the reader is

[1] Edition of 1818; pp. 135-44.

strongly urged to go through the whole Essay. Wells was never tired of *varying the circumstances* in every possible way. Consider, for instance, his experiments with metals, in connection with the formation of dew. He varied the kind of metal, the size of the metal, the thickness of the metal, and the position of the metal with respect to the ground; he exposed the metal alone, and the metal closely attached to some other substance; he exposed the metal dry, and the metal purposely first moistened; he removed the metal from place to place during the night; and so on, seemingly almost indefinitely. *He varied one circumstance at a time*; the detection of a *difference* enabled him to form a hypothesis, which he then promptly checked by means of further experiment.

CHAPTER XXXII

Joseph Black

(1728–1799)

[We now give a few instances of experimental investigations by famous chemists. It is a little difficult to make a choice from the large number of eminent men whose names appear in every textbook dealing with the history of Chemistry; but Black, Priestley, Gay-Lussac, and Davy may certainly be regarded as worthy representatives of the others.

Black was Professor of Chemistry in the University of Edinburgh from 1766 to 1797. He graduated at that University in 1754, and immediately afterwards revealed himself as a great scientific discoverer. At that time the causticity of the alkalis was attributed to their *absorbing* an imaginary fire-essence known as *phlogiston*, an hypothesis which Black overthrew. Black showed, for example, that the causticity acquired by "crude lime" on ignition is due to the *expulsion* of a ponderable gas, *carbonic acid*, which he named "fixed air", meaning that it was found not only as a separate fluid, but as *fixed* in solid bodies. The discovery was embodied in a Paper, which Black read in 1755, "Experiments upon Magnesia Alba, Quicklime, and other Alkaline Substances". There are very few finer examples of inductive investigation in the whole range of Science. The following extract is taken, with slight omissions and modifications, from Part II of the Paper.][1]

Fixed Air in Lime and in Alkalis

It is sufficiently clear [from previous experiments] that the calcareous earths in their native state, and that the alkalis and mag-

[1] The whole paper is reproduced in the *Alembic Club Reprints*, which the reader should consult.

nesia[1] in their ordinary condition, contain a large quantity of fixed air,[2] and this air certainly adheres to them with considerable force, since a strong fire is necessary to separate it from magnesia, and the strongest is not sufficient to expel it entirely from fixed alkalis,[3] or take away their power of effervescing with acid salts.

Hypothesis.—These considerations led me to conclude that the relations between fixed air and alkaline substances, were somewhat similar to the relation between these and acids; that as the calcareous earths and alkalis attract acids strongly and can be saturated with them, so they also attract fixed air, and are in their ordinary state saturated with it: and when we mix an acid with an alkali or with an absorbent earth, that the air is then set at liberty, and breaks out with violence; because the alkaline body attracts it more weakly than it does the acid, and because the acid and air cannot both be joined to the same body at the same time.

Further Hypothesis.—I also imagined that, when the calcareous earths are exposed to the action of a violent fire, and are thereby converted into quicklime, they suffer no other change in their composition than the loss of a small quantity of water and of their fixed air. The remarkable acrimony which we perceive in them after this process, was not supposed to proceed from any *additional* matter received from the fire, but seemed to be an essential property of the pure earth, depending on an attraction for those several substances which it then became capable of corroding or dissolving, which attraction had been insensible as long as the air adhered to the earth, but discovered itself upon the separation.

This supposition was founded upon an observation of the most frequent consequences of combining bodies in chemistry. Commonly, when we join two bodies together, their acrimony or attraction for other substances becomes immediately either less perceivable or entirely insensible; although it was sufficiently strong and remarkable before their union, and may be rendered evident again by disjoining them. A neutral salt, which is composed of an acid and alkali, does not possess the acrimony of either of its constituent parts. It can easily be separated from water, has little or no effect upon metals, is incapable of being joined to inflammable bodies, and of corroding or dissolving animals and vegetables; so that the attraction both of the acid and alkali for these several substances seems to be suspended till they are again separated from one another.

Crude lime was therefore considered as a peculiar acrid earth

[1] That is, *magnesia alba.* [2] That is, *carbon dioxide.* [3] That is, *potash* and *soda.*

rendered mild by its union with fixed air; and quicklime as the same earth, in which, by having separated the air, we discover that acrimony or attraction for water, for animal, vegetable, and for inflammable substances.

The General Theory Considered.—According to our theory, the relation of the calcareous earth to air and water appeared to agree with the relation of the same earth to vitriolic and vegetable acids. As chalk, for instance, has a stronger attraction for the vitriolic than for the vegetable acid, and is dissolved with more difficulty when combined with the first than when joined to the second: so it also attracts air more strongly than water, and is dissolved with more difficulty when saturated with air than when compounded with water only.

A calcareous earth deprived of its air, or in the state of quicklime, greedily absorbs a considerable quantity of water, becomes soluble in that fluid, and is then said to be slaked; but as soon as it meets with fixed air, it is supposed to quit the water and join itself to the air, for which it has a superior attraction, and is therefore restored to its first state of mildness and insolubility in water.

When slaked lime is mixed with water, the fixed air in the water is attracted by the lime, and saturates a small portion of it, which then becomes again incapable of dissolution, but part of the remaining slaked lime is dissolved and composes lime water.

If this fluid be exposed to the open air, the particles of quicklime which are nearest the surface gradually attract the particles of fixed air which float in the atmosphere. But at the same time that a particle of lime is thus saturated with air, it is also restored to its native state of mildness and insolubility; and as the whole of this change must happen at the surface, the whole of the lime is successively collected there under its original form of an insipid calcareous earth, called the cream or crusts of lime water.

When quicklime itself is exposed to the open air, it absorbs the particles of water and of fixed air which come within its sphere of attraction, as it meets with the first of these in greatest plenty, the greatest part of it assumes the form of slaked lime; the rest is restored to its original state; and if it be exposed for a sufficient length of time, the whole of it is gradually saturated with air, to which the water as gradually yields its place.

If quicklime be mixed with a dissolved alkali, it shows an attraction for fixed air superior to that of the alkali. It robs the salt of its air, and thereby becomes mild itself, while the alkali is conse-

quently rendered more corrosive, or discovers its natural degree of acrimony or strong attraction for water, and for bodies of the inflammable, and of the animal and vegetable kind; which attraction was less perceivable as long as it was saturated with air. And the volatile alkali when deprived of its air, besides this attraction for various bodies, discovers likewise its natural degree of volatility, which was formerly somewhat repressed by the air adhering to it, in the same manner as it is repressed by the addition of an acid.

Consequences of the Theory.—This account of lime and alkalis recommended itself by its simplicity, and by affording an easy solution of many phenomena, but appeared upon a nearer view to be attended with consequences that were so very new and extraordinary, as to render suspicious the principles from which they were drawn.

I resolved, however, to examine, in a particular manner, such of these consequences as were the most unavoidable, and found the greatest number of them might be reduced to the following propositions:—

Proposition I.—If we only separate a quantity of air from lime and alkalis, when we render them caustic they will be found to lose part of their weight in the operation, but will saturate the same quantity of acid as before, and the saturation will be performed without effervescence.

Proposition II.—If quicklime be no other than a calcareous earth deprived of its air, and whose attraction for fixed air is stronger than that of alkalis, it follows that, by adding to it a sufficient quantity of alkali saturated with air, the lime will recover the whole of its air, and be entirely restored to its original weight and condition; and it also follows, that the earth separated from lime water by an alkali, is the lime which was dissolved in the water now restored to its original mild and insoluble state.[1]

These are necessary conclusions from the above suppositions. I determined to inquire into the truth of them by way of experiment. I therefore engaged myself in a set of trials; the history of which is here subjoined.

The Consequences Proved to be Consonant with the Theory.—Desiring to know how much of an acid a calcareous earth will absorb, and what quantity of air is expelled during the dissolution, I saturated 120 grains of chalk with diluted spirit of salt; 421 grains of the acid finished the dissolution, and the chalk lost 48 grains of air.

This experiment was necessary before the following, by which

[1] Three other Propositions are also stated, making five in all.

I proposed to inquire into the truth of the first proposition, so far as it relates to quicklime.

120 grains of chalk were converted into a perfect quicklime, and lost 52 grains in the fire. This quicklime was slaked, or reduced to a perfect liquor with an ounce of water, and then dissolved in the same manner and with the same acid as the 120 grains of chalk in the preceding experiment; 414 grains of the acid finished the saturation without any sensible effervescence or loss of weight.

It therefore appears from these experiments that no air is separated from quicklime by an acid, and that chalk saturates nearly the same quantity of acid after it is converted into quicklime as before.

With respect to the second proposition, I tried the following experiments:—

A piece of perfect quicklime made from 120 grains of chalk, and which weighed 68 grains, was reduced to a very fine powder, and thrown into a filtrated mixture of an ounce of a fixed alkaline salt and two ounces of water. After a slight digestion, the powder being well washed and dried, weighed 118 grains. It was similar in every trial to a fine powder of ordinary chalk, and was therefore saturated with air which must have been furnished by the alkali.

60 grains of pure salt of tartar[1] was dissolved in 14 pounds of lime water, and the powder thereby precipitated, being carefully collected and dried, weighed 150 grains. When exposed to a violent fire, it was converted into a true quicklime, and had every other quality of a calcareous earth.

This experiment was repeated with the volatile alkali, and also with the fossil or alkali of sea-salt, and exactly with the same event.[2]

[1] That is, *potassium carbonate*.

[2] The remaining three Propositions are similarly dealt with, and the investigation continues. But the logical finality of Black's conclusions can hardly be realized unless the whole paper is read.

CHAPTER XXXIII

Joseph Priestley

(1733–1804)

[Joseph Priestley, though not a professional man of science, devoted a good deal of attention to Chemistry, and contributed greatly to our knowledge of gases. In 1774 he discovered oxygen. But his researches on *Different Kinds of Air* are usually considered to be more remarkable for the impulse they gave to controversy and experiment than for their concrete results. Whatever success he gained was probably due almost as much to good luck as to good method, for he held that all discoveries are made by chance. He possessed great insight, but apparently he failed to see the need of constant and rigorous verification. Yet his exceedingly important results went far to build up Chemistry into a Science. At Leeds he lived next door to a brewery, and amused himself with experiments on the "fixed air" (CO_2) produced there. Thus his researches began. His works are well worth reading if only because of the *naïveté* with which he tells the whole story of his experiments. The following extract gives a good general idea of his method.]

Fixed Air

It was in consequence of living for some time in the neighbourhood of a brewery, that I was induced to make experiments on fixed air, of which there is always a large body, ready formed, upon the surface of the fermenting liquor, generally about 9 inches or a foot in depth, within which any kind of substance may be very conveniently placed; and though, in these circumstances, the fixed air must be continually mixing with the common air, and is therefore far from being perfectly pure, yet there is a constant fresh supply from the fermenting liquor, and it is pure enough for many purposes.

A person, who is quite a stranger to the properties of this kind of air, would be agreeably amused with extinguishing lighted candles, or chips of wood in it, as it lies upon the surface of the fermenting liquor; for the smoke readily unites with this kind of air, probably by means of the water which it contains; so that very little or none of the smoke will escape in the open air, which is incumbent upon it. It is remarkable that the upper surface of this smoke, floating in the fixed air, is smooth and well defined; whereas the lower surface is exceedingly ragged, several parts hanging down to a considerable distance within the body of the fixed air, and

sometimes in the form of balls, connected to the upper stratum by slender threads, as if they were suspended.

Making an agitation in this air, the surface of it is thrown into the form of waves; and if by this agitation any of the fixed air be thrown over the side of the vessel, the smoke which is mixed with it will fall to the ground, as if it were so much water, the fixed air being heavier than common air.

The red part of burning wood was extinguished in this air, but I could not perceive that a red-hot poker was sooner cooled in it.

Fixed air does not instantly mix with common air. Indeed, if it did, it could not be caught upon the surface of the fermenting liquor. A candle put under a large receiver, and immediately plunged very deep below the surface of the fixed air, will burn some time. But vessels with the smallest orifices, hanging with their mouths downwards in the fixed air, will, *in time*, have the common air, which they contain, perfectly mixed with it.

Considering the near affinity between water and fixed air, I concluded that if a quantity of water was placed near the yeast of the fermenting liquor, it could not fail to imbibe that air, and thereby acquire the principal properties of Pyrmont, and some other medicinal mineral waters. Accordingly I found that when the surface of the water was considerable, it always acquired the pleasant acidulous taste that Pyrmont water has. The readiest way of impregnating water with this virtue, in these circumstances, is to take two vessels, and to keep pouring the water from one into the other when they are both of them held as near the yeast as possible. In this manner I have sometimes, in the space of two or three minutes, made a glass of exceedingly pleasant sparkling water, which could hardly be distinguished from very good Pyrmont, or rather Seltzer water.

But the *most effectual* way of impregnating water with fixed air is to put the vessels which contain the water into glass jars, filled with the purest fixed air, made by the solution of chalk in diluted oil of vitriol, standing in quicksilver. In this manner, I have in about two days made a quantity of water to imbibe more than an equal bulk of fixed air, so that it must have been stronger than the best imported Pyrmont. If a sufficient quantity of quicksilver cannot be procured, *oil* may be used with sufficient advantage, for this purpose, as it imbibes the fixed air very slowly.

The *readiest* method of preparing this water for use is to agitate it strongly with a large surface exposed to the fixed air. By this

means, more than an equal bulk of air may be communicated to a large quantity of water in the space of a few minutes.

Water thus impregnated with fixed air readily dissolves iron; so that if a quantity of iron filings be put in it, it presently becomes a strong chalybeate, and of the mildest and most agreeable kind.

I have recommended the use of *chalk* and *oil of vitriol* as the cheapest, and, upon the whole, the best materials for the purpose.

Whereas some persons had suspected that a quantity of the oil of vitriol was rendered volatile by this process, I examined it, by all the chemical methods that are in use; but could not find that water thus impregnated contained the least perceivable quantity of that acid.

The heat of boiling water will expel all the fixed air, if a phial containing the impregnated water be held in it; but it will often require above half an hour to do it completely.

Having succeeded so well with artificial Pyrmont water, I imagined that it might be possible to give *ice* the same virtue, especially as cold is known to promote the absorption of fixed air by water; but in this I found myself quite mistaken. I put several pieces of ice into a quantity of fixed air, confined by quicksilver, but no part of it was absorbed in two days and two nights.

I then took a quantity of strong artificial Pyrmont water, and putting it into a thin glass phial, I set it in a pot that was filled with snow and salt. This mixture instantly freezing the water that was contiguous to the sides of the glass, the air was discharged plentifully, so that I caught a considerable quantity in a bladder tied to the mouth of the phial.

The pressure of the atmosphere assists very considerably in keeping fixed air confined in water; for in an exhausted receiver, Pyrmont water will absolutely boil, by the copious discharge of its air. This is also the reason why beer and ale froth so much in *vacuo*. I do not doubt, therefore, that by the help of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring.

Insects and animals which breathe very little are stifled in fixed air, but are not soon quite killed in it. Flies and butterflies will generally become torpid, and seemingly dead, after being held a few minutes over the fermenting liquor; but they revive again after being brought into the fresh air. But there are very great varieties with respect to the time in which different kinds of flies will either become torpid in the fixed air, or die in it. A large, strong frog

was much swollen, and seemed to be nearly dead, after being held about six minutes over the fermenting liquor; but it recovered upon being brought into the common air. A snail treated in the same manner died presently.

Fixed air is presently fatal to vegetable life. At least sprigs of mint growing in water, and placed over the fermenting liquor, will often become quite dead in one day, or even in a less space of time; nor do they recover when they are afterwards brought into the common air. I am told, however, that some other plants are much more hardy in this respect.[1]

CHAPTER XXXIV

Joseph Louis Gay-Lussac

(1778–1850)

[Gay-Lussac was one of the most distinguished of French physicists and chemists. From 1808 to 1832 he was Professor of Physics at the Sorbonne, and afterwards Professor of Chemistry at the Jardin des Plantes. His work was remarkable not only for its range, but for its intrinsic worth, its accuracy of detail, its experimental ingenuity, its descriptive clearness, and the soundness of its inferences. His name is closely associated with the law of gaseous volumes, with the law of variation of gaseous volume with temperature, with researches on iodine and cyanogen, and with many analytical methods (for instance, the method of titration). The following extract[2] is from a Paper he read before the Philomathic Society in 1808.]

On the Combination of Gaseous Substances with each other

Substances, whether in the solid, liquid, or gaseous state, possess properties which are independent of the force of cohesion; but they also possess others which appear to be modified by this force, and which no longer follow any regular law. The same pressure applied to all solid or liquid substances would produce a diminution of

[1] From *Experiments and Observations on Different Kinds of Air* (1774), pp 25–43. Priestley's paper on the discovery of Oxygen is included in the *Alembic Club Reprints* (No. 7); it should be read in conjunction with Scheele's paper on the same subject (*Reprint* No. 8).

Priestley's account of the apparatus he devised and used will be found in the volume above mentioned (pp. 6–22). A copy of this volume may sometimes be discovered in a second-hand book-shop. The old plates are interesting.

[2] See *Alembic Club Reprint* No. 4, and *Mémoires de la Société d'Arcueil*, ii (1809), pp. 207–34.

volume differing in each case, while it would be equal for all elastic fluids. Similarly, heat expands all substances; but the dilatations of liquids and solids have hitherto presented no regularity, and it is only those of elastic fluids which are equal and independent of the nature of each gas. The attraction of the molecules in solids and liquids is, therefore, the cause which modifies their special properties; and it appears that it is only when the attraction is entirely destroyed, as in gases, that bodies under similar conditions obey simple and regular laws. At least it is my intention to make known some new properties in gases, the effects of which are regular, by showing that these substances combine amongst themselves in very simple proportions.

It is a very important question in itself, and one much discussed amongst chemists, to ascertain if compounds are formed in all sorts of proportions. M. Proust, who appears first to have fixed his attention on this subject, is of opinion that the metals are susceptible of only two degrees of oxidation, a *minimum* and a *maximum*; but led away by this seductive theory, he has seen himself forced to entertain principles contrary to physics in order to reduce to two oxides all those which the same metal sometimes presents. M. Berthollet thinks, on the other hand, that compounds are always formed in very variable proportions, unless they are determined by special causes, such as crystallization, insolubility, or elasticity. Lastly, Dalton has advanced the idea that compounds of two bodies are formed in such a way that one atom of the one unites with one, two, three, or more atoms of the other. It would follow from this mode of looking at compounds that they are formed in constant proportions, the existence of intermediate bodies being excluded[1], and in this respect Dalton's theory would resemble that of M. Proust; but M. Berthollet has already strongly opposed it, and we shall see that in reality it is not entirely exact. Such is the state of the question now under discussion; it is still very far from receiving its solution, but I hope that the facts which I now proceed to set forth, facts which had entirely escaped the notice of chemists, will contribute to its elucidation.

Suspecting from the exact ratio of 100 of oxygen to 200 of hydrogen, which M. Humboldt and I had determined for the proportions of water, that other gases might also combine in simple ratios, I have made the following experiments.—I prepared *fluoboric*[1], *muriatic*, and *carbonic* gases, and made them combine successively

[1] "Obtained by distilling pure fluoride of lime with vitreous boracic acid."

with ammonia gas. (1) 100 parts of *muriatic gas* saturate precisely 100 parts of ammonia gas, and the salt which is formed from them is perfectly neutral, whether one or other gases is in excess. (2) *Fluoboric gas*, on the contrary, unites in two proportions with ammonia gas. When the acid[1] gas is put first into the graduated tube, and the other gas is then passed in, it is found that equal volumes of the two condense, and that the salt formed is neutral. But if we begin by first putting the ammonia gas into the tube, and then admitting the fluoboric gas in single bubbles, the first gas will then be in excess with regard to the second, and there will result a salt with excess of base, composed of 100 of fluoboric gas and 200 of ammonia gas. (3) If *carbonic gas* is brought into contact with ammonia gas, by passing it sometimes first, sometimes second into the tube, there is always formed a sub-carbonate composed of 100 parts of carbonic gas and 200 of ammonia gas. It may, however, be proved that neutral carbonate of ammonia would be composed of equal volumes of each of these components. M. Berthollet, who has analysed this salt, obtained by passing carbonic gas into the sub-carbonate, found that it was composed of 73·34 parts by weight of carbonic gas and 26·66 of ammonia gas. Now if we suppose it to be composed of equal volumes of its components, we find from their known specific gravity, that it contains by weight 71·81 per cent of carbonic acid and 28·19 per cent of ammonia, a proportion differing only slightly from the preceding.

If the neutral carbonate of ammonia could be formed by the mixture of carbonic gas and ammonia gas, as much of one gas as of the other would be absorbed; and since we can only obtain it through the intervention of water, we must conclude that it is the affinity of this liquid which competes with that of the ammonia to overcome the elasticity of the carbonic acid, and that the neutral carbonate of ammonia can only exist through the medium of water.

Thus we may conclude that muriatic, fluoboric, and carbonic acids take exactly their own volumes of ammonia gas to form neutral salts, and that the last two take twice as much to form *sub-salts*. It is very remarkable to see acids so different from one another neutralize a volume of ammonia gas equal to their own; and from this we may suspect that if all acids and all alkalis could be obtained in the gaseous state, neutrality would result from the combination of equal volumes of acid and alkali.

It is not less remarkable that whether we obtain a neutral salt

[1] "Alkaline" in the original.

or a *sub-salt*, their elements combine in simple ratios which may be considered as limits to their proportions. Accordingly, if we accept the specific gravity of muriatic acid determined by M. Biot[1] and myself, and those of carbonic gas and ammonia given by MM. Biot and Arago, we find that dry muriate of ammonia is composed of 100 parts of ammonia to 160·7 parts of muriatic acid (i.e. 38·35 per cent to 61·65 per cent), a proportion very far from that of M. Berthollet, viz., 100 of ammonia to 213 of acid.

In the same way, we find that sub-carbonate of ammonia contains 100 parts of ammonia to 127·3 of carbonic acid (i.e. 43·98 per cent to 56·02 per cent); and the neutral carbonate 100 parts of ammonia to 254·6 of carbonic acid (i.e. 28·19 per cent to 71·81 per cent).

It is easy from the preceding results to ascertain the ratios of the capacity of fluoboric, muriatic, and carbonic acids; for since these three gases saturate the same volume of ammonia gas, their relative capacities will be inversely as their densities, allowance having been made for the water contained in muriatic acid.

We might even now conclude that gases combine with each other in very simple ratios; but I shall still give some fresh proofs.

According to the experiments of Berthollet, *ammonia* is composed of 100 parts of nitrogen to 300 of hydrogen (by volume).

I have found that *sulphuric acid* is composed of 100 parts of sulphurous gas to 50 of oxygen gas.

When a mixture of 50 parts of oxygen and 100 of carbonic oxide is inflamed, these two gases are destroyed and their place taken by 100 parts of carbonic acid gas. Consequently, *carbonic acid* may be considered as being composed of 100 of carbonic oxide gas to 50 of oxygen gas.

Davy, from the analysis of various *compounds of nitrogen with oxygen*, has found the following proportions by weight:—

Nitrous oxide	...	63·30	of nitrogen to	36·70	of oxygen
Nitrous gas	...	44·05	„	55·95	„
Nitric acid	...	29·50	„	70·50	„

Reducing these proportions to volumes we find, respectively, 100 to 49·5, 100 to 108·9, and 100 to 204·7. The first and last of these proportions differ only slightly from 100 to 50 and 100 to 200; it is only the second which diverges somewhat from 100 to 100. The difference, however, is not very great, and is such as we might

[1] "As muriatic acid contains ¼ its weight of water, we must only take ¾ of the density for that of real muriatic acid."

expect in experiments of this sort; and I have assured myself that it is actually *nil*. On burning the new combustible substance from potash in 100 parts by volume of nitrous gas, there remained over exactly 50 parts of nitrogen, the weight of which, deducted from that of nitrous gas, yields as result that this gas is composed of equal parts by volume of nitrogen and oxygen.

We may then admit the following numbers for the proportions by volume of the compounds of nitrogen and oxygen: nitrous oxide, 100 of nitrogen to 50 of oxygen; nitrous gas, 100 to 100; and nitric acid, 100 to 200.

From my experiments, *oxygenated muriatic acid*[1] is composed of 22·92 parts of oxygen to 77·08 of muriatic acid (by weight). Converting these quantities into volumes, we find that oxygenated muriatic acid is formed of 300 of muriatic acid to 103·2 of oxygen, a proportion very nearly 300 to 100.[2]

Thus it appears evident to me that gases always combine in the simplest proportions when they act on one another; and we have seen in reality in all the preceding examples that the ratio of combination is 1 to 1, 1 to 2, or 1 to 3. It is very important to observe that in considering weights there is no simple and finite relation between the elements of any one compound; it is only when there is a second compound between the same elements that the new proportion of the element that has been added is a multiple of the first quantity. Gases, on the contrary, in whatever proportions they may combine, always give rise to compounds whose elements by volume are multiples of each other.[3]

[1] Chlorine.

[2] "In the proportion by weight of oxygenated muriatic acid, the muriatic acid is supposed to be free from water, whilst in the proportion by volume, it is supposed to be combined with ¼ of its weight of water, which I have proved to be absolutely necessary for its existence in the gaseous state." With this statement of Gay-Lussac's, compare the next chapter.

[3] Gay-Lussac now proceeds to deal with "the apparent contraction of volume which gases experience on combination", and to show that this follows a general law. The reader should refer to the paper. *Alembic Club Reprint* No 4 contains, in addition to this paper, extracts from papers by Dalton and Avogadro, also dealing with the molecular theory

CHAPTER XXXV

Sir Humphry Davy

1778–1829)

[The name of Davy is popularly associated with the safety-lamp, but his fame is mainly due to his discoveries in electro-chemistry. He was appointed Professor of Chemistry at the Royal Institution in 1802, and the liberality of the Committee of that Institution supplied Davy with two enormously powerful batteries, with the help of which he conducted the brilliant investigations which resulted in the discovery of potassium and sodium. In a lecture given in 1809, he brought forward proofs that oxymuriatic acid is a simple body, termed by him chlorine, and that muriatic acid is a compound of that element with hydrogen. The famous chlorine controversy raged during the early years of last century, and the extracts below are from some of Davy's papers bearing on the subject.

As an experimenter, Davy was remarkably quick and resourceful. He was a complete master of scientific method. Before he was twenty-one he wrote, "It is only by forming theories, and then comparing them with facts, that we can hope to discover the true system of nature".]

Is there Oxygen in Oxymuriatic Acid?

I have made many new experiments with the hope of decomposing chlorine, but they have been all unavailing; nor have I been able to gain the slightest evidence of the existence of that oxygen which many persons still assert to be one of its elements.

I kept sulphuret of lead for some time in fusion in chlorine, the results were sulphurane (Dr. Thomson's liquor) and plumbane (muriate of lead); not an atom of sulphate of lead was formed in the experiment, though if any oxygen had been present, this substance might have been expected to have been produced.

I heated plumbane (muriate of lead) in sulphurous acid gas, and likewise in carbonic acid gas, but no change was produced; now, if oxygen had existed either in chlorine, or in its combination with lead, there is every reason to believe that the attractions of the substances concerned in these experiments would have been such as to have produced the insoluble and fixed salts of lead, the sulphate in the first case, and the carbonate in the second.

I shall not enter into any discussion upon the experiments in which water is said to be produced by the action of muriatic gas on ammonia: there is, I believe, no enlightened and candid person who has witnessed the results of processes in which large quantities of

muriate of ammonia, made by the combination of the gases in close vessels, have been distilled, without being satisfied that there is no more moisture present than the minute quantity which is known to exist in the compound vapours diffused through ammoniacal and muriatic acid gases, which cannot be considered either as essential to the existence of the gases, or as chemically combined with them.

One of the first experiments that I made, with the hope of detecting oxygen in chlorine, was by acting upon it by ammonia, when I found that no water was formed, and that the results were merely muriate of ammonia and azote; and the driest muriate of ammonia, I find, when heated with potassium, converts it into muriate of potassa, which result would be impossible on the hypothesis of oxymuriatic gas being a compound of oxygen, for, if there was a separation of water during the formation of the muriate, the same oxygen could not be supposed to be detached in water, and yet likewise to remain so as to form part of a neutral salt.

If water had been really formed during the action of chlorine on ammonia, the result would have been a most important one; it would have proved either that chlorine or azote was a compound, and contained oxygen, or that both contained this substance; but it would not have proved the existence of oxygen in chlorine till it had been shown that the azote of the ammonia was unchanged in the operation.

Some authors continue to write and speak with scepticism on the subject, and demand stronger evidence of chlorine being undecompounded. These evidences it is impossible to give. It has resisted all attempts at decomposition. In this respect it agrees with gold, silver, hydrogen, and oxygen.

By the same mode of reasoning as that in which oxygen is conceived to exist in chlorine, any other species of matter might be supposed to form one of its constituent parts; and by multiplying words all the phenomena might be satisfactorily explained. Thus in the simple view of the formation of muriatic acid, it is said one volume of chlorine combines with one of hydrogen, and they form two volumes of muriatic acid gas. In the hypothesis of chlorine containing oxygen, it is said, the oxygen of the chlorine combines with the hydrogen to form water, and this water unites to an unknown something, or dry muriatic acid, to produce a gaseous body. If it were asserted that chlorine contained azote, oxygen, and this unknown body, then it might be said that in the action of hydrogen on chlorine, the azote, the oxygen, and the chlorine, having all attrac-

tions for hydrogen, enter into union with it, and form a quadruple compound.

Berzelius has lately adduced some arguments, which he conceives are in favour of chlorine being a compound of oxygen from the laws of definite proportions; but I cannot regard these arguments of my learned and ingenious friend as possessing any weight. By transferring the definite proportions of oxygen to the metals, which he has given to chlorine, the explanation becomes a simple expression of facts; and there is no general canon with respect to the multiples of the proportions in which different bodies combine. Thus, azote follows peculiar laws in combining with every different body; it combines with three volumes of hydrogen, with half a volume of oxygen, with one, two, and one and a half of the same body, and with four volumes of chlorine.

The chemists in the middle of the last century had an idea that all inflammable bodies contained phlogiston or hydrogen. It was the glory of Lavoisier to lay the foundations for a sound logic in chemistry, by showing that the existence of this principle, or of other principles, should not be assumed where they could not be detected.

In all cases, in which bodies support combustion or form acids, oxygen has been supposed by the greater number of modern chemists to be present; but as there are many distinct species of inflammable bodies, so there may be many distinct species of matter which combine with them with so much energy as to produce heat and light; and various bodies appear capable of forming acids; thus hydrogen enters into the composition of nearly as many acids as oxygen, and three bodies, namely, sulphuretted hydrogen, muriatic acid, and fluoric acid, which contain hydrogen, are not known to contain oxygen. The existence of oxygen in the atmosphere, and its action in the economy of nature, and in the processes of the arts, have necessarily caused it to occupy a great portion of the attention of chemists, and, being of such importance, and in constant operation, it is not extraordinary that a greater number of phenomena should be attributed to it than it really produces.

In the views that I have ventured to develop, neither oxygen, chlorine, or fluorine, are asserted to be elements; it is only asserted that, as yet, they have not been decomposed.[1]

[1] From the *Phil. Trans.* for 1814, vol. civ, pp. 62-73. See pp. 71-5 of *Alembic Club Reprint* No. 9. In the same reprint will be found a particularly interesting extract from another paper of Davy's: "On the Fallacy of the Experiments in which Water is said to have been formed by the Decomposition of Chlorine" (*Phil. Trans.* for 1818, vol. cviii). *Alembic Club*

CHAPTER XXXVI

Robert Boyle

(1627–1691)

[Robert Boyle was one of the founders of the Royal Society. He was the first great investigator who carried out in his labours the principles of the *Novum Organum*. His strength lay in his patient research and observation of facts. Boyle's name is perhaps best known in connection with the Law concerning the relation of the volume and the pressure of a gas. His numerous works (of which Dr. Peter Shaw's classified abridgment is the most useful edition) include *New Experiments touching the Spring of the Air*, and *Hydrostatical Paradoxes*. One of these paradoxes, so called, has been selected for inclusion here.

The pressure of the air was quite unknown to the ancients, and was only first perceived by Galileo on the occasion of an ordinary pump refusing to draw water above a certain height. Before that time it had always been supposed that water rose by suction in a pipe because nature *abhorred a vacuum*, and so obliged the water to enter in order to supply the place of the air sucked out. Although the true cause of the phenomenon eventually occurred to Galileo, it was not satisfactorily demonstrated until his pupil Torricelli, in 1642, thought of using mercury as a substitute for water. But Torricelli's discovery was much disputed, and at last Pascal proposed a *crucial experiment*. Pascal could see that if the column of mercury varied with the pressure of the air, the height of the column would be diminished if the experiment were made at a higher elevation, say on a mountain. In 1647 Pascal caused the experiment to be performed on the Puy de Dome, and the point at issue was settled once for all.

Boyle "presented" his Paradoxes to the Royal Society in 1664. It would appear from the following that even at that date the old notion that "nature abhors a vacuum" had not entirely disappeared.]

Paradox X.—The cause of the ascent of water in syphons, and of its flowing through them, may be explicated without having recourse to nature's abhorrency of a vacuum.

Both philosophers and mathematicians having too generally confessed themselves reduced to fly to a *fuga vacui*, for an account of the cause of the running of water and other liquids through syphons; and even those moderns that admit a vacuum, having either left the phenomenon unexplained, or endeavoured to explain it by disputable notions; I think the curious will be much obliged to Mon-

Reprint No. 6 gives extracts from Davy's papers on "The Decomposition of the Alkalis and Alkaline Earths".

The reader is strongly advised, especially if he is interested in Chemistry, to refer to the other reprints of the Alembic Club. Researches by Cavendish, Wollaston, Dalton, Hooke, Priestley, Scheele, Graham, Jean Rey, Faraday, Berthollet, Pasteur, and others, are reprinted in convenient form; and the labour of hunting out original records in the great national libraries or libraries of the learned societies may thus be avoided.

sieur Pascal for having ingeniously endeavoured to show, that this difficult problem need not reduce us to have recourse to a *fuga vacui*. And indeed his explanation of the motion of water in syphons seems to me so consonant to hydrostatical principles that I think it not necessary to alter anything in it. But as for the experiment he proposes to justify his reasoning, I fear his readers will scarce be much invited to attempt it. For besides that it requires a great quantity of quicksilver, and a new kind of syphon 15 or 20 feet long, the vessels of quicksilver must be placed 6 or 7 yards under water, that is, at so great a depth, that I doubt whether men who are not divers will be able conveniently to observe the progress of the trial.

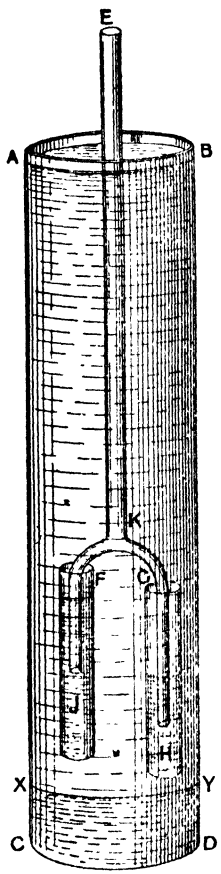


Fig. 8

Wherefore we will substitute a way.—Provide a glass jar A B C D, of a good wideness, and half a yard or more in depth; provide also a syphon of two legs F K and K G, to which is joined at the upper part of the syphon a pipe E K, in such manner that the cavity of the pipe communicates with the cavities of the syphon. To each of the two legs of this new syphon must be tied with a string a glass tube, J and H, sealed at one end; the open end of each tube admits a good part of the leg of the syphon to which it is fastened, which leg must reach a pretty good way beneath the surface of the water, with which the said tube is to be almost filled. But as one of these legs is longer than the other, so the surface of the water in the suspended tube I which is fastened to the shorter leg K F, must be higher (that is, nearer to K or A B) than the surface of the water in the tube H suspended from the longer leg K G; that (as usual in syphons) the water may run from a higher vessel to a lower.

All things being thus provided, and the pipe E K being made fast that it may not be moved, pour oil of turpentine into the jar A B C D,[1] till it reach higher than the top of the syphon F K G (whose orifice E you may, if you please, in the meantime close with your

[1] If you have not much oil, pour in water beforehand till it reaches near the bottom of the suspended tubes, as to the level X Y.

finger, or otherwise, and afterwards unstop), and then the oil pressing upon the water will make it ascend into the legs of the syphon, and pass through it, out of the uppermost vessel J into the lowermost H; and if the vessel J were supplied with water, the course of the water through the syphon would continue longer than here (by reason of the paucity of water) it can do.

Now in this experiment we manifestly see the water made to take its course through the legs of a syphon from a higher vessel into a lower, and yet the top of the syphon being perforated at K, the air has free access to each of the legs of it, through the hollow pipe E K which communicates with them both. So that, in our case, where there is no fear of a vacuum, the fear of a vacuum cannot with any show of reason be pretended to be the cause of the water running. Wherefore, we must seek out some other.

And it will not be very difficult to find, that it is partly the pressure of the oil, and partly the contrivance and situation of the vessels, if we will but consider the matter attentively. For the oil that reaches much higher than K presses upon the surface of the external water in each of the suspended tubes J and H. I say the *external water*, because the oil floating upon the water has no access to the cavity of either of the legs F and G. Wherefore, since the oil gravitates upon the water outside the legs, and not upon that inside them, and since its height above the water is great enough to press up the water into the cavity of the legs of the syphon and impel it as high as K, the water must by that pressure be made to ascend.

And this raising of the water happening at first in both legs, there will be a kind of conflict about K betwixt the two ascending portions of water, and therefore we will now examine which must prevail.

And if we consider that the pressure sustained by the two parcels of water in the suspended tubes J and H depends upon the height of the oil that presses upon them respectively, it may seem at the first view that the water should be driven out of the lower vessel into the higher. For if we suppose that part of the shorter leg that is un-immersed under water to be 6 inches long, and the un-immersed part of the longer leg to be 7 inches, then, because the surface of the water in the vessel J is an inch higher than that of the water in the vessel H, it will follow that there is a greater pressure upon the water in which the longer leg is dipped by the weight of an inch of oil; so that that liquid being an inch higher upon the surface of the water in the tube H than upon that in the tube J it

seems that the water ought rather to be driven from H towards K than from J towards K.

But then we must consider that though the descent of the water in the leg G be more resisted than that in the other leg by as much pressure as the weight of *an inch of oil* can amount to, yet being longer by an inch than the water in the leg F, it tends downwards more strongly by the weight of *an inch of water*, by which length it exceeds the water in the opposite leg. So that an inch of water being (*ceteris paribus*) heavier than an inch of oil, the water in the longer leg, notwithstanding the greater resistance of the external oil, has a stronger endeavour downwards than has the water in the shorter leg, though the descent of this be resisted but by a depth of oil less by an inch. So that all things computed, the motion must be made towards that way where the endeavour is most forcible, and consequently the course of the water must be from the upper vessel and the shorter leg, into the longer leg and so into the lower vessel.

The application of this to what happens in syphons is obvious enough. For, when once the water is brought to run through a syphon, the air (which is a fluid and has some gravity, and has no access into the cavity of the syphon) must necessarily gravitate upon the water in which the legs of the syphon are dipped, and not upon that which is within the syphon; and consequently, though the incumbent air has a somewhat greater height upon the water in the lower vessel than upon that in the upper, yet the gravitation it thereby exercises upon the former more than upon the latter, being very inconsiderable, the water in the longer leg much preponderating (by reason of its length) over the water in the shorter leg, the efflux must be out of that leg, and not out of the other. And the pressure of the external air being able to raise water (as we find by suction pumps) to a far greater height than that of the shorter leg of the syphon, the efflux will continue, for the same reason, until the exhaustion of the water or some other circumstance alters the case. But if the legs of the syphon should exceed 34 or 35 feet of perpendicular altitude, the water would not flow through it, the pressure of the external air being unable to raise water to such a height. And if a hole being made at the top of a syphon, that hole should be unstopped while the water is running, the course of it would presently cease. For in that case the air would gravitate upon the water, inside as well as outside the cavity of the syphon; and so the water in each leg would, by its own weight, fall back into the vessel belonging to it.

But because this last circumstance, though clearly deducible from hydrostatical principles and experiments, has not, that I know of, been verified by particular trials, I caused two syphons to be made, the one of tin, the other of glass, each of which had, at the upper part of the bend, a small round hole or socket, which I could stop and unstop, at pleasure, with my finger. So that when the water was running through the syphon, if I removed my finger, the water would presently fall, partly into one and partly into the other of the vessels underneath. And if the legs of the syphon were so unequal in length, that the water in the one had a far greater height than in the other, there seemed to be, when the liquid began to take its course through the syphon, some light pressure from the external air upon the finger with which I stopped the orifice of the socket made at the bend.

And on this occasion I will add what I more than once tried,—to show at how very minute a passage the pressure of the external air may be communicated to bodies fitted to receive it. For, having for this purpose stopped the orifice of one of the above-mentioned syphons (instead of doing it with my finger), with a piece of oiled paper, carefully fastened with cement to the sides of the socket, I found as I expected that though by this means the syphon was so well closed that the water ran freely through, yet, if I made a hole with the point of a needle, the air would, at so very little an orifice, insinuate itself into the cavity of the syphon, and thereby gravitating inside as well as outside, make the water in the legs to fall down into the vessels. And though, if I held the point of the needle in the hole I made, and then caused someone to suck at the longer leg, this small stopper sufficed to make the syphon fit for use; yet, if I removed the needle, the air would get in at the hole and put a final stop to the course of the water. Nor was I able to take out the needle and put it in again so nimbly, but that the air found time to get in at the syphon; and, till the hole were again stopped, render it useless, notwithstanding that the water was by suction endeavoured to be set a running.

CHAPTER XXXVII

Sir Isaac Newton

(1642–1727)

[Sir Isaac Newton, one of the greatest mathematicians and physicists the world has ever known, was elected a Fellow of Trinity College, Cambridge, in 1667, and a Fellow of the Royal Society in 1672. Of his numerous works, the famous *Principia* is the best known. Despite his remarkable powers and his penetrating intellect, he was a singularly modest and unassuming man. A short time before his death he uttered this memorable sentiment: "I seem to have been only like a boy playing on the seashore and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst a great ocean of truth lay all undiscovered before me".[1] His character as a man was almost beyond reproach, though his behaviour towards Leibnitz relative to the discovery of the calculus shows that he was quite capable of asserting and defending his rights.—The following investigation is from the First Book of the *Opticks*, in which branch of Science Newton made many important discoveries.]

The Light of the Sun consists of Rays differently Refrangible.[2] The Proof by Experiments

Experiment 1.—In a very dark chamber, at a round hole about one-third part of an inch broad made in the shutter of a window, I placed a glass prism, whereby the beam of the sun's light which came in at the hole might be refracted upwards toward the opposite wall of the chamber, and there form a coloured image of the sun. The axis of the prism was in this and the following experiment perpendicular to the incident rays. About this axis I turned the prism slowly, and saw the refracted light on the wall, or coloured image of the sun, first to descend and then to ascend. Between the descent and ascent, when the image seemed stationary, I stopped the prism and fixed it in that position. For in that position, the refractions of the light at the two sides of the refracting angle, that is at the entrance of the rays into the prism, and at their going out of it, were equal to one another. So also in other experiments, as often as I would have the refractions on both sides the prism to be equal to one another, I noted the place where the image of the sun formed by the refracted light stood still between its two contrary motions; and when the image fell upon that place, I made fast the prism. And in this position it is to be understood that all the prisms are placed in

[1] Brewster's *Life of Newton*, vol. ii. [2] "Prop. II, Theor. II" (considerably abridged).

the following experiments, unless some other position is described. The prism, therefore, being placed in this position, I let the refracted light fall perpendicularly upon a sheet of white paper at the opposite wall of the chamber, and observed the figure and dimensions of the solar image formed on the paper by that light. This image was oblong and not oval, but terminated with two rectilinear and parallel sides, and two semicircular ends. On its sides it was bounded pretty distinctly, but on its ends very indistinctly, the light there decaying and vanishing by degrees. The breadth of this image answered to the sun's diameter, and was about $2\frac{1}{8}$ inches, including the penumbra. For the image was $18\frac{1}{2}$ feet distant from the prism, and at this distance that breadth if diminished by the diameter of the hole in the window shutter, that is by $\frac{1}{4}$ inch, subtended an angle at the prism of about half a degree, which is the sun's apparent diameter. But the length of the image was about $10\frac{1}{4}$ inches, and the length of the rectilinear sides about 8 inches; and the refracting angle of the prism whereby so great a length was made, was 64 degrees. With a less angle, the length of the image was less, the breadth remaining the same. If the prism was turned about its axis that way which made the rays emerge more obliquely out of the second refracting surface of the prism, the image soon became an inch or two longer, or more; and if the prism was turned about the contrary way, so as to make the rays fall more obliquely on the first refracting surface, the image soon became an inch or two shorter. And therefore in trying this experiment I was as curious as I could be in placing the prism by the above-mentioned rule exactly in such a position that the refractions of the rays at their emergence out of the prism might be equal to that at their incidence on it. This prism had some veins running along within the glass from one end to the other, which scattered some of the sun's light irregularly, but had no sensible effect in increasing the length of the coloured spectrum. For I tried the same experiment with other prisms with the same success.

Now the different magnitude of the hole in the window shutter, and different thickness of the prism where the rays passed through it, and different inclinations of the prism to the horizon, made no sensible changes in the length of the image. Neither did the different matter of the prisms make any: for in a vessel made of polished plates of glass cemented together in the shape of a prism and filled with water, there is the like success of the experiment according to the quantity of the refraction. It is farther to be

observed that the rays went on in right lines from the prism to the image, and therefore at their going out of the prism had all that inclination to one another from which the length of the image proceeded, that is the inclination of more than $2\frac{1}{2}$ degrees. And yet according to the laws of Optics commonly received, they could not possibly be so much inclined to one another.

Let EG represent the window shutter, F the hole therein through which a beam of the sun's light was transmitted into the darkened chamber, and ABC a triangular plane whereby the prism is supposed to be cut transversely through the middle of the light. And let XY be the sun, MN the paper on which the solar image or spec

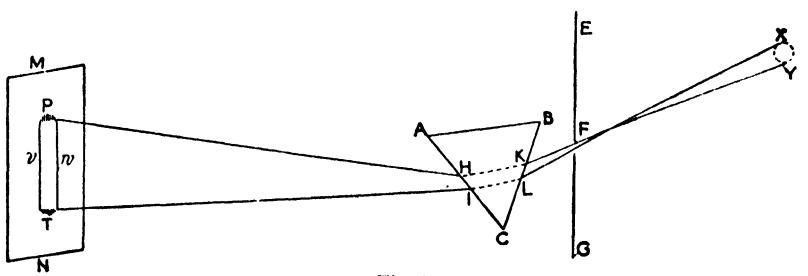


Fig. 4

trum is cast, and PT the image itself whose sides towards v and w are rectilinear and parallel, and ends towards P and T semicircular. YKHP and XLIT are two rays, the first of which comes from the lower part of the sun to the higher part of the image, and is refracted in the prism at K and H, and the latter comes from the higher part of the sun to the lower part of the image, and is refracted at L and I. Since the refractions on both sides of the prism are equal to one another, that is, the refraction at K equal to that at I, and the refraction at L equal to that at H, so that the refractions of the incident rays at K and L taken together are equal to the refractions of the emergent rays at H and I taken together: it follows, by adding equal things to equal things, that the refractions at K and H taken together are equal to the refractions at I and L taken together, and therefore the two rays being equally refracted have the same inclination to one another after refraction which they had before, that is an inclination of half a degree answering to the sun's diameter. So then, the length of the image PT would by the rules of common optics subtend an angle of half a degree at the prism, and consequently be equal to the breadth vw; and therefore the image would be round. Thus it would be, were the two rays

XLIT and YKHP, and all the rest which form the image P*w*T*v*, alike refrangible. And therefore seeing by experience it is found that the image is not round, but about five times longer than broad, the rays which, going to the upper end P of the image, suffer the greatest refraction, must be more refrangible than those which go to the lower end T, unless the inequality of refraction be casual.

This image or spectrum PT was coloured, being red at its least refracted end T, and violet at its most refracted end P, and yellow, green, and blue in the intermediate spaces. Which agrees with the proposition that lights which differ in colour do also differ in refrangibility.

It appears, then, that in equal incidences there is a considerable inequality of refractions. But whence this inequality arises, whether it be that some of the incident rays are refracted more and others less, constantly, or by chance, or that one and the same ray is by refraction disturbed, shattered, dilated, and, as it were, split and spread into many diverging rays, as Grimaldo supposes, will appear by the experiments that follow.

Experiment 2.—Considering that, if in the last experiment, the image of the sun should be drawn out into an oblong form, either by a dilatation of every ray, or by any other casual inequality of the refractions, the same oblong image would, by a second refraction made sideways, be drawn out as much in breadth by the like dilatation of the rays, or other casual inequality of the refractions sideways, I tried what would be the effects of such a second refraction. For this end I ordered all things as in the last experiment, and then placed a second prism immediately after the first in a cross position to it, that it might again refract the beam of the sun's light which came to it through the first prism. In the first prism this beam was refracted upwards, and in the second sideways. And I found that by the refraction of the second prism, the breadth of the image was not increased, but its superior part which in the first prism suffered the greater refraction, and appeared violet and blue, did again in the second prism suffer a greater refraction than its inferior part, which appeared red and yellow, and this without any dilatation of the image in breadth.

Let S represent the sun, F the hole in the window, ABC the first prism, DH the second prism, Y the round image of the sun made by a direct beam of light when the prisms are taken away, PT the oblong image of the sun made by that beam passing through the first prism alone when the second prism is taken away, and *pt* the

image made by the cross refractions of both prisms together. Now if the rays which tend towards the several points of the round image Y were dilated and spread by the refraction of the first prism, so that they should not any longer go in single lines to single points, but that every ray being split, shattered, and changed from a linear ray to a superficies of rays diverging from the point of refraction, and lying in the plane of the angles of incidence and refraction, they should go in those planes to so many lines reaching almost from one end of the image P T to the other; and if that image should thence become oblong, those rays and their several parts tending towards the several points of the image P T ought to be again dilated and spread sideways by the transverse refraction of the second prism, so as to compose a four-square image, such as is represented at $\pi\tau$.

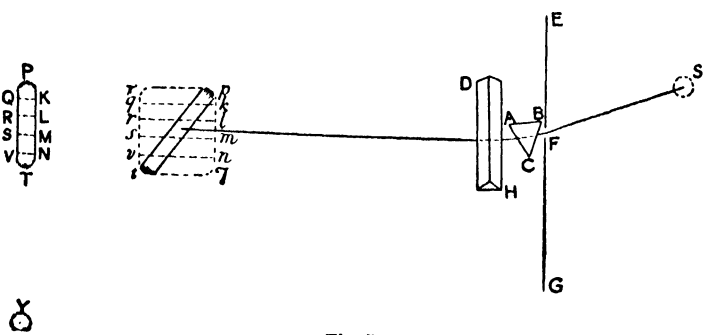


Fig. 5

For the better understanding of which let the image P T be distinguished into five equal parts P Q K, K Q R L, L R S M, M S V N, N V T. And by the same irregularity that the orbicular light Y is by the refraction of the first prism dilated and drawn out into a long image P T, the light P Q K, which takes up a space of the same length and breadth with the light Y, ought to be, by the refraction of the second prism, dilated and drawn out into the long image $\pi q k p$; and the light K Q R L into the long image $k q r l$; and so with the rest; and all these long images would compose the four-square image $\pi\tau$. Thus it ought to be, were every ray dilated by refraction, and spread into a triangular superficies of rays diverging from the point of refraction. For the second refraction would spread the rays one way as much as the first does another, and so dilate the image in breadth as much as the first does in length. And the same thing ought to happen, were some rays casually refracted more than others.—But the event is otherwise. The image P T was *not* made

broader by the refraction of the second prism, but only became oblique, as it is represented by *p t*, its upper end P being by the refraction translated to a greater distance than its lower end T. So then the light which went towards the upper end P of the image was (at equal incidences) more refracted in the second prism than the light which tended towards the lower end T, that is the blue and violet, than the red and yellow; and therefore was more refrangible. The same light was by the refraction of the first prism translated farther from the place Y to which it tended before refraction; and therefore suffered as well in the first prism as in the second a greater refraction than the rest of the light, and consequently was more refrangible than the rest even before its incidence on the first prism.

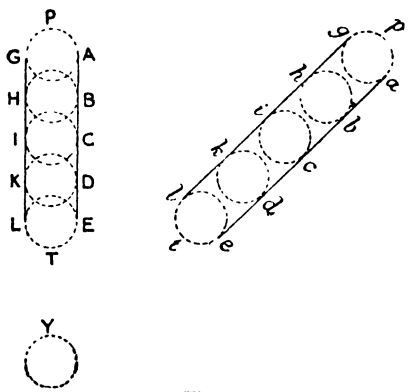


Fig. 6

Sometimes I placed a third prism after the second, and sometimes also a fourth after the third; but the rays which were more refracted than the rest in the first prism were also more refracted in all the others, and that without any dilatation of the image sideways.

But that the meaning of the experiment may more clearly appear, it is to be considered that the rays which are equally refrangible do fall upon a circle answering to the sun's disc. For this was proved in the first experiment. Let therefore A G represent the circle which all the most refrangible rays, propagated from the whole disc of the sun, would illuminate and paint upon the opposite wall if they were alone; E L the circle which all the least refrangible rays would in like manner illuminate and paint if they were alone; B H, C I, D K, the circles which so many intermediate sorts of rays would successively paint upon the wall if they were singly propagated from the sun in successive order, the rest being always intercepted; and conceive that there are other intermediate circles without number, which innumerable other intermediate sorts of rays would successively paint upon the wall if the sun should successively emit every sort apart. And seeing the sun emits all these sorts at once, they must all together illuminate and paint innumerable equal circles, of all which, being according to

their degrees of refrangibility placed in order in a continual series, that oblong spectrum P T is composed which I described in the first experiment. Now if the sun's circular image Y which is made by an unrefracted beam of light was by any dilatation of the single rays, or by any other irregularity in the refraction of the first prism, converted into the oblong spectrum P T, then ought every circle A G, B H, &c., in that spectrum, by the cross refraction of the second prism again dilating or otherwise scattering the rays as before, to be in like manner drawn out and transformed into an oblong figure, and thereby the breadth of the image P T would now be as much augmented as the length of the image Y was before by the refraction of the first prism; and thus by the refractions of both prisms together would be formed a four-square figure $p\pi t\tau$, as I described above. Wherefore, since the breadth of the spectrum P T is not increased by the refraction sideways, it is certain that the rays are not split or dilated or otherwise irregularly scattered by that refraction, but that every circle is by a regular and uniform refraction translated entire into another place, as the circle A G by the greatest refraction into the place $a g$; the circle B H by a less refraction into the place $b h$; and so of the rest; by which means a new spectrum $p t$ inclined to the former P T is in like manner composed of circles lying in a right line; and these circles must be of the same size as the former, because the breadths of all the spectrums Y, P T, and $p t$ at equal distances from the prisms are equal.

Experiment 3.—In the middle of two thin boards I made round holes one-third part of an inch in diameter, and in the window shutter a much broader hole to let into my darkened chamber a large beam of the sun's light. I placed a prism behind the shutter in that beam to refract it towards the opposite wall, and close behind the prism I fixed one of the boards, in such manner that the middle of the refracted light might pass through the hole made in it, and the rest be intercepted by the board. Then at the distance of about 12 feet from the first board I fixed the other board in such manner that the middle of the refracted light which came through the hole in the first board and fell upon the opposite wall might pass through the hole in this other board, and the rest being intercepted by the board might paint upon it the coloured spectrum of the sun. And close behind the board I fixed another prism to refract the light which came through the hole. Then I returned speedily to the first prism, and by turning it slowly to and fro about its axis, I caused the image which fell upon the second board to move up and down

upon that board, that all its parts might successively pass through the hole in that board and fall upon the prism behind it. And in the meantime I noted the places on the opposite wall to which that light after its refraction in the second prism did pass; and by the difference of the places I found that the light which being most refracted in the first prism did go to the blue end of the image, was again more refracted in the second prism than the light which went to the red end of that image. And this happened whether the axes of the two prisms were parallel or inclined to one another and to the horizon in any given angles.

Let F be the wide hole in the window shutter, through which the sun shines on the first prism ABC, and let the refracted light

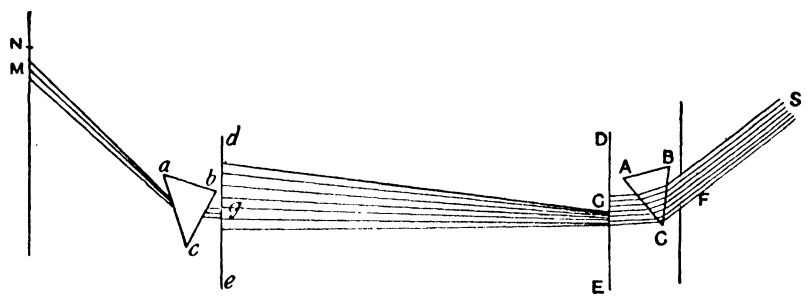


Fig. 7

fall upon the middle of the board DE, and the middle part of that light upon the hole G made in the middle of that board. Let this trajected part of the light fall again on the middle of the second board *d c*, and there paint such an oblong coloured image of the sun as was described in the first experiment. By turning the prism ABC slowly to and fro about its axis, this image will be made to move up and down the board *d e*, and by this means all its parts from one end to the other may be made to pass successively through the hole *g* which is made in the middle of the board. In the meanwhile another prism *a b c* is to be fixed next after that hole *g* to refract the trajected light a second time. And these things being thus ordered, I marked the places M and N of the opposite wall upon which the refracted light fell, and found that whilst the two boards and second prism remained unmoved, those places by turning the first prism about its axis were changed perpetually. For when the lower part of the light which fell upon the second board *d e* was cast through the hole *g*, it went to a lower place M on the wall; and when the higher part of that light was cast through the same hole

g, it went to a higher place N on the wall; and when any intermediate part of the light was cast through that hole, it went to some place on the wall between M and N. The unchanged position of the holes in the boards, made the incidence of the rays upon the second prism to be the same in all cases. And yet in that common incidence, some of the rays were more refracted and others less. And those were more refracted in this prism which by a greater refraction in the first prism were more turned out of the way, and therefore for their constancy of being more refracted are deservedly called more refrangible.[1]

In this way Newton at last arrives at his great generalization that the light of the sun is not homogeneous but consists of rays of different refrangibility.

CHAPTER XXXVIII

Michael Faraday

(1791–1867)

[Faraday was appointed assistant to Sir Humphry Davy at the Royal Institution in 1813, and became Director of the laboratory at the same Institution in 1825, and Fullerian Professor of Chemistry in 1833. With no Mathematics beyond simple Arithmetic, Faraday displayed powers of experiment and generalization so extraordinary that in these respects he stands on the same level as Newton himself. "The rare ingenuity of his mind was ably seconded by his manipulative skill, while the quickness of his perceptions was equalled by the calm rapidity of

[1] For the completion of this investigation, the reader must turn to the original

There are few facts in the history of optics more singular than that Newton should have believed that different bodies when shaped into prisms all produced prismatic spectra of equal length, or dispersed the red and violet rays to equal distances, when the mean refraction, or the refraction of the middle ray of the spectrum, was the same. This opinion, which he deduced from no direct experiments, and into which no theoretical views could have led him, seems to have been impressed on his mind with all the force of an axiom. In one of his experiments he had occasion to counteract the refraction of a prism of glass by a prism of water. Had he completed the experiment and studied the result of it when the mean refraction of the two prisms was the same, he could hardly have failed to observe that the prism of water did not correct the colour of the prism of the glass, and would thus have been led to the great truth that different bodies have different dispersive powers It is curious to observe, as happened in this experiment, what trifling circumstances often arrest an investigator when on the very verge of a discovery. Newton had mixed with the water which he used in his prism a little sugar of lead, in order to increase the refractive power of the water; but the sugar of lead having a higher dispersive power than water, made the dispersive power of the water prism equal to that of the prism of glass, so that if Newton had completed the experiment, the use of the sugar of lead would have prevented him from making the important discovery which was almost within his possession. (See Brewster's *Life of Newton*, vol. i, ch. v.)

his movements." Dr. Bence Jones,[1] Professor Tyndall,[2] and Dr. J. H. Gladstone,[3] all speak of his sweetness of disposition and nobility of character, and refer time after time to his amazing fertility of resource as an investigator. His *Experimental Researches in Electricity* have been well described as imperishable. The following investigation forms the eighteenth series of the second volume.]

On the Electricity Evolved by the Friction of Water and Steam against other Bodies

2075. Two years ago an experiment was described by Mr. Armstrong and others, in which the issue of a stream of high-pressure steam into the air produced abundance of electricity. The source of the electricity was not ascertained, but was supposed to be the evaporation or change of state of the water, and to have a direct relation to atmospheric electricity. I have at various times since May of last year been working upon the subject, and the Royal Society may perhaps think a compressed account of my results and conclusions worthy its attention.

2076. The apparatus I have used was not competent to furnish me with much steam or a high pressure, but I found it sufficient

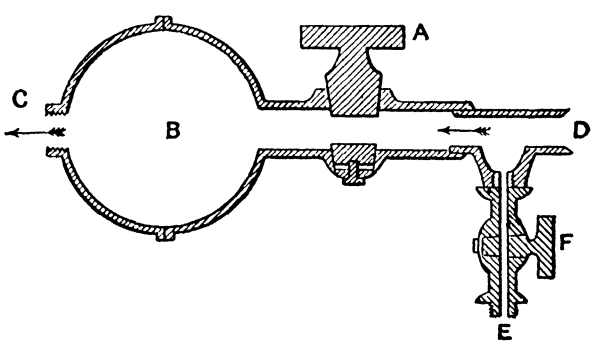


Fig. 8

for my purpose, which was the investigation of the effect and its cause, and not necessarily an increase of the electric development. The boiler I used would hold about ten gallons of water. A pipe $4\frac{1}{2}$ feet long was attached to it, at the end of which was a large stop-cock (A) and a metal globe (B), of the capacity of 32 cubic inches, which I will call the *steam-globe*, and to this globe by its mouthpiece (C), could be attached various forms of apparatus, serv

[1] *Life of Faraday*, by Dr Bence Jones.
[2] *Faraday as a Discoverer*, by Professor Tyndall.
[3] *Michael Faraday*, by Dr. J. H. Gladstone.

ing as vents for the issuing steam.[1] Thus, (*a*) *a cock* could be connected with the steam-globe at C, and this cock be used as the experimental steam-passage; or (*b*) *a wooden tube* could be screwed in; or (*c*) a small *metal or glass tube* put through a good cork and the cork screwed in; and in these cases the steam way of the globe and tube (D) leading to the boiler was so large that they might be considered as part of the boiler, and these terminal passages (from C) as the obstacles which, restraining the issue of steam, produced any important degree of friction.

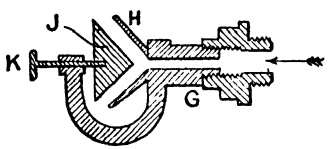


Fig. 9

2077. Another issue piece (for screwing in at C) consisted of a metal tube (G) terminated by a metal funnel (H), and of a *cone* (J) advancing by a screw (K) more or less into the funnel, so that the steam as it rushed forth beat against the cone; and this cone could either be electrically connected with the funnel and boiler, or be insulated.

2078. Another terminal piece consisted of a tube (L) with a stop-cock (M) and *feeder* (N) attached to the top part of it, by which any fluid could be admitted into the passage, and carried on with the steam. (The feeder was a glass-tube or retort-neck, fitted by a cork into the cap of the stop-cock.)

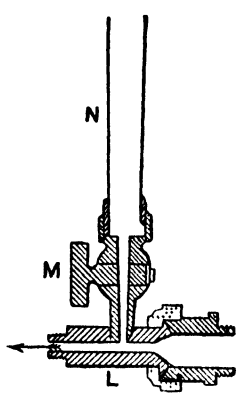


Fig. 10

2079. In another terminal piece, a small *cylindrical chamber* (P) was constructed, into which different fluids could be introduced, so that, when the cocks were opened, the steam passing on from the steam-globe should then

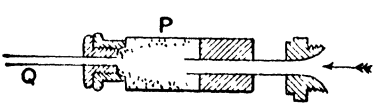


Fig. 11

enter this chamber and take up anything that was there, and so proceed with it into the final passage (Q), or out against the cone (J), according as the apparatus had been combined together.

2080. The pressure at which I worked with the steam was from

[1] In the diagram, D represents the 4½-ft. pipe coming from the boiler; E is a drainage tube and F its cock, for removing the water condensed in the pipe D.

8 to 13 inches of mercury, never higher than 13 inches, or about two-fifths of an atmosphere.

2081. The boiler was insulated on three small blocks of lac. The insulation was so good that when the boiler was attached to a gold-leaf electrometer and charged purposely, the divergence of the leaves did not alter either by the presence of a large fire, or the abundant escape of the results of combustion.

2082. When the issuing steam produces electricity, there are two ways of examining the effect; either the insulated boiler may be observed, or the steam may be examined, but these states are always contrary one to the other. I attached to the boiler both a gold-leaf and a discharging electrometer; the first showed any charge short of a spark, and the second, by the number of sparks in a given time, carried on the measurement of the electricity evolved. The state of the steam may be observed either by sending it through an insulated wide tube in which are some diaphragms of wire gauze, which serves as a discharger to the steam, or by sending a puff of it near an electrometer when it acts by induction; or by putting wires and plates of conducting matter in its course, and so discharging it. To examine the state of the boiler or substance against which the steam is excited, is far more convenient than to go for the electricity to the steam itself; and in this paper I shall give the state of the former, unless it be otherwise expressed.

2083. Proceeding to *the cause of the excitation*, I may state first that I have satisfied myself it is not due to evaporation or condensation, nor is it affected by either the one or the other. When the steam was at its full pressure, if the valve were suddenly raised and taken out, no electricity was produced in the boiler, though the evaporation was for the time very great. Again, if the boiler were charged by excited resin before the valve was opened, the opening of the valve and consequent evaporation did not affect this charge. Again, having obtained the power of constructing steam passages which should give either the positive, or the negative, or the neutral state (2102, 2110, 2117), I could attach these to the steam way, so as to make the boiler either positive, or negative, or neutral at pleasure with the same steam, and whilst the evaporation for the whole time continued the same. So that the excitation of electricity is clearly independent of the evaporation or of the change of state.

2084. The issue of *steam alone* is not sufficient to evolve electricity. To illustrate this point I may say that the *cone apparatus* (2077) is an excellent exciter; so also is a *boxwood tube*, soaked in

water, and screwed into the steam-globe. If with either of these arrangements the steam-globe be empty of water, so as to catch and retain that which is condensed from the steam, then after the first moment (2089), and when the apparatus is hot, the issuing steam excites no electricity; but when the steam-globe is filled up so far that the rest of the condensed water is swept forward with the steam, abundance of electricity appears. If then the globe be emptied of its water, the electricity ceases; but upon filling it up to the proper height, it immediately reappears in full force. So when the *feeder apparatus* (2078) was used, whilst there was no water in the passage tube, there was no electricity; but on letting in water from the feeder, electricity was immediately evolved.

2085. The electricity is due entirely *to the friction of the particles of water* which the steam carries forward against the surrounding solid matter of the passage, or that which, as with the cone, is purposely opposed to it, and is in its nature like any other ordinary case of excitement by friction. As will be shown hereafter, a very small quantity of water properly rubbed against the obstructing or interposed body, will produce a very sensible proportion of electricity.

2086. Of the many circumstances affecting this evolution of electricity, there are one or two which I ought to refer to here. *Increase of pressure* greatly increases the effect, simply by rubbing the two exciting substances more powerfully together. Increase of pressure will sometimes change the positive power of a passage to negative; not that it has power of itself to change the quality of the passage, but as will be seen presently (2108), by carrying off that which gave the positive power; no increase of pressure, as far as I can find, can change the negative power of a given passage to positive.

2087. *The shape and form of the exciting passage* has great influence, by favouring more or less the contact and subsequent separation of the particles of water and the solid substance against which they rub.

2088. When the mixed steam and water pass through a *tube* or *stop-cock* (2076), they may issue, producing either a hissing smooth sound, or a rattling rough sound; and with the *cone apparatus* these conditions alternate suddenly. With the smooth sound, little or no electricity is produced; with the rattling sound, plenty. The rattling sound accompanies that irregular rough vibration, which casts the water more violently and effectually against the substance of the passage, and which again causes the better excitation. I converted the end of the passage into a steam-whistle, but this did no good.

2089. If there be no water in the steam-globe, upon opening the steam-cock the *first effect* is very striking; a good excitement of electricity takes place, but it very soon ceases. This is due to water condensed in the cold passages, producing excitement by rubbing against them. Thus, if the passage be a stop-cock, whilst cold it excites electricity with what is supposed to be steam only; but as soon as it is hot, the electricity ceases to be evolved. If, then, whilst the steam is issuing, the cock be cooled by an insulated jet of water, it resumes its power. If, on the other hand, it be made hot by a spirit-lamp before the steam be let on, then there is *no* first effect.

2090. We find, then, that *particles of water rubbed against other bodies by a current of steam evolve electricity.* For this purpose, however, it is not merely water but *pure* water which must be used. On employing the *feeding apparatus* (2078), which supplied the rubbing water to the interior of the steam passage, I found, as before said, that with steam only I obtained no electricity (2084). On letting in distilled water, abundance of electricity was evolved; on putting a small crystal of sulphate of soda, or of common salt into the water, the evolution ceased entirely. Re-employing distilled water, the electricity appeared again; on using the common water supplied to London, it was unable to produce it.

2091. Again, using the steam-globe and a *boxwood tube* (2076), which excites well if the water distilling over from the boiler be allowed to pass with the steam, when I put a small crystal of sulphate of soda, of common salt, or of nitre, or the smallest drop of sulphuric acid, into the steam-globe with the water, the apparatus was utterly ineffective, and no electricity could be produced. On withdrawing such water, and replacing it by distilled water, the excitement was again excellent; on adding a very small portion of any of these substances, it ceased; but upon again introducing pure water it was renewed.

2092. Common water in the steam-globe was powerless to excite. A little potash added to distilled water took away all its power; so also did the addition of *any* of those saline or other substances which give conducting power to water.

2093. The effect is evidently due to the water becoming so good a conductor, that upon its friction against the metal or other body, the electricity evolved can be immediately discharged again, just as if we tried to excite lac or sulphur by flannel which was damp instead of dry. It shows very clearly that the exciting effect, when it occurs, is due to water and not to the passing steam.

2094. As ammonia increases the conducting power of water only in a small degree, I concluded that it would not take away the power of excitement in the present case. Accordingly, on introducing some to the pure water in the globe, electricity was still evolved though the steam of vapour and water was able to redden moist turmeric paper. But the addition of a very small portion of dilute sulphuric acid, by forming sulphate of ammonia, took away all power.

2095. When in any of these cases, the steam-globe contained water which could not excite electricity, it was beautiful to observe how, on opening the drainage cock which was inserted into the steam-pipe before the steam-globe (the use of which was to draw off the water condensed in the pipe before it entered the steam-globe), electricity was instantly evolved; yet a few inches further on the steam was quite powerless, because of the small change in the quality of the water over which it passed and which it took with it.

2096. When a *wooden* or *metallic tube* (2076) was used as the exciting passage, the application of solution of salts to the outside end of the tube in no way affected the evolution. But when a *wooden cone* (2077) was used, and that cone moistened with the solutions, there was no excitement on first letting out the steam, and it was only as the solution was washed away that the power appeared; soon rising, however, to its full degree.

2097. Having ascertained these points respecting the necessity of water and its purity, the next for examination was *the influence of the substance against which the stream of steam and water rubbed.* For this purpose I first used *cones* (2077) of various substances, either insulated or not; and the following, namely, brass, boxwood, ivory, linen, kerseymere [1], white silk, sulphur, caoutchouc, oiled silk, melted caoutchouc and resin, all became negative, causing the stream of steam and water to become positive. The fabrics were applied stretched over wooden cones. The melted caoutchouc was spread over the surface of a boxwood or a linen cone, and the resin cone was a linen cone dipped in a strong solution of resin in alcohol and then dried. A cone of wood dipped in oil of turpentine, another cone soaked in olive oil, were at first inactive, and then gradually became negative, at which time the oil of turpentine and olive oil were found cleared off from the parts struck by the stream of steam and water. A cone of kerseymere, which had been dipped in alcoholic solution of resin and dried two or three times in succession,

[1] A woollen fabric.

was very irregular, becoming positive and negative by turns, in a manner difficult to comprehend at first, but easy to be understood hereafter (2113).

2098. The end of a rod of shell-lac was held a moment in the stream of steam, and then brought near a gold-leaf electrometer; it was found excited negatively. The corner of a plate of sulphur showed the same effect.

2099. Another mode of examining the substances rubbed was to use it in the shape of wires, threads, or fragments, holding them by an insulated handle in the jet whilst they were connected with a gold-leaf electrometer. *All* the substances thus examined[1] were rendered negative, though not in the same degree. This apparent difference in degree did not depend *only* upon the specific tendency to become negative, but also upon the conducting power of the body itself whereby it gave its charge to the electrometer; upon its tendency to become wet, by which its conducting quality was affected; and upon its size or shape.

2100. For the purpose of preventing condensation on the substance, I made a platinum wire white-hot by an insulated voltaic battery, and introduced it into the jet: it was quickly lowered in temperature by the stream of steam and water to 212°, but of course could never be below the boiling-point. No difference was visible between the effect at the first instant of introduction or any other time. It was always instantly electrified and negative.

2101. The threads I used were stretched across a fork of stiff wire, and the middle part of the thread was held in the jet of vapour. In this case the thread, if held exactly in the middle of the jet, and looked at endways to the thread, was seen to be still, but if removed the least degree to the right or left of the axis of the stream, it (very naturally) vibrated, or rather rotated, describing a beautiful circle, of which the axis of the stream was the tangent. The interesting point was to observe that when the thread rotated, travelling as it were with the current, there was little or no electricity evolved, but that when it was nearly or quite stationary, there was abundance of electricity, thus illustrating the effect of friction.

2102. The difference in the quality of the substances (2099) gives a valuable power of arrangement at the jet. Thus if a metal, glass, or wood tube[2] (2076) be used for the steam issue, the boiler is

[1] Faraday gives a list of thirty different substances he actually tried, e.g. various metals, fabrics, hair, glass, ivory, sulphur, charcoal, asbestos, fluor-spar, &c.

[2] A boxwood tube 3 in. long, and one-fifth of an inch inner diameter, well soaked in distilled water and screwed into the steam-globe, is an admirable exciter.

rendered well negative and the steam highly positive; but if a quill tube, or better still, an ivory tube be used, the boiler receives scarcely any charge, and the stream of steam is also in a neutral state. This result not only assists in proving that the electricity is not due to evaporation, but is also very valuable in the experimental enquiry. It was in such a neutral jet of steam and water that the excitation of the bodies already described (2099) was obtained.

2103. Substances, therefore, may be held either in the neutral jet from an ivory tube, or in the positive jet from a wooden or metal tube; and in the latter case effects occurred which if not understood, would lead to great confusion. Thus an insulated wire was held in the stream issuing from a glass or metal tube, about half an inch from the mouth of the tube, and was found to be unexcited; on moving it in one direction a little further off, it was rendered positive; on moving it in the other direction nearer to the tube, it was negative. This was simply because, when near the tube in the forcible part of the current, it was excited and rendered negative, rendering the steam and water more positive than before, but that when further off, in a quieter part of the current, it served merely as a discharger to the electricity previously excited in the exit tube and so showed the same state with it. Platinum, copper, string, silk, wood, plumbago, but not quill, ivory, and bear's hair, could, in this way, be made to assume either one state or the other, according as they were used as exciters or dischargers, the difference being determined by their place in the stream. A piece of fine wire gauze held across the issuing jet shows the above effect very beautifully; the difference of one-eighth of an inch either way from the neutral place will change the state of the wire gauze.

2104. If instead of an excited jet of steam and water (2103), one issuing from an ivory tube (2102), and in the neutral state, be used, then the wires, &c., can no longer be made to assume both states. They may be excited and rendered negative (2099), but at no distance can they become dischargers, or show the positive state.

2105. We have already seen that the presence of a very minute quantity of matter able to give conducting power to the water took away all power of excitation (2090, &c.) up to the highest degree of pressure, i.e. of mechanical friction that I used (2086); and the next point was to ascertain whether it would be so for all the bodies rubbed by the stream, or whether differences in degree would begin to manifest themselves. I therefore tried all these bodies again, at

one time adding about 2 grains of sulphate of soda to the 4 ounces of water which the steam-globe retained as a constant quantity when in regular action, and at another time adding not a fourth of this quantity of sulphuric acid (2091). In both cases all the substances (2099) remained entirely unexcited and neutral. Very probably great increase of pressure might have developed some effect (2086).

2106. With dilute sulphuric acid in the steam-globe, varying from extreme weakness to considerable sourness, I used tubes and cones of zinc, but could obtain *no trace* of electricity. Chemical action, therefore, appears to have nothing to do with the excitement of electricity by a current of steam.

2107. Having thus given the result of the friction of the steam and water against so many bodies, I may here point out the remarkable circumstance of water being *positive* to them all. It very probably will find its place above all other substances, even cat's hair and oxalate of lime. We shall find hereafter that we have power not merely to prevent the jet of steam and water from becoming positive, as by using an ivory tube (2102), but also of reducing its own power when passing through or against such substances as wood, metal, glass, &c. Whether with a jet so reduced we shall still find amongst the bodies above mentioned (2099) some that can render the stream positive and others that can make it negative, is a question yet to be answered.

For the remainder of the investigation, the reader must refer to the original. Briefly, Faraday proceeds as follows:—

2108–2122. Various substances (e.g. turpentine, olive oil) are introduced into the Feeding Apparatus (2078), or Cylindrical Chamber (2079), in order to see what other bodies, than water, would do if their particles were carried forward by the current of steam.

2123–2128. Theoretical considerations.

2129–2137. Compressed air used instead of steam.

2138–2140. Experiments with the air current and dry powders.

2141–2143. Remarks concerning electrification by friction.

Faraday thus concludes his investigation: Finally, I may say that *the cause of the evolution of electricity by the liberation of confined steam is not evaporation; and further, being, I believe, friction, it has no effect in producing, and is not connected with, the general electricity of the atmosphere: also, that as far as I have been able to proceed, pure gases,*

that is, gases not mingled with solid or liquid particles, do not excite electricity by friction against solid or liquid substances.

Almost any of Faraday's researches will be found to be of absorbing interest. One that used to appeal strongly to the writer was Faraday's attempt to discover a possible relation between Gravity and Electricity.—See *Researches*, vol. iii, §§ 2702–2717.

CHAPTER XXXIX

Other Investigators and Writers

Limits of space do not permit of extracts from the works of other investigators, but the reader himself may, with profit, refer to all or any of the following:—

1. *Benjamin Franklin* (1706–1790) was perhaps the most eminent American statesman and philosopher of his time. He was the first to demonstrate the identity of lightning with electricity. His work *Observations and New Experiments on Electricity, made at Philadelphia in America*, includes investigations full of suggestive matter and method, all very interesting and quaintly expressed.

2. *Henry Cavendish* (1731–1810) was a chemist and physicist whose researches went far to place chemical investigation upon a thoroughly sound basis. The Alembic Club reprints include two of Cavendish's papers,—*Experiments on Air*,—read in 1784 and 1785. His *Electrical Researches*, in 696 articles, have been edited by Clerk Maxwell. In this volume reference may specially be made to the famous experiment now commonly known as the "*Cavendish*" *experiment* but sometimes associated with the name of Biot, §§ 218–231; and to the investigation of the *Electrical effects in the torpedo fish*, §§ 395-437.

3. *Sir Humphry Davy* (1778–1829).—See Ch. XXXV.—A little book of 148 pages, *The Safety Lamp, with some Researches on Flame*, published by Davy in 1818, forms an excellent model of investigation.

4. *Sir David Brewster* (1781–1868) was an accurate observer whose general method was empirical rather than mathematical. His *Treatise on Optics* is a general work on the subject, but much of it is

the result of Brewster's own investigation. See, in particular, the chapters on "Polarisation", pp. 157–243.

5. *Lord Kelvin* (William Thomson) (1824–1907) held the Chair of Natural Philosophy in the University of Glasgow for fifty-three years, and was universally recognized as one of the greatest physicists of his time. He published hundreds of original papers bearing on almost every branch of physical science. Many of these papers are difficult to read, but the following, amongst others, will be found to be fine examples of investigation: (1) "Atmospheric Electricity", in *Papers on Electrostatics and Magnetism*, Art. xvi, pp. 192–236; (2) "Elasticity", in vol. iii. of *Mathematical and Physical Papers*, pp. 3–84. (Pp. 84–112 of the latter deal with the mathematical theory of Elasticity, and is beyond the ordinary reader.)

6. *Lord Lister* (1827–1912), the famous surgeon, became a Fellow of the Royal College of Surgeons at the age of twenty-five, and was elected a Fellow of the Royal Society at the age of thirty-three. He was created a baronet in 1883, was raised to the peerage in 1897, and was one of the original members of the Order of Merit (created 1902). From 1860 to 1877 he held appointments in the Universities of Glasgow and Edinburgh, and then came to London. From an early period of his professional career he had been deeply impressed by the great mortality which was then commonly attendant upon surgical operations, and, having decided that the cause must be discoverable, he set about making the search. Deriving hints from Pasteur's work, his profound acumen and untiring patience soon narrowed down the investigation to the best methods of protecting wounds from injurious organisms. How brilliant was his ultimate success, all the world knows. The main results of his life's great work are embodied in *The Collected Papers of Joseph, Baron Lister*. The following papers, amongst others, may be recommended for careful study. Vol. i, "An Inquiry regarding the parts of the Nervous System which regulate the Contractions of the Arteries" (pp. 27–47); "On the Appreciation of a Knowledge of Hydrostatics and Hydraulics to Practical Medicine" (pp. 186–188); vol. ii, "Observations on Ligature of Arteries on the Antiseptic System" (pp. 86–101); "On the Principles of Antiseptic Surgery" (pp. 340–8).

7. *D. I. Mendeléeff* (born 1834) was appointed Professor of Chemistry in the University of St. Petersburg in 1864. His original work covers a wide range, from questions in applied chemistry to the most general problems in Chemical and Physical Theory. The

reader may be referred to his Faraday lecture, delivered at the Royal Institution, 1889, on "The Periodic Law of the Chemical Elements" (see pp. 489–508 of vol. ii of his *Principles of Chemistry*); and to a paper he wrote in 1902, "An Attempt towards a Chemical Conception of the Ether" (*ib.* pp. 509–529).

8. *Lord Rayleigh, O.M., F.R.S.* (born 1842), was Senior Wrangler in 1865, succeeded Clerk Maxwell as Cavendish Professor of Physics in 1879, and was appointed Professor of Natural Philosophy at the Royal Institution in 1887. His work has ever been remarkably noteworthy for its extreme accuracy and precision. He "combines the highest mathematical acumen with refinement of experimental skill". "His textbook on *Sound* is one of the finest examples of a scientific treatise extant." In conjunction with Sir W. Ramsay he discovered argon. This discovery was the outcome of a long series of delicate weighings and of minute experimental care in the determination of the relative density of nitrogen, undertaken in order to determine the atomic weight of that gas. Lord Rayleigh's *Scientific Papers* are published in four volumes. Some of the papers are of a very specialized character, but amongst those of more general interest may be mentioned those on (*a*) "Harmonic Echoes", vol. i, pp. 188–9; (*b*) "On the Dark Plane which is formed over a Heated Wire in Dusty Air", vol. ii, pp. 151–4; and (*c*) "Foam", vol. iii, pp. 351–62; and there are numerous other papers which will strongly appeal to many readers. The papers on the density of nitrogen and on argon are in vol. iv, Nos. 197, 201, 210, 214, 215, 218, and 219. No serious student of physical science can afford not to read, with the utmost care, the four volumes mentioned.

There have been and still are many eminent men of science whose work is better known by its *results* than by the methods actually used. It is possible to turn over the pages of volume after volume of the Proceedings of the Royal Society, without getting any real insight into the methods of the various contributors. There is almost an entire absence of that self-revelation which appears on every page of Newton or Faraday. Exigencies of space no doubt partly account for this, but there is the further reason that the contributions are intended for specialists who are content to know the general lines of procedure adopted and the conclusions arrived at.

It is, however, very necessary for young teachers to distinguish between textbooks which are mere compilations and those which

are written by men acknowledged to be leading authorities in their own departments. The fullness of knowledge and the mastery of the subject in the latter case, lead to an entirely different type of presentation—a presentation illuminated by a thousand side-lights the existence of which will probably not even be suspected by the compiler.

It may be useful to refer the reader to a few instances of the work of eminent men of science, selected specially because worthy of very careful study. In these instances the actual *method* adopted by the different writers will, as far as the reader is concerned, be largely inferential. Most of the instances are, primarily, examples of the *elucidation or demonstration of principles*, and should be read not so much for the sake of the actual facts recorded as for the general lines of procedure, the illustrations, the logical development and arrangement, the logical argument, the logical conclusions, and so forth. Many other excellent examples from the works of the same writers could, of course, easily be selected.

9. *J. Clerk Maxwell, F.R.S.* (1831–79), was the first holder of the Professorship of Experimental Physics at Cambridge, and it was under his direction that the plans of the Cavendish laboratory were prepared. His treatise on *Electricity and Magnetism* was pronounced by the late Professor Tait to be "one of the most splendid monuments ever raised by the genius of a single individual". The reader may refer to this treatise, Part I, ch. i, "Description of Phenomena" (pp. 31–67); ch. ii, "Elementary Mathematical Theory of Statical Electricity" (pp. 68–95); Part II, ch. iv, "Electrolysis" (pp. 345–55); and "Electrolytical Polarisation" (pp. 356–66).

10. *John Tyndall, F.R.S.* (1820–93), was a colleague of Faraday at the Royal Institution, having been appointed Professor of Natural Philosophy there in 1854. He succeeded Faraday as superintendent on the latter's death in 1867. Tyndall was noted for his remarkable power of exposition to the unlearned, and in this respect far excelled all his contemporaries. But he lacked the cultivated caution of a Faraday, or the depth of a Clerk Maxwell. Any of the following will be found full of interest: *Heat a Mode of Motion*, chapters on "Radiant Heat" (pp. 269–423); "The Azure of the Sky" (pp. 468–95)[1]; the same subjects in *Contributions to Molecular Physics*

[1] *Heat a Mode of Motion* is written in a popular form. Tyndall's original papers may, however, be obtained in a convenient volume,—*Contributions to Molecular Physics in the Domain of Radiant Heat.*

in the Domain of Radiant Heat; *Sound*, "Acoustic Transparency of the Atmosphere in Relation to the Question of Fog-signalling" (pp. 284–358); *Fragments of Science*, vol. i, "Dust and Disease" (pp. 131–93); vol. ii, "Spontaneous Generation" (pp. 292–336).

11. *T. H. Huxley, F.R.S.* (1825–95), like his friend Tyndall, possessed very exceptional powers of lucid exposition. His mind was a "clear, cold, logic-engine". Dr. A. R. Wallace, speaking from intimate personal knowledge, recently said that, in sheer intellectual power, Huxley was superior to Darwin. Reference may be made to *Darwiniana*, "Causes of the Phenomena of Organic Nature" (pp. 303–475); *Physiography*, "Geology of the Thames Basin" (pp. 272–98).

12. *Thomas Preston, F.R.S.*, for some time an esteemed colleague of the present writer's, was cut off, a few years ago, in the very prime of life. His *Theory of Heat* and *Theory of Light* are standard works. In the former, the "Preliminary Sketch" (pp. 1–100), and in the latter, "Diffraction, Graphic Methods" (pp. 243–74), are well worth reading.

Selections from the works of living writers are made with some diffidence, lest the authors should feel that the selections are not sufficiently representative. But most, if not all, of the following will appeal to those who delight in close reasoning and clear exposition:

13. *Sir Oliver Lodge, F.R.S.*: *Modern Views of Electricity* (1889 edition), "The Dielectric" (pp. 18–29); *Electrons*, "Electric View of Matter" (pp. 146–62).

14. *Sir Joseph Larmor, F.R.S.*: *Aether and Matter*, "The Scope of Mechanical Explanation; The idea of Force" (pp. 268–88).

15. *Sir J. J. Thomson, O.M., F.R.S.*: British Association *Presidential Address*, 1909; and *Elements of Electricity and Magnetism*, "Electrical Images and Inversion" (pp. 138–83).

16. *Professor J. H. Poynting, F.R.S.*, and *Sir J. J. Thomson, O.M., F.R.S.*: *Properties of Matter*, "Capillarity" (pp. 135–72).

17. *Sir William Crookes, O.M., F.R.S.*: *Select Methods in Chemical Analysis*, "Electrolytic Analysis; and Gas Analysis" (pp. 616–47). These are admirable models of lucid instructions.

18. *Lothar Meyer*: *Outlines of Theoretical Chemistry* (translated by Bedson and Williams), "Valency", "Atomic Linking", "Isomerism" (pp. 65–111).

19. *Wilhelm Ostwald*: *The Principles of Inorganic Chemistry* (translated by Findlay); the chapters on "Water" (pp. 106–52); "Sulphur" (pp. 253–305); and one of the metals, for instance, "Mercury" (pp. 656–72).

20. *Dr. Harold Wager, F.R.S.*: *Vegetable Cytology.* (*Ency. Brit.*)

21. *Professor S. H. Vines, F.R.S.*: *Text Book of Botany*, "Physiology of Plant Movement" (pp. 735–66).

22. *Rev. A. H. Cooke*: *Cambridge Natural History*, vol. on *Mollusca*, "Enemies of the Mollusca, Means of Defence, Mimicry, and Protective Coloration" (ch. iii); "Form, Composition, and Growth of the Shell of Mollusca" (ch. ix).

It is rather an invidious thing to make selections of this kind, and there are numerous other workers in the world of Science whose names are well worthy of inclusion in such a list as the above. But those given are sufficiently representative.

BOOK IV

SCIENTIFIC METHOD IN THE CLASSROOM

CHAPTER XL

Some Elementary Principles of Science Teaching

§ 1. The Heuristic Method

"No heuristic method here, if I can help it", said an angry Science master. "What does Professor Armstrong know about teaching? He says to a boy who cannot yet distinguish a test-tube from a totem, 'Here is a laboratory and here is a piece of rusty iron. I am going to lock you up in the laboratory and keep you there until you have discovered the cause[1] of the rusting of iron'!"

Like most reformers, Professor Armstrong has often been misrepresented and maligned, but as he is quite capable of taking care of himself, there is no need to put in any kind of defence on his behalf here. Brief reference to the heuristic method is, however, necessary.

As Professor Armstrong himself puts it, "Heuristic methods of teaching are methods which involve our placing students as far as possible in the attitude of discoverers, methods which involve their *finding out* instead of being merely told about things".[2] The main question is not "the teaching of this or that science, but of giving *training* in the A B C of *scientific method*, of making all education scientific; with the object of putting thinking heads on the shoulders of the rising generation".[3]

Thirty or forty years ago, such practical Science as was attempted in the few school laboratories then existing was of no appreciable value; the teaching was confined mainly to the lecture-room. This

[1] It is singular how the notion still prevails that the problem in connection with the rusting of iron is to discover the *cause* of the rusting. So far as I know, Professor Armstrong has never set such an impossible problem to elementary pupils. His suggested problem is to find the *general conditions* under which iron rusts. (Those interested in this particular matter, apart from the teaching question, may refer to a recent investigation by Dr Newton Friend, published in the *Journal of the Iron and Steel Institute*.)

[2] *The Teaching of Scientific Method*, p. 236. [3] *ib.* p. 33.

"teaching" had certain well-marked characteristics: all facts were told; principles were stated, and then—occasionally—verified; and the reasoning, so far as reasoning was employed at all, was entirely deductive. The lessons took the form of *lectures*; the teacher talked, and the boys—sometimes—listened.[1] Of scientific method there was none whatever.

Professor Armstrong was one of the first to point out the necessity for a radical alteration of method. From the very outset his plea was for a method that would involve sustained intellectual effort on the part of the pupils; he urged that passive observation and didactic statement should be replaced by active observation and original investigation. And thus the old order of things began gradually to give place to the new.

"Young scholars cannot be expected", says Professor Armstrong, "to find out everything for themselves, but the facts must always be so presented to them that the process by which results are obtained is made sufficiently clear as well as the methods by which any conclusions based on the facts are deduced."

The first essential step in an experiment is "to have a clear conception of the nature of the quest in which it is proposed to engage. When the motive is clear, some clue must be sought for and followed up."[2]

The rusting of iron investigation is not, of course, the first task which the teacher of Chemistry will set to the beginner, as is so often stated to be the case. In point of fact, the sequence actually suggested is as follows: (1) Lessons on common objects and common substances; (2) lessons on measurement; (3) studies of the effect of heat on substances; (4) the problem stage,—the rusting of iron, the burning of chalk, the action of acids on oxides, &c. Quantitative work, almost from the first, enters largely into the teaching, and data are gradually accumulated which lead, gradually, to the enunciation of theoretical principles.

"The essential feature in the Chemistry scheme is that students

[1] The writer received his first Science lesson, at the age of ten, from the local County Analyst, who was engaged by the School Authorities as a visiting master to teach Chemistry. There was no laboratory available, but there was a well-fitted lecture-room, and in later lessons a few gases were prepared. But the first lesson, which extended over an hour, was frankly a lecture on the atomic theory. No experiments whatever were performed, but the formulæ and equations which covered the blackboard greatly impressed at least one small boy.

[2] Professor Armstrong suggests that teachers should master Baden-Powell's *Aids to Scouting*, and read detective stories. Of the latter it is preferable to read Poe's stories (for example, *The murders in the Rue Morgue* and *The mystery of Marie Roget*), than to read those of Poe's English imitators.

are to be set to work to *solve problems* experimentally." "Quantitative experiments are introduced at the outset and are insisted on as all-important." "Each student receives a paper of instructions, which are advisedly made as bare as possible, so as to lead him to find out for himself how to set to work." But "in teaching children to experiment, a teacher must exercise extraordinary self-restraint in withholding information; however slowly the subject may develop, it must be allowed to develop solely on the basis of the facts established in the course of the inquiry taken in conjunction with common knowledge".[1] It is the function of the teacher to guide, not to tell; he should never help a boy over a stile until the boy has had at least one good try to get over himself.

Dr. Alexander Smith, Professor of Chemistry in the University of Chicago, is one of the recognized authorities on scientific method in America. His sympathies are wholly on the side of the heuristic method, though, as might be expected in any man with an original and independent mind, he has his own opinion over many details. His general views are fairly represented by the following extracts:—

There are "parts of the subject to which the heuristic method is not applicable. If, for instance, we suggest that a pupil should discover the fundamental laws of the subject for himself, we are putting upon him an impossible task. Verification is the term more applicable to work in this direction."

In Professor Armstrong's first problem, "pupils must not only find out for themselves, but as far as possible be led to imitate the detective's method and find out *how* to find out for themselves". "It is evident that the nature of the directions will have much to do with the attitude of the pupil towards his work. In the ideal application of this method, however, no book and no directions are used."

"While work exclusively on heuristic lines does not furnish the knowledge of Chemistry which is expected in Secondary Schools, it is evident that the attitude is one to be cultivated.—Practically, the effort will be to include as much heuristic work as possible in the school course."[2]

Another well-known American writer on Scientific Method is

[1] *The Teaching of Scientific Method*, pp. 206, 241; see also pp. 300-66 ("The British Association Course").

[2] Alexander Smith, *The Teaching of Chemistry*, p 100, &c. See the same writer's *Laboratory Outline of General Chemistry*, a book which is full of suggestive hints for dealing with laboratory problems. The first three chapters of his *Introduction to Inorganic Chemistry* should also be read.

Dr. E. H. Hall, Professor of Physics in Harvard University. His views of the heuristic method will be gathered from the following:—

"What are the conditions of success? A very competent teacher who knows the ground thoroughly, and will not delude himself or his pupils with exaggerated notions of their independence and originality in Science; and a very small class. The method sometimes advocated of teaching children to swim by throwing them into deep water, will surely be fatal to a very large proportion of the unhappy youngsters unless there is some experienced person with every group of three or four. Usually I am sure that the teacher who thinks to let his pupils 'find out everything for themselves' will find out for himself that he has somehow got the hardest part of the undertaking. For visible progress must be made, tangible results must be reached."

"I would keep the pupils just enough in the dark as to the possible outcome of his experiment, just enough in the attitude of discovery, to leave him unprejudiced in his observations; and then I would insist that his inferences, so far as they profess to be derived from his own seeing, must agree with the record, previously made, of these observations."[1]

When due allowance is made for the essential difference of treatment that must necessarily be made in the teaching of many sections of Physics, as compared with Chemistry, it is quite obvious, from Professor Hall's own work as practised at Harvard, that the difference between himself and Professor Armstrong is mainly one of words. (A specimen of his laboratory instructions is given on p. 374.)

Professor Hall is unquestionably right when he says that the heuristic method presupposes a very competent teacher. In the hands of an unskilled teacher the method is almost certain to result in disaster.

It is now generally conceded that the introduction of the heuristic method has led to an enormous improvement in the teaching of the elementary stages of Science. But in the more advanced stages it would seem that the method has still to be worked out. In those schools where the standard of attainments in Science is fairly high, dogmatic teaching is, not infrequently, allowed gradually to supersede the heuristic teaching of the Lower and Middle Forms.

There is no cast-iron about the method. It is not hemmed in by a body of inelastic rules. In essence it implies simply an orienta

[1] *The Teaching of Physics*, p. 278, &c.

tion of mind towards the subject in hand. That orientation secured, the details are of relatively little importance. But some teachers misconceive the real intention of the method, and seem to think that Professor Armstrong would regard them as guilty of a particularly heinous crime if, for example, they ever took their pupils into the lecture-room. It is true that many successful teachers now dispense almost entirely with formal lecture-table work, and instead make use of the first and last quarter hours of each laboratory period. This answers very well with elementary pupils, but, as the work progresses, occasional hours at the demonstration-table become almost indispensable; for the teacher must find time for generalization, for formally establishing laws, for performing such experiments as are beyond the pupils' skill, for going over difficulties, for driving home fundamental principles; and so forth. All this is, however, merely supplementary to the laboratory work. The term "lecture-room" is responsible for much misconception of the proper function of Science teaching.

§ 2. Laboratory "Instructions"

The Laboratory "instructions" or "directions" provided by a Science teacher for his pupils always afford a clue to the worth of his work. If the instructions contain too little information, the pupils' investigation necessarily comes to a stop or proceeds on a wrong track; if too much, the work tends to degenerate into mechanical routine. The form of the instructions must always depend upon the exact stage of the pupils' previous knowledge, though the particular form in which a series of problems are cast will depend much upon the pupils' general intelligence and their previous and collateral training. The following sets of instructions should be carefully compared.

1. *Dr. W. F. Ganong*, Professor of Botany in Smith College (Mass.), says that his first object in teaching is to form the scientific instinct,—the habit of observation, comparison, and experiment. He presents to his class every new topic in the form of "a problem so arranged as to be solved through proper inductive processes by the pupils' own efforts". These problems, which form a series of original investigations, are introduced by questions asked in a form to direct attention to the leading facts and phases of the subject.—Here is his first paper[1] of instructions:—

[1] Ganong, *The Teaching Botanist*, pp. 161-2.

I. *a.* Study the outside of the dry Lima Beans; compare several specimens, and observe what features are common to all and what are individual; minutely observe:—

(1) What is the typical shape?
(2) What is the colour?
(3) What markings have they?

Answer, as far as possible, by drawings made twice the natural size; add notes to describe features which drawing cannot express.

b. Remove the coatings from soaked seeds.

(1) What effect has the soaking had upon the markings, size, and shape?
(2) How many coats are there?
(3) Do the external markings bear any relation to the structures inside?
(4) What shapes have the structures inside, and how are they connected with one another?

Answer, as before, by drawings and notes.

II. Study fully in the same way the Horse Bean.

III. Describe the *resemblances* and the *differences* of the Lima and the Horse Beans.

Two two-hour periods are required for this investigation; three, if possible.

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2. The following is taken from the List of Exercises in Physics, by *Professor Hall*, required of Candidates for admission to Harvard.

Friction between Solid Bodies

Apparatus: a spring balance of about 250 gm. capacity; a rectangular wood-block about 8 cm. × 8 cm. × 4 cm.; a smooth sheet of paper about 18″ × 12″.

(1) *First consider the velocity of the motion; that is, ask whether the force required to keep up a slow steady motion is greater or less than that required to keep up a more rapid steady motion.*

Lay the block upon one of its broad sides, and attach it to the spring balance by a thread passing around but not under the block. Load the block with weights until the force required to maintain a slow steady motion is about 3 oz. Draw the block parallel to its

grain along the sheet of paper several times with a very slow steady motion, and then several times with an equally steady motion two or three times as fast. (As the paper is likely to grow somewhat smoother under the repeated rubbing, do not make all the slow trials first, but change from slow to fast and fast to slow a number of times.)

Record your conclusion as to whether the slow or the more rapid motion requires the greater force.

(2) *Next try to find out whether, the total weight being the same as before, it is easier or harder to draw the block on a narrower side than on a broad side.*

Use the same block and the same load of weights, pulling it now, as before, parallel to its grain. (The sides of the block must always be clean, and the broad and narrow sides as equally smooth as possible.)

Record your conclusion as to whether the broad side or the narrow side offers the greater resistance to the motion.

(3) *Finally, ask what connection there is between the total mass drawn and the force required to draw it.*

For this purpose, vary the weights placed upon the block, using not less than 6 oz. for the least, and as much as 16 oz. for the greatest load.

Add to the load in each case the weight of the block itself, and make the record in the following form, W being the load, and b the weight of the block:—

$W + b$.	F (Force required).
.....	.....
.....	.....
.....	.....
.....	.....

Look for any simple relation between $(W + b)$ and F.

(In the next part of the investigation, the block was made to slide down a sloping board covered with a sheet of paper, arrangements being made for varying the steepness of the board.)

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3. *Professor Alexander Smith* quotes the following instructions from "a well-known laboratory outline": "Treat a few small crystals of potassium iodide with concentrated sulphuric acid. What do you notice? Compare with the results obtained when potassium bromide and sodium chloride are treated in a similar way." This

brief statement constitutes the whole of the directions for the investigation, and Professor Smith has found by experience that it is hopelessly inadequate. Without further instructions in regard to the materials used and the general method of setting to work, anything like uniformity of results cannot possibly be expected; and no elementary student is ever likely to realize without a good deal of guidance that there are several distinct products concerned, though much will depend, of course, upon what he already knows about HCl, H_2S, and SO_2.

Professor Smith's own directions for the investigation are as follows:—

Place about a gram of potassium iodide in a test-tube, and moisten it with concentrated sulphuric acid. Warm, if necessary. Investigate the result as follows:—

(1) Breathe across the mouth of the test-tube to ascertain the effect of the gas on moist air. What gas previously made showed the same behaviour? Remembering the similarity between the halogens and between their corresponding compounds, what do you infer in this case? To confirm this conclusion, lower a glass rod dipped in ammonia hydrate into the test-tube; also a strip of filter paper dipped in lead nitrate solution [R].[1]

(2) What is the colour of the gas, or any part of it? What is the coloured body?[2] Was there any corresponding product when sulphuric acid acted on a chloride? By what kind of chemical action could this coloured substance be formed from the one identified in (1)?

(3) Study the odour of the gas and describe it. Was there any effect on the lead nitrate which remained unexplained in (1)? Can you now explain it [R]?[3]

The work in (1) and (2) leads to the recognition of two distinct gaseous products. That in (3) will yield one, and perhaps two others. Still another distinct solid product may be observed on the walls of the tube. Construct separate equations representing the formation of the first product from the original materials, and of each of the others from this product and sulphuric acid. What two properties of sulphuric acid and what property of hydrogen iodide are illustrated by this set of experiments?[4]

[1] [R] indicates that the pupil needs information he cannot have gained in previous work, and must therefore *refer* to some book or to his teacher. Here he is ignorant of the action of iodides on lead salts.

[2] This assumes that Iodine has been handled before.

[3] This assumes that hydrogen sulphide has not yet been studied.

[4] *The Teaching of Chemistry and Physics*, pp. 102-4.

4. We next give a paper of directions, for the comparative study of lead and silver, by *Professor Armstrong*. "The experiments are chosen so as to afford an insight into the principles of the methods ordinarily employed in qualitative and quantitative analyses."[1]

Comparative Study of Silver and Lead[2]

Silver.—*Symbol*, Ag (*Argentum*). *Atomic weight*, 107·67. *Specific heat*, ·05701.

Lead.—*Symbol*, Pb (*Plumbum*). *Atomic weight*, 206·47. *Specific heat*, ·03140.

(1) Determine the relative density of lead and silver at a known temperature by weighing in air and in water.

(2) Separately heat known weights of lead and silver for some time in the air, allow to cool, then weigh.

(3) Separately convert known weights of lead and silver into nitrates; weigh the latter. From the data thus obtained, calculate the *equivalents* of lead and silver.

(4) Convert the known weights of nitrates thus obtained into chlorides; weigh the latter.

(5) Compare the action on lead and silver of chlorhydric acid; of dilute and concentrated sulphuric acid, using the acid both cold and hot; and of cold and hot nitric acid.

(6) Using solutions of the nitrates, compare their behaviour with chlorhydric and sulphuric acids, hydrogen sulphide, potassium iodide, and potassium chromate. Ascertain the behaviour of the precipitate formed by chlorhydric acid when boiled with water and when treated with ammonia solution.

(7) Compare the behaviour of lead and silver compounds on charcoal before the blowpipe.

(8) Tabulate the results of your experiments with lead and silver in parallel columns.

(9) Ascertain whether the substances given you contain lead or silver.

(10) Determine silver in an alloy of lead and silver by cupellation.

(11) Study the method of determining silver volumetrically by means of a *standard solution* of ammonium thiocyanate. Determine the percentage of silver in English silver coinage.

(12) Determine silver as chloride by precipitation.

[1] *The Teaching of Scientific Method*, p. 230. [2] *ib.* pp. 233–4.

(13) Dissolve a known weight of lead in nitric acid, precipitate it as sulphate, collect and weigh the latter.

(14) What are the chief ores of lead and silver? How are lead and silver extracted from their ores? How is silver separated from lead? How is it separated from burnt Spanish pyrites? What are the chief properties and uses of lead and of silver? State the composition of the chief alloys of lead and silver.[1]

The stage in the students' course of work when this investigation would be given is pretty obvious.—It is quite clear that during such an investigation the average student would constantly experience doubt and difficulty. The *skilled* teacher would, in such circumstances, always give him a bare minimum of information,—would indeed give him a mere hint, or perhaps ask a leading question, rather than give him information of a direct kind.

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5. Mr. J. B. Russell, in his book *Notes on the Teaching of Elementary Chemistry*, provides us with numerous examples of admirably worked out laboratory instructions. The one reproduced here is, as will be seen, intended for young beginners:—

Some Effects of Heat on Substances

Heat, one at a time, each of the substances named in the list as directed, and try to see all that happens.

Mercury.	Soda.	Lead nitrate.
Nitre.	Borax	Sal-ammoniac.
Iodine.	Red lead.	Blue Vitriol.

(1) Describe the substance in such a way that a person after reading your description might readily pick out the right substance from among the others.

(2) Place on a strip of paper, bent in the form of a shoot, about sufficient of the substance to cover a shilling, and introduce it into a clean dry test-tube without soiling the sides.

(3) Heat the tube at first gently and then more strongly. Watch *all* that happens. Allow to cool and examine the residue.

[1] Of course an investigation of this kind requires a considerable amount of time. But it not infrequently happens that time is badly economized in a chemical laboratory, pupils standing idle during such processes as filtering, drying, &c. A well-taught pupil can often keep two or three different operations going at the same time.

(4) Immediately after each experiment, write down an account of all that you have observed, and in addition answer the following questions:—

(i) Does the substance left in the tube appear to you to be the same as, or different from, the original substance?

(ii) Does any substance appear to leave the tube?—if so, describe it.

(iii) From which of these substances are *two*, or more, distinct substances obtainable?

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6. Finally, we give an example of laboratory directions of a totally different kind. It is taken from a textbook in common use.

SOME PROPERTIES OF BROMINE

(1) Notice the offensive odour of bromine, taking care not to expose the eyes to the vapour, and only to smell it when freely diluted with air.

(2) Place a cork in a bottle containing bromine, and observe that the cork is rapidly destroyed.

(3) Put a drop of bromine in some water. Notice that it sinks, partly dissolving and giving a yellow solution.

(4) Cool some bromine water to 4°. Crystals having the composition $Br_2,10H_2O$?, will form.

(5) Add a drop of bromine water to solution of iodide of potassium containing starch. Iodine will be liberated and form blue iodide of starch: $2KI + Br_2 = 2KBr + I_2$.

(6) Pass hydrogen through a U-tube containing fragments of pumice-stone soaked with bromine, and provided with a jet; ignite the mixture which escapes. Clouds of dense colourless acid fumes, resembling those of damp hydrogen chloride, will testify to the formation of an acid fuming gas. This is hydrogen bromide (HBr).

(7) Place some Dutch gold in bromine. The metal will combine with the bromine, but much less readily than with chlorine.

(8) Place a strip of Turkey-red twill in some bromine water. It will be bleached much less rapidly than when chlorine is employed.

With such instructions as these, pupils would engage in mere mechanical routine; for the instructions give away the whole case

and the pupils know exactly what is going to happen, or at any rate what is supposed to happen, before they perform an experiment. Work of this kind is of no appreciable value whatever, and is admirably designed to make the hostile humanist scoff at the "intellectual claims of science".

However good the instructions may be, the work will never proceed successfully unless the teacher is constantly in touch with every pupil. Unexpected difficulties are bound to arise, and different difficulties with different pupils. It must be borne in mind that, without frequent suggestions and warnings from the teacher, the average pupil is seldom capable of giving, with any degree of precision and accuracy, concrete interpretation to laboratory instructions, however carefully these may be drawn up.

§ 3. The Pupil's Notebook

An essential part of the training in the laboratory is the systematic writing up of notes. These notes should be a faithful record, in the pupils' own words, of all that is done, and should be written out in ink as the work proceeds.

Except when really difficult points of theory are being dealt with—points requiring great precision of expression—notes should never be dictated. A Science teacher who dictates notes advertises his own incompetence. In Professor Armstrong's words, the dictation of notes by a teacher should be regarded as a criminal offence.[1]

"As an incident to the writing, the pupil usually finds that his thoughts on the subject were not so perfectly organized as he had supposed. In framing written answers to the questions in the laboratory instructions, he is stimulated to group the facts in new ways, and is assisted in studying the subject by the discovery of gaps in his thoughts and in his observations."[2]

The exact recording of laboratory notes affords "training in preciseness and proportion in exposition of original data", and "imposes direction, definiteness, and completeness in observation". "It enables the teacher to make sure that the student has actually and fully worked out his topics."

"It is only when the motive is clearly written out that it is clearly understood,—that the meaning or intention of the experi-

[1] *The Teaching of Scientific Method*, p. 265; see also p. 266.
[2] Alex. Smith, *The Teaching of Chemistry*, p. 125.

ment is fully grasped, and this is equally true of the result." "The thing to be impressed on the pupils is that a record should tell a plain tale to people who are not present when the record is made."[1]

The correction of the notes is a very important part of the teacher's duties, but such correction sometimes becomes a most grievous burden. The burden is often much heavier than it need be. If the notes are written out, as they certainly should be, during the actual progress of the work, all mistakes can be pointed out at the bench; for the teacher can quite easily arrange to glance through the notes last made,—never more than a few lines,—every time he goes to the pupil to see what progress he is making. But the teacher himself ought not, as a rule, to make the correction. He should have a code of symbols for indicating different types of mistakes, and all corrections should be made by the pupil himself. The pupil's own act of correction necessarily calls forth his sustained attention, much more forcibly than any correction by the teacher. But of course the notebooks require a thorough examination periodically, say two or three times a Term.

§ 4. Manipulation

Manipulative skill comes only with much practice, and those Science teachers whose laboratory training has been slight, should spare no efforts to acquire a sound knowledge of laboratory practice and of laboratory arts. Want of acquired manipulative skill too often leads to an evasion of the most telling form of laboratory instruction, and to courses of work which are entirely unworthy of Upper Form boys. If a Science teacher is to do justice to his professional work, anything less than two or three years of constant laboratory practice, under the direction of a professor of recognized eminence, is of little use.

Those teachers who have not had this good fortune, and who cannot even now find an opportunity of undergoing the necessary training in Institutions of University rank, should, by dint of much practice and by means of all the help they can obtain from text books written by men who are recognized authorities in their own departments, add to their practical knowledge and skill. There are several excellent modern books now obtainable, written specially for the solitary worker, who, however, should obtain, if possible, Faraday's *Chemical Manipulation*. This book, although written as far

[1] Hall, *The Teaching of Physics*, p. 216, &c.

back as 1828, is still a veritable gold-mine for all students of practical science, for in its 650 pages are to be found minute practical instructions for all sorts of experiments,—instructions which in many cases have never been improved upon.

The simplicity of the means by which Faraday made his experiments is often astonishing, and he took a keen delight in showing how easily apparatus might be extemporized. He once exhibited to a large audience all the rudimentary experiments in statical electricity by using an electrical machine which he thus improvised at a few minutes' notice. He inverted a four-legged stool to serve for a stand, and took a white glass-bottle for the cylinder. A cork was fastened into the mouth of this bottle, and a bung was fastened with sealing-wax to the other end. Into the cork was inserted a handle for rotating this bottle, and in the centre of the bung was a wooden pivot on which it turned; while with some stout wire he made crutches on two of the legs of the stool, for the axles of this glass cylinder to work upon. The silk rubber he held in his hand. A japanned tea canister resting on a glass tumbler formed the conductor, and the collector was the head of a toasting-fork.[1]

To the uninitiated all this may seem very simple, but such ingenuity comes only of prolonged practical experience; it is not innate in the novice.

CHAPTER XLI

Instances of Investigation Attempted by Pupils

The circumstances in which the following problems were set are stated in each case. Strictly speaking, perhaps, neither the term "investigation" nor the term "research" is applicable to the instances given, as in no case was the pupil wholly ignorant of the particular matter he had to take in hand. But then this is usually the case in school work.[2]

[1] See J. H. Gladstone's *Life of Faraday*, and G. Iles' *Inventors at Work*.

[2] In order to facilitate the following of the arguments in these examples, some slight alterations have been made in the English and in the numbering of the sections. But the actual matter and the reasoning are entirely unchanged, and are the pupils' own. Most of the problems were given by the author, a good many years ago, to his own pupils. He is naturally debarred from making use of examples drawn from schools with which he is now officially associated.

§ 1. An Instance from English Grammar

The following was given to an Upper Form during the slack week between the annual examination and the holidays. Books of reference were allowed.

"Distinguish between the different kinds of 'connective' words in English grammar, and show clearly the difference between adverbs and conjunctions. Use the following words for purposes of illustration, making up sentences showing their ordinary, and not any exceptional, usage: *accordingly, because, before, consequently, lest, since, then, there, therefore, until, when, where, which, while, who, why.* Frame definitions of an adverb and a conjunction, in accordance with the functions you consider the words actually discharge."

Here is one of the more successful results:—

The following sentences show the different possible uses of the given words:—

1. I received a pressing invitation to go; *accordingly* I went.
2. He will succeed *because* he is trusted.
3*a*. He is standing *before* the fire.
3*b*. I have heard that report *before*.
3*c*. Do not go *before* I come.
4. My pocket has been picked; *consequently* I have no money.
5. I will not make a noise *lest* I disturb you.
6*a*. I have not seen him *since*.
6*b*. He has been here twice *since* Tuesday.
6*c*. He has not smiled *since* his son died.
6*d*. I must believe it *since* you say so.
7. *Then* the clock struck.
8. He was standing *there* for an hour.
9. He has been idle; *therefore* he must be punished.
10. He did not speak *until* the people became silent.
11*a*. *When* will you start?
11*b*. You were out *when* I called.
12*a*. *Where* shall we go?
12*b*. Show me the shop *where* you bought the book.
13*a*. *Which* do you prefer?
13*b*. I received the book *which* you sent me.
14. Hold the horse *while* I put on my gloves.
15*a*. *Who* called this morning?

15*b*. I asked him *who* called this morning.
16*a*. *Why* did you strike the dog?
16*b*. He asked me *why* I struck the dog.

These sentences may be grouped, according to the grammatical meaning of the words, as follows:—

3*a*. He is standing *before* the fire.
6*b*. He has been here twice *since* Tuesday.

In these sentences, *before* and *since* are prepositions.

3*b*. I have heard that report *before*.
6*a*. I have not seen him *since*.

In these sentences, *before* and *since* are simple adverbs, modifying the verbs *heard* and *seen*, respectively.

1. I received a pressing invitation to go; *accordingly* I went.
4. My pocket has been picked; *consequently* I have no money.
9. He has been idle; *therefore* he must be punished.

In these sentences, *accordingly*, *consequently*, and *therefore*, are simple adverbs. *Therefore*, for example, is the exact grammatical equivalent of *for that reason*, and evidently modifies the verb *punished*. "He has been idle" and "he must be punished" are, grammatically, two complete and independent sentences. There is a continuity *in thought*, but no *grammatical* connection. Hence the grammatical function of *therefore* is in no sense conjunctive but only adverbial. It is wrong to class it either amongst the conjunctions or amongst the conjunctive adverbs.

13*a*. *Which* do you prefer?
15*a*. *Who* called this morning?

Which and *who* are here interrogative pronouns.

13*b*. I received the book *which* you sent me.
15*b*. I asked him *who* called this morning.

Here, *which* and *who* perform two functions: (1) the function of a *pronoun*, as in 13*a* and 15*a*; (2) the function of a *connective* between two sentences. As, however, their primary function is that of a pronoun, we call them *connective pronouns*, instead of *pronominal connectives*. They are also called *relative pronouns*. "Which

you sent me" is the equivalent of a descriptive adjective attached to *book*; "who called this morning" is the equivalent of a noun, the object of *asked* (cf. the sentence "I asked him a *question*").

7. *Then* the clock struck.
8. He was standing *there* for an hour.
11*a*. *When* will you start?
12*a*. *Where* shall we go?
16*a*. *Why* did you strike the dog?

Then, *there*, *when*, *where*, and *why* are simple adverbs, limiting the action of the verbs in the respective sentences; *then* and *when* are adverbs of *time*; *there* and *where*, of *place*; *why*, of *reason*.

11*b*. You were out *when* I called.
12*b*. Show me the shop *where* you bought the book.
16*b*. He asked me *why* I struck the dog.

In these cases, *when*, *where*, and *why* perform the function of an adverb exactly as in 11*a*, 12*a*, and 16*a*; they modify the verbs in the subordinate sentences they introduce. But they perform a second function; they join a subordinate to a principal sentence, i.e. they are *connectives*. But their primary function is that of an adverb, and we therefore call them *connective adverbs* or *conjunctive adverbs*, not adverbial conjunctions. They might be called *relative* adverbs, just as connective pronouns are called relative pronouns, for they have an antecedent, expressed or implied. (You were out *then* [= at the time] when I called.)

In 11*b* the adverb *when* modifies the verb *called* in the subordinate clause it introduces, and the subordinate clause itself (*when I called*) has the force of an adverb with respect to the principal sentence. In 12*b*, *where you bought the book* is the equivalent of an adjective qualifying the noun *shop*. In 16*b*, *why I struck the dog* is the equivalent of a noun, the object of the verb *asked*.

2. He will succeed *because* he is trusted.
5. I will not make a noise *lest* I disturb you.
6*c*. He has not smiled *since* his son died.
6*d*. I must believe it *since* you say so.

10. He did not speak *until* the people became silent.
14. Hold the horse *while* I put on my gloves.
3*c*. Do not go *before* I come.

In these sentences, *because*, *lest*, *since*, *until*, *while*, and *before* are conjunctions (of cause, purpose, and time). They are not adverbs; they do not modify the meaning of any word in the sentences containing them; their function is to connect dependent sentences to principal sentences; and *these dependent sentences have the force of adverbs* limiting the action of the verbs in the respective principal sentences. In 2, for instance, *because* does not by itself modify the verb *is trusted*; but the clause *because he is trusted* is an adverbial clause modifying the verb *will succeed*.

There are thus three sorts of words connecting sentences together:—

1. Connective or relative *pronouns*.
2. Conjunctive *adverbs*.
3. *Conjunctions*.

We must therefore define *conjunctions* as *connective words which have neither a pronominal nor an adverbial signification*. They usually join sentences together, but the conjunction *and* sometimes joins words only, as in the sentence, "The school is between the house *and* the church".

Adverbs (1) add to the meaning, and (2) limit the application of, the word to which they are attached. *Rains heavily* means all that *rains* means, and *heavily* as well; the adverb thus adds to the meaning of the verb. *Heavily* also limits the application of the verb *rains*; for *rains* applies to all occasions, *rains heavily* to fewer occasions.

An adverb may in this way be attached to—

(1) A verb: she walks *slowly*.
(2) An adjective: her pace is *too* slow.
(3) An adverb: she walks *very* slowly.
(4) A preposition: she found her purse *just* outside the door.

(Here, *just* is attached to the preposition *outside*, rather than to the whole phrase *outside the door*.)

(5) A conjunction: she has done her best *ever* since she went away.

(Here, the adverb is attached more closely to the conjunction *since* than to the whole sentence *since she went away.*)

(6) A complete sentence: *Evidently*, she knew her way about.

Thus an adverb may be defined as *a word which adds to the meaning and limits the application of verbs, adjectives, adverbs, prepositions, conjunctions, and complete sentences.* Adverbs may be (1) simple, (2) conjunctive. Both kinds may be classified according to their meaning, —time, place, manner, degree, &c.

For good or for ill, English grammar occupies a much less prominent place in the curriculum than when this exercise was attempted, and the exercise would no doubt now be regarded as a waste of time. Nevertheless the result certainly does show a nice appreciation of the function of words. There may be differences of opinion about the boy's conclusions, but that does not matter.

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§ 2. An Instance from the Art Room

The following was given to a Sixth Form boy who, a year or two later, won an open mathematical scholarship:—

"The accompanying figure (fig. 12) represents the usual perspective drawing of a cylinder standing on its base on the ground, a few feet from the observer. It is sometimes said that the original of the major axis XY of the ellipse is a diameter of the circle forming the top of the cylinder. Is this right? Investigate the mathematical relations generally."

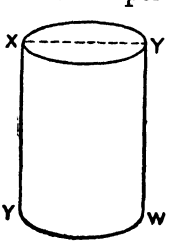

Fig. 12

Let AEBF (fig. 13) be the plan of the cylinder, D the position of the observer, DE and DF the plans of two tangent planes drawn from the observer to touch the cylindrical surface, and RJLS the picture plane. It is evident that the line EF is

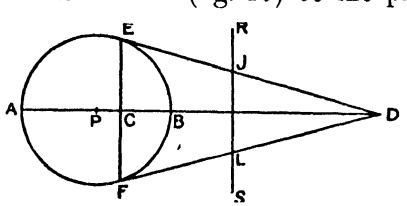

Fig. 13

the original of the major axis J L of the ellipse. (Any other line in the circle, parallel to E F, even a diameter through P, would when projected on the picture plane, be represented by a line shorter than J L. This is evident from the figure.)

Now E F is the polar of the point D with respect to the circle, or C and D are inverse points with respect to the circle.

∴ P C.P D = P B², or A, C, B and D form a harmonic range. Take a vertical plane through A D, i.e. through the diameter of the cylinder and the observer (fig. 14). Let Y represent the observer's eye, R'G H S' the picture plane, Y A, Y C, Y B rays from the observer's eye to A, C, and B, and Y D a vertical through the observer. A M N B is a medial section of the cylinder.

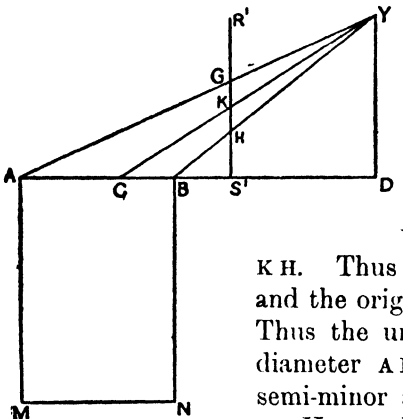

Fig. 14

Now Y.A C B D is a harmonic pencil. And since G H is a transversal parallel to the ray Y D of the pencil, G H must be bisected by Y C, the conjugate of Y D, i.e. G K = K H. Thus the minor axis G H is bisected at K, and the original of G K is A C, and of K H is C B. Thus the unequal sections A C and C B of the diameter A B of the circle become the equal semi-minor axes of the ellipse.

Hence the major axis of the ellipse is the projected polar E F, and the semi-minor axes are the projected unequal sections of the diameter A B. In other words, the lines A B and E F in the plan are the originals of the minor and major axes respectively, and their point of intersection C is the original of the intersection of the elliptic axes. Also the lines X V and Y W in the perspective sketch are the lines of contact of the tangent planes (from the eye) and the cylinder. It is evident that the portion of the cylindrical surface seen by the observer is less than that portion unseen, i.e. the portion beyond the lines of contact of the tangent planes from the eye.

This was the result of twenty-five minutes' work. The boy had been working problems in harmonic division, and the solution gave him little trouble. But he had, at the outset, entirely wrong notions of the perspective projection of a circle. This appeared to be due to the fact that when he had been taught to draw the cylinder, he had been told that the original of the point of intersection of the elliptic

axes was the centre of the circle. The same boy afterwards investigated the more difficult case of the cylinder lying on the ground, oblique to the picture plane. He did not at first see that precisely the same principle applies as in the former case. His solution, though correct, was rather clumsy, and is not worth reproducing.

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§ 3. An Instance from Botany

The pupil, a girl of sixteen, knew already that warmth and moisture are necessary for the germination of seeds. She knew how to prepare oxygen and carbon dioxide, and had some knowledge of the properties of these gases; she knew that alkaline pyrogallol[1] absorbed oxygen, that caustic potash absorbed carbon dioxide, and knew the lime-water test for carbon dioxide. She was told that a living plant sometimes takes in and sometimes gives out oxygen, and sometimes takes in and sometimes gives out carbon dioxide. She was also told that when one was given out the other was usually taken in.—The problem set was to discover the process that went on in germinating seeds.

The laboratory instructions were based upon Professor Ganong's experimental course on Respiration, and his U-tube form of respiroscope was used. This consists of a common U-tube, inverted; one end is closed with a cork on which is placed some wet sphagnum to support the germinating seeds. The open end dips into a tall bottle, the tube fitting into the neck sufficiently closely to prevent any appreciable chemical action on the part of the atmosphere on the reagent in the bottle, but not so close as to interfere with the atmospheric pressure on the surface of the reagent. The seeds used were oats which had already been allowed to begin to germinate, the radicles being from $\frac{1}{8}$ to $\frac{3}{8}$ in. long.

The girl had been previously shown how to use the respiroscope.

1. I removed the cork from the mouth of a bottle in which some oat seeds had been germinating, and plunged a lighted taper into the bottle. The taper was immediately extinguished. This may have been due to CO_2 given off from the seeds; or, if the O had been absorbed by the seeds, to the N of the air; or to both these things; or to the presence of some other gas that does not support

[1] She was provided with a concentrated mixture of pyrogallic acid and caustic potash.

combustion. The conclusion from the experiment is therefore uncertain.

2. I put some oat seeds in the respiroscope, the open end of which I placed in a tall bottle of KHO solution; and I put the apparatus on one side for examination two or three days later. On examination, I found that the seeds had grown considerably, and that the KHO solution had risen in the respiroscope about $\frac{1}{8}$ of its length. On a further examination, two days still later, the KHO had risen in the respiroscope about $\frac{1}{7}$ of its length, and the seeds had grown still more.

The conclusion is that the KHO rose to take the place of a portion of the air that had been used up by the seeds, though whether any CO_2 had been given out and absorbed by the KHO, it is not possible to say.

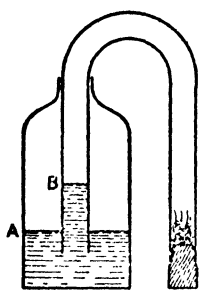

Fig. 15

If the KHO had risen $\frac{1}{5}$ the length of the respiroscope, I should have felt pretty certain that O had been used up. And it does seem certain that more of the air was used up than $\frac{1}{7}$, seeing that the rise of the KHO cannot correctly represent the total amount of what seems to have disappeared. For the pressure at the level A (fig. 15) within the bottle is the pressure of the atmosphere, and therefore the pressure at the same level A within the arm of the respiroscope is also equal to that of the atmosphere; but within the respiroscope this pressure is represented by a column of KHO (between the levels A and B), in addition to a certain pressure due to the enclosed gas. The density of this gas must therefore be less than that of the atmosphere; and the higher the KHO rises in the respiroscope, the less dense must the enclosed gas become. This makes me think that although only about $\frac{1}{7}$ *appears* to be absorbed, perhaps as much as $\frac{1}{5}$ has *really* been absorbed. If this is so, there can be hardly any doubt that the oxygen has been used up.

3. I had arranged a similar respiroscope with germinating seeds, but with its open arm placed in water instead of in KHO. The seeds germinated almost as well as they did before, but the water within the tube remained at just about the same level as at the start, all the time. As in this case the liquid does not rise, some gas must be given out as well as some gas taken in by the seeds. From the last experiment we are now bound to conclude that the gas given out was CO_2, and that it must have been absorbed by the

KHO, for it is the only gas absorbed by KHO; and the amount of gas used up by the seeds seems to be just about equal to the gas given out by them.

4. In a bottle holding some clear lime-water, I suspended a piece of wire netting supporting some wet blotting paper on which were placed a few germinating seeds. I fixed the stopper of the bottle tightly in, and placed the apparatus in the conservatory. The lime-water became milky after a day or two. This conclusively proved that CO_2 was given out.

But we do not yet know that it was really O that was taken in by the seeds.

5. So I tested the gas remaining in the respiroscope after Experiment 2. No oxygen seemed to be present.

6. I tried to make seeds grow in jars of N, H, and CO_2. There was no growth in either case.

7. I took another respiroscope and placed the open arm in alkaline pyrogallol. In a short time the solution rose up the tube about $\frac{1}{6}$ the length of the tube. This showed that the O had been absorbed. The germinating seeds did not appear now to grow at all. So there seems to be no doubt that O is necessary and is taken in by the germinating seeds.

The general conclusion is that in the germinating of oat seeds, O is used up, and CO_2 is given out; and that the two gases thus taking part in the exchange are about equal in quantity. The action seems to be a good deal like the action of breathing in animals.

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§ 4. An Instance from Chemistry

The work previously done and the stage reached by the pupil responsible for the following, as well as the form given to the laboratory directions provided, may be inferred with sufficient accuracy from the general result. The problem set was to discover the nature of bleaching by bleaching powder. As will be seen, the experimental investigation was followed by a search for additional evidence from the books of the school library.

Jars of H, O, Cl, HCl gas, and Chlorine water provided.

1. The Cl gas easily distinguished by its colour and smell.

2. Breathed across the mouths of the jars of H, O, and Cl. No noticeable result.

3. Lowered a jet of burning H into a jar of Cl. The H burnt with a pale yellow flame. Fumes escaped from the mouth of the jar. These were recognized as HCl.

4. Took two jars, one of H and one of Cl, each covered with a glass plate, and placed them mouth to mouth, plates being then withdrawn. Jars shaken to ensure mixture of gases. Lighted taper applied to one. A sharp explosion occurred. Whitish fumes appeared; recognized as HCl.

5. Covered the mouth of the second jar and exposed to diffused daylight. After a few hours, the greenish colour of the gas had disappeared. Removed plate. Breathed across the mouth of the jar; dense fumes appeared; recognized as HCl.

These three experiments show that *Cl has a powerful affinity for H.*

6. Placed some dry coloured calico in jars of dried H and O, by fastening it to a cork cemented to the under side of the covering glass plate. Colour in each case unchanged.

7. Repeated the last experiment, the jars containing a little water as well as the respective gases. Colour of calico still unchanged, whether calico itself was kept dry or made wet.

8. Exposed coloured calico to HCl gas, also to HCl solution. Colour unchanged.

9. Placed some dry coloured calico in a jar of Cl. Some lumps of $CaCl_2$ had been placed in the jar, to dry the Cl. Colour of calico very slightly changed.

10. Repeated the last experiment, substituting concentrated H_2SO_4 for $CaCl_2$, and first allowing the jar of gas to stand for some hours, to ensure complete drying before inserting the calico. Colour entirely unchanged.

11. Moistened the coloured calico and inserted it in a jar of undried Cl. Calico bleached almost immediately.

These experiments seem to show that neither H, O, HCl, nor Cl alone, has any more power than water to bleach coloured calico; and a similar remark applies to H, O, and HCl acting with water. But *bleaching is immediately effected by the combined action of Cl and* OH_2.

12. With common writing ink, wrote on a piece of printed paper

and inserted paper into a jar of moist Cl. Writing bleached; printing unchanged. As the blackness of the writing ink is said to be due to organic colouring matter, and that of printers' ink to carbon, it seems that Cl and OH_2 acting together can attack organic colouring matter, but not carbon.

13. Filled a long glass tube, sealed at one end, with a solution of Cl and OH_2; inverted it over a shallow vessel of the same solution, and exposed it to sunlight. Bubbles of gas were evolved, and collected at the top of the tube. Liquid gradually lost its colour. The disappearance of the colour suggests the disappearance of the Cl, and as there is nothing present but Cl and OH_2, the gas at the top of the tube must be either H or O; but Cl is known to have a strong affinity for H; the gas is therefore probably O. Tested this gas and proved it to be O. Tested the liquid and found it acid. Acid proved to be HCl, as expected.

This experiment seems to show that in the action of bleaching by Cl and OH_2, the colour is destroyed either by the newly formed O, or by the newly formed HCl. From what I have already learnt of chemistry, I do not see how the latter is possible; and Experiment 6 has shown that ordinary free O can have no effect. But I know that "nascent" gases are said sometimes to be very powerful, and it seems likely, therefore, that the newly formed *atoms* of O, before they have recombined into molecules, are the active agent here. Perhaps, then, the bleaching takes place by the Cl withdrawing the H from the OH_2 and leaving the "atomic" O free to oxidize the coloured compound and so render it colourless.

14. Made some bleaching powder by passing Cl into slaked lime. The following is said to be the reaction:—

$$2Ca(OH)_2 + 2Cl_2 = CaCl_2 + Ca(OCl)_2 + 2H_2O,$$
$$\text{or} \quad Ca(OH)_2 + Cl_2 = Ca(OCl)Cl + H_2O.$$

15. If bleaching powder is a *mixture* of calcium chloride ($CaCl_2$) and calcium hypochlorite ($Ca(OCl)_2$), the $CaCl_2$ ought to be dissolved out by alcohol, in which it is easily soluble. Tried this experiment; shook up some bleaching powder in alcohol, filtered, and evaporated. Scarcely any residue. Therefore the $CaCl_2$ is not soluble, and therefore bleaching powder is not a mixture. This is confirmed by bleaching powder showing little sign of being deliquescent, whereas $CaCl_2$ is strongly deliquescent.

The relation in which bleaching powder stands to $CaCl_2$ on the

one hand, and to $Ca(OCl)_2$ on the other, seems to be indicated by the thick lines in the following diagrams:—

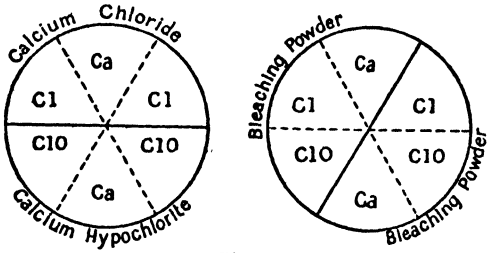

Fig. 16

16. Made a mixture of bleaching powder in water. Added dilute H_2SO_4. Cl evolved. Therefore acid seems to be capable of turning Cl out of bleaching powder.

17. Shook up some bleaching powder in water, and decanted the clear solution. Dipped some coloured calico in the solution, then in dilute acid. The calico was bleached.

The bleaching is apparently due to the combined action of Cl and OH_2. Since Cl is evolved, and since, with H_2SO_4, $CaSO_4$ is certainly precipitated, perhaps the following equation represents the reaction:—

$$Ca(OCl)Cl + H_2SO_4 = CaSO_4 + H_2O + Cl_2.$$

Reference to Books in Library:—

18. The great majority of organic compounds are said to be colourless, and the molecules of such compounds are usually very complex. Hence even a slight chemical change in the molecules of a coloured compound, affecting only one or two of the atoms of the molecule, is almost sure to result in a colourless material. Thus, if Cl unites with the H of water and sets free O, this O will, in "oxidizing" the coloured compound, not "destroy" it, but will very probably be productive of a colourless compound.

19. One book says that *dry* Cl may sometimes bleach, that then the Cl unites with the H of the dye, and so converts the dye into a non-coloured compound. But I have tried a good many coloured fabrics in dry Cl and could get no result worth mentioning.

20. Another book says that the so-called bleaching action of Cl is almost always due to oxidation by hypochlorous acid (HClO). When Cl is dissolved in water, a small part of the Cl interacts with a little of the water:—

$$H_2O + Cl_2 = HClO + HCl.$$

But only traces of HCl and HClO are produced, the change coming quickly to a standstill, the interaction being reversible. The products HCl and HClO interact more vigorously to produce, reversely, Cl and OH_2 again. But when the solution is *exposed to sunlight*, the HClO decomposes, and O is produced. The removal of the HClO prevents the reverse action proceeding, so that the direct action continues, under continued exposure to sunlight, to completion.

21. I took some Cl water that had been standing for a little while in diffused daylight and tested for HClO, by shaking up well with Hg. A precipitate slightly brownish but nearly white was formed. Warmed this precipitate with dilute HCl. The brownish portion seemed to dissolve; the white insoluble precipitate that remained was mercurous chloride. The brownish precipitate that dissolved indicated the presence of HClO, but there was not much of it, and I did not feel very certain. I did not know of any confirmatory test.

22. It seems probable that although O is proved to be the direct agent in the action of bleaching, this O is not released directly by the action of Cl upon the H of H_2O, but is released from HClO, which itself is formed, with HCl, by the action of Cl upon OH_2.

(1) $Cl_2 + H_2O \rightarrow HClO + HCl.$
(2) $HClO \rightarrow HCl + O.$

If this is so, it is not necessary to consider the meaning of "nascent" oxygen, which nobody seems to understand really. It is better to consider the HClO as a powerful oxidizing agent, than to consider that the required energy comes from "atomic" oxygen just released by the action of Cl upon the water molecules.

23. It was shown (in 17) that the action of acid on bleaching powder was to liberate Cl, and an equation was constructed showing that the complete action might be as follows:—

$$Ca(OCl)Cl + H_2SO_4 = CaSO_4 + H_2O + Cl_2.$$

One book states that the bleaching is then brought about by the Cl generated within the fibres of the wet cloth, and there setting free the O from the water. But, from what we have just learned, the real action seems to consist in the acid liberating HClO:—

$$Ca(OCl)Cl + H_2SO_4 = CaSO_4 + HClO + HCl.$$

Most of the HClO now temporarily interacts with the HCl:—

$$HClO + HCl = H_2O + Cl_2.$$

but as a certain portion of the HClO gives up its O, the reverse action is able to begin:—

$$Cl_2 + H_2O = HClO + HCl,$$

and so the process continues until the Cl is all used up, the HClO meanwhile giving up its O and oxidizing the coloured fabric.

It therefore seems wrong to say that bleaching is directly effected by Cl; it is more correct to say by HClO.

The investigation does not quite end at this point. Experiments involving crystallization and dialysis were performed with the object of proving whether bleaching powder is a "mixture" or a "compound". But either the experiments were beyond the pupil's skill, or the directions given in the books consulted were inadequate. At all events the conclusions were negative, save that it was suggested that dry bleaching powder is a compound, but that in aqueous solution it becomes dissociated into chloride and hypochlorite.

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§ 5. An Instance from Physics

The boy who did the following illustrated his notes with a large number of diagrams, but it is not necessary to reproduce many of these. The boy had done some good, fairly advanced work in Electricity and Magnetism, Heat, and Inorganic Chemistry, but all he knew of Hydrostatics was the usual small amount done in connection with densities, &c., three years before.

"Determine in the case of the syphon barometer, (1) whether the tube must be of uniform bore; (2) whether it must be placed in a vertical position; (3) whether the tube may be irregular in shape; (4) whether the height of the mercury column will be affected if a piece of iron be allowed to float upon the mercury in the cistern; (5) whether the barometer will give correct readings (*a*) if in a sealed room, (*b*) if the cistern is closed except by a pinhole; and (6) show how to correct a scale to give *direct* readings of the barometric height.—As far as possible you are to prove everything experimentally from first principles. You may consult what books you like, but you may not accept any assumptions made by the writers of the books; nor may you make any assumptions from your own previous knowledge."

1. Liquids take up the shape of their containing vessels, and if poured out on a level surface, spread themselves out on that surface; and the more perfect the liquid the more quickly do its parts act. Water, for instance, acts more quickly than glycerine. All this is a matter of common observation. In a perfect liquid, if there was one, we may assume there would be no resistance to change of shape, i.e. one part could slide over another without any friction whatever. Since, then, a liquid yields immediately to the slightest force, a fluid at rest cannot exert any frictional or tangential force against the surface in contact with it. Therefore *the pressure of a fluid at rest is always perpendicular to the surface with which it is in contact.* (Principle I.)

2. Experimented with a glass globe containing (*a*) a neck fitted with a piston, and (*b*) a series of small holes above, below, and at the sides.—Filled the globe with water and applied pressure to the piston. The issuing jets showed that the pressure was transmitted about equally in all directions.

3. Experimented with a larger glass globe containing four necks, each neck fitted with a cork containing half-inch glass tubes so arranged as to test the pressure at the top, bottom, side, and centre of the globe, by means of water columns. Filled the globe with water, and applied additional pressure by pouring more water into one of the necks. The result showed that the pressure was transmitted through the liquid equally in all directions.

4. The conclusion is that *when pressure is communicated to any part of a liquid, it is transmitted without change of intensity equally in all directions through the liquid.* (Principle II.)

5. It follows that *a pressure applied to any unit area will be felt without change of intensity on every other unit area.* (Principle III.) Hence, in a closed vessel filled with water and containing two pistons, A and B, B having x times the area of A, if unit force be applied to A, x units must be applied to B to keep it from being forced out.

6. If one brick rests on a second, the second is subjected to the pressure of the first. If a third brick is added, the bottom brick is subjected to the pressure of the other two, the middle brick to the pressure of one. If a fourth be added, the bottom one is now subjected to a pressure of three; and so on. From this we may infer that we should probably experience an increase of pressure with an increase of depth in a liquid.

7. Experimented with a glass tube, $12'' \times 2\frac{1}{2}''$, open at both

ends.—A movable base consisting of a metal disk was pressed against one end of the tube and the whole was lowered vertically into a jar of water. The disk was maintained in position. Inference: water exerts an upward pressure.

8. By means of a string attached to the disk and passing through the tube, the disk was connected to the scale-pan of a balance, and counterpoised by shot placed in the second pan. Different weights were now placed in the second pan, and just sufficient water poured into the tube each time to make the disk fall off. Each time this happened the weight added always showed the same ratio to the height of the water poured in.

9. Counterpoised the movable disk as in the last experiment, but, instead of adding weights, plunged the tube and disk into the jar of water (as in Experiment 7). The disk always fell off when the levels of the water in the tube and the jar were the same. Thus the water in the jar exerts an upward pressure on the disk, and the greater the depth of the disk the greater the pressure.

10. With Professor Hall's pressure gauge (thistle funnel covered with membrane and connected by rubber tubing with horizontal glass tube containing a liquid index), tested the pressure in various parts and in various directions of a large vessel of water.

Inferences from these experiments:—

11. *The pressure in a liquid varies directly as the depth.* (Principle IV.)

12. *The pressure at a given point is equally great in all directions.* (Principle V.)

13. *The pressure is equally great at all points in the same horizontal plane.* (Principle VI.)

14. Experimented with Pascal's vases, the bases being of the same area, but the shapes of the vases differing greatly.—In all cases, for the same weight in the scale-pan, the disk fell off when the water rose to a particular level. The experiment proved—

15. That *the pressure on the base of the vessel depends solely upon the height of the liquid above the base.* (Principle VII.) The pressure is independent of the shape of the vessel and of the quantity of liquid in the vessel.

16. That *whatever be the shape of the vessel, the pressure on the base is equal to the weight of a cylindrical column of water with a base equal to the base of the vessel and a height equal to the height of the contained water.* (Principle VIII.)

17. The truth of Principle VIII is obvious when the vessel is

cylindrical, as at A. When the vessel is shaped like B the result may be explained thus: the pressure at any point x is perpendicular to the side (Principle I), and produces a *reaction* equal in magnitude and opposite in direction. Resolving this force into horizontal and vertical components m and n, we are able to see how all such horizontal components may be neglected, and how all the vertical com-

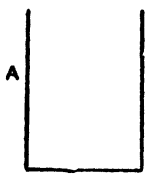

Fig. 17

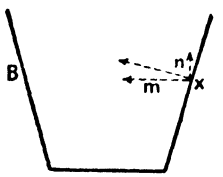

Fig 18

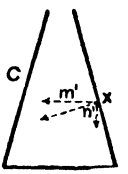

Fig. 19

ponents act *upwards* to support the water over the slant side. Thus the pressure on the base is *less* than the weight of the whole of the liquid in the vessel, the difference being supported by the slant sides.

18. When the vessel is shaped like C, the component n' acts *downwards*, and so increases the pressure on the base; the base is thus subjected to a pressure *greater* than the weight of the liquid.

(The pressure thus exerted must not be confused with the pressure which the vessel and its contents exert on the table or other supporting body.)

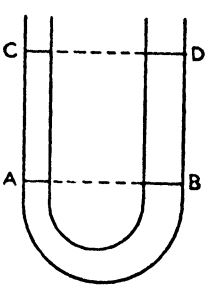

Fig. 20

19. In 17 and 18 it will be seen that if the area of the base is increased the pressure on it will increase.

20. Observation shows that *a liquid always falls from a higher level to a lower level.* (Principle IX.)

21. Experiments with variously shaped vessels showed that *the surface of a liquid in equilibrium is always horizontal* (Principle X); and that

22. *All liquids seek their level.* (Principle XI.)

23. Thus in a U-tube, whether the arms are of equal or unequal cross section, the contained liquid will maintain a level.

24. The sectional area of the right arm DB of the U-tube CABD (fig. 20) was four times that of the left arm CA. Mercury was poured into the tube until it reached the level AB. If water is poured into the arm CA, the pressure on the surface A is transmitted equally to B, and in order that the mercury may retain its original level in the arm DB, we must pour into DB an amount of water four times as great as was poured into CA. (Principles II

and III.) Experiment showed this to be the case; also that the water in the two arms now reached the same level CD.

25. If, then, we poured two different kinds of liquids in the respective arms, say one twice as dense as the other, the less dense liquid column ought to be twice as high as the more dense, if the mercury retained its original level AB.

26. Found, by using the specific-gravity bottle, that the relative density of alcohol was ·8. Poured water into the arm CA of the U-tube, and then alcohol into the right arm DB until the mercury was restored to its original level. The height of the water column to the height of the alcohol column was, as expected, 4 : 5. Therefore the U-tube may be used for finding the relative densities of two liquids by measuring only the *heights* of the respective columns above the mercury level.

27. If the liquids do not mix, we might dispense with the mercury and measure the heights of the columns from the horizontal plane passing through the surface of separation of the liquids.

28. Experimented with inverted U-tubes (Hare's apparatus), using water and alcohol.—Varied the experiments as follows: (*a*) Upright tubes of the same bore; (*b*) upright tubes of different bores; (*c*) sloping tubes of different bores; (*d*) irregular tubes of different bores; (*e*) levels of liquids in beakers the same; (*f*) levels of liquids different; (*g*) beakers at different levels. The results were always the same; the heights of the columns of alcohol and water, measured vertically from the surfaces of the liquids in the beakers, were always in the ratio of 5 to 4. The size, shape, and position of the tubes, the position of the beakers, the levels of the liquids in the beakers, made no difference at all to the result. This was expected, considering the various Principles already established experimentally. The apparatus can therefore be used for finding the relative densities of liquids.

29. The pressure of the atmosphere may be ignored, since it affects both columns in exactly the same way. Inside the tube the reduced pressure is exactly the same on each column; on the surface of the liquids in the beakers, the outer atmospheric pressure is also just about the same in the two cases; one beaker would have to be very much higher than the other for the difference in the atmospheric pressure to be at all measurable.

30. So in the case of each liquid, we have the outer atmospheric pressure balanced by a pressure due to a liquid column increased by the pressure due to the rarefied air. Thus one liquid column balances

another, or the pressure due to the one on unit area is equal to the pressure due to the other on unit area.

31. The questions about the barometer can now be answered.

The barometer is an instrument for transmitting the pressure of the atmosphere, and this pressure is transmitted to, and the amount indicated by, a column of mercury. It is practically a long U-tube, one arm containing a column of air extending from the earth to the upper surface of the atmosphere, and the other containing mercury. Therefore, what applies to the ordinary U-tube containing balancing columns also applies to the barometer.

32. The tube need not be of uniform bore. The pressure will depend merely upon the *vertical height* of the mercury column. From the previous experiments it is clear that the shape of the tube, the position of the tube, and the regularity of the tube will not in any way affect the vertical height of the column of mercury. (See Principles II, IV, VII, and VIII.)

33. Floated a piece of iron in the cistern of mercury of a barometer. The mercury rose in the tube. This was to be expected, as the column of mercury now has to support the pressure of the atmosphere *and* a piece of iron.

From this it follows that, in the wheel barometer, since the iron float is necessarily rather heavier than the counterpoise, any diminution of atmospheric pressure which is less than the difference between these weights would not be sufficient to raise the iron float, with the consequence that the diminished pressure would not be indicated on the dial. If only for this reason, the wheel barometer is not to be relied upon.

34. The barometer will not give correct readings of the outer atmosphere if placed in a sealed room, i.e. a room which has absolutely no communication with the outer air. But usually, however carefully doors, windows, and chinks may be fastened and stopped up, air will force its way through the porous walls, ceiling, and floor, and the barometer will register the pressure of the outside atmosphere correctly.

35. A pin-hole is quite sufficient for the atmosphere to exert its full force. The size of the aperture is quite immaterial. This is an application of an extreme case of one of Pascal's vases (fig. 19).

36. The height of the barometer is best taken by means of *two* readings, viz. the readings of the heights of the mercury in the two limbs of the U-tube; and taking their difference. But a *direct* scale may be constructed as follows.—Suppose the cross section of

the open limb (supposed uniform) be ten times the area of the cross section of the closed limb. Then when the mercury in the tube (the closed limb) rises one inch, the level in the cistern (the open limb) will sink one-tenth of an inch. Thus there is now an increased difference between the mercury levels of $1\frac{1}{10}$ in. It follows, therefore, that a length which is actually one real inch on the scale must be marked $1\frac{1}{10}$ in. And so on in proportion.

If there is no simple relation between the cross sections of the cistern and tube, the scale must be made by trial.

It is a very easy matter to criticize the result of this investigation. For instance, "principles" have been established that are not afterwards used, and assumptions have been made that what applies to the pressure of a liquid applies also to the pressure of a gas; there is no differentiation of "fluid" pressures. And so on. Yet, for a boy, the result cannot but be regarded as very satisfactory. The subject is difficult, and very few textbooks deal with it adequately. The phrase "pressure at a point" is responsible for much vague thinking on the part of those beginning hydrostatics

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CHAPTER XLII

Solving Mathematical Problems in the Classroom

"The profound study of Mathematics seems to injure the more general and useful mode of reasoning,—that by induction. Mathematical truths being, so to speak, *palpable*, the moral feelings become less sensible to impalpable truths."[1]

"Nothing is less applicable to life than a mathematical argument. A proposition couched in ciphers, is decidedly either true or false. In all other relations the true and the false are so intermingled that frequently instinct alone can decide us in the strife of motives, sometimes as powerful on the one side as on the other."[2]

"He who is styled a mathematician very frequently succeeds in passing for a deep thinker, although under that name are included the veriest dunderheads in existence, incapable of any business what-

[1] Walpole, in *Walpoliana*, vol. i, p. 113. [2] De Stael, *De l'Allemagne*, vol. i, p. 163.

soever which requires reflection, since this cannot be immediately performed by the easy process of connecting symbols, which is more the product of routine than of thought."[1]

Thus spoke three of Sir William Hamilton's "cloud of witnesses" who were brought forward to prove to the world that Mathematics was a wholly unprofitable study. Sir W. Hamilton himself looked upon Mathematics as altogether beneath the attention of intellectual men, the subject being "so easy that it affords no real discipline". His temerity in discussing a subject about which he knew practically nothing at all is little short of amazing, and Mill had a very easy task in demolishing the whole of the arguments of the famous Scottish metaphysician.[2]

Yet the fact remains that, even to this day, a considerable number of people are inclined to speak slightingly of the study of Mathematics and of the usefulness of mathematical knowledge. This is perhaps due not so much to the survival of the old opinion that the subject affords no practice in the estimation of conflicting probabilities, and therefore does nothing towards developing the kind of sagacity most required in the conduct of practical affairs; it is rather that, when the ordinary synthetical demonstration of a mathematical problem is, step by step, followed out, the whole thing seems so simple, every step following directly and logically from the previous step, that the process seems to demand no real intellectual effort at all.

Now this synthetic or deductive method is the method by which the mathematician *exhibits the result* of his work. Such a demonstration rarely or never reveals the secret whereby he *discovers the method* by which he attains the result. Actually, the problem is always solved by *analysis*, not by synthesis; the reasoning is, in the main, *inductive*, not deductive. Although it is true that deductive reasoning appears to form the very essence of all mathematical exposition, it is a mistake to think that the actual work of the mathematician is entirely, or even primarily, of a deductive character. His work is largely inductive, and is therefore in important respects akin to that of the man of science.[3]

[1] Lichtenberg, *Vermischte Schriften*, II., p. 187.

[2] See Hamilton's *Discussions on Philosophy*, pp. 263–340; and Mill's *Examination of Hamilton's Philosophy*, pp 591–616

[3] It is not intended to suggest here that Mathematics has now become a "practical" subject. With the arguments of the extreme advocates of the two mathematical schools of thought—the advocates of the ultra-academic school on the one side, and those who, on the other side, would reduce Mathematics to a number of "tips" and "formulæ" for engineering practice—this chapter has nothing to do The extent to which Mathematics should, for teaching purposes, be made a "concrete" subject is still a matter of dispute.

Let us suppose that a teacher, finding his class unable to solve a particular mathematical problem he has set them, "works out" the problem himself. No doubt the pupils will, as a rule, be able to follow without difficulty the different steps of the solution and be convinced of its accuracy. But what have they gained? They have had no real share in the work. They are still ignorant as to the way in which the teacher *discovered how* to solve the problem. To work through, in this way, a whole problem for a class is not only unnecessary; it is a teaching blunder of a serious kind. All that the pupils really require, or, at all events, ought to be given, is a clue by which they may set to work themselves. The art of teaching Mathematics consists, at bottom, in telling the children just enough, but no more, to enable them to initiate a plan for successfully assailing the central difficulty of a problem. To tell them more than this is, psychologically, altogether wrong.

Although, when a problem has to be solved, the procedure to be followed is that of analysis, it is a fallacy to think that, for effecting such an analysis, explicit and final directions can be given which would enable a pupil to proceed, with certainty of success, to the solution of any proposed problem or to the demonstration of any proposed theorem. No such instructions are possible, though, according to the ordinary textbooks, all we have to do is to assume the truth of the theorem or the solution of a problem, deduce consequences from this assumption, and combine them with results which have been already established. If a consequence can be deduced which coincides with some result already established, it *may happen* that by starting from the consequences which we deduced and retracing our steps, we can succeed in giving a synthetical demonstration of the theorem or solution of the problem. But such a rule is altogether too vague for general application, because no exact instructions can be formulated by which we are to combine our assumptions with results already established.[1]

1. Suppose the following well-known exercise in Geometry be given to a class of young pupils:—

The opposite sides AD, BC of the parallelogram ABCD are bisected

[1] For further remarks on Geometrical Analysis, see almost any textbook on Geometry. For some admirably-worked-out examples of geometrical analysis, see *Euclid*, Book XIII, Heath's edition, vol. iii, "Propositions on the Regular Solids", especially the alternative solutions of Pappus. (The term Geometrical Analysis must not, of course, be confused with Analytical Geometry.)

in E and F; AF and EC are joined, cutting BD in G and H. Show that BG, GH, and HD are equal.

We begin in the usual way by *assuming* that BG, GH, and HD are equal. The first question that would probably occur to us to ask, is, How have we sometimes been able, in previous exercises, to prove two or more lines equal? and the answer almost certain to be forthcoming is,—By means of congruent triangles. The symmetry of the figure will now suggest how BG and HD may probably be proved equal, since the triangles BGF and EHD *appear* to be congruent. The quicker boys will soon observe that GH may be similarly dealt with by drawing a parallel through G or through H. The slower boys may require the help of a few suggestive questions on this point, which is the only possible real difficulty likely to be experienced by them in the whole exercise. The apparent congruence of the three triangles being now seen, the teacher will leave the remainder of the work entirely to the class. Any further help in completing the analysis ought to be quite unnecessary; and to give any help in setting out the subsequent deductive demonstration would be unwise. The whole of the teacher's work ought, in fact, to be confined to two or three questions; and the form he makes these questions take, and the way in which he deals with the answers, will be a searching test of his skill. He should tell the boys nothing that they can, reasonably, discover for themselves.

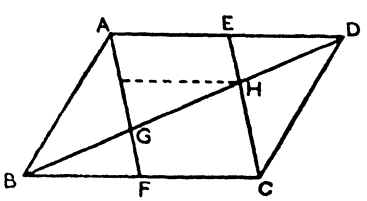

Fig. 21

2. Or, suppose the given problem to be, *Divide a given line into extreme and mean ratio.*—Without some kind of a hint, the ordinary boy will probably be unable to make a start; and if he is merely shown the construction given in many textbooks on modern Geometry, he is as much mystified as to *how* such a method could have been devised, as he is if he is referred to Euclid ii, 11, or vi, 30. But if he is told to solve the problem algebraically, and so, if possible, obtain a hint for the geometrical solution, he sees at once that the whole thing really depends upon discovering a method of drawing a line $\sqrt{5}$ units in length. And if he has previously been taught how to find the square roots of numbers graphically, he can now solve the given problem without any further aid whatever. Further, he will obtain the clue to the origin of the textbook con-

structions, Euclid's included. To *explain* these constructions to him, therefore, would be a confession of a lack of teaching skill.

3. We may take as a third example, *To inscribe a square within a given triangle.*

In working through an examination paper in geometry, a boy naturally prefers a rider which is appended to a proposition, for he knows that he probably has to deal with an application of the general principle underlying that proposition, and he is therefore supplied with the key.

This particular problem would, in such a case, probably be appended to a proposition concerning similar polygons, more particularly the inscribing of one polygon within another; a quicker

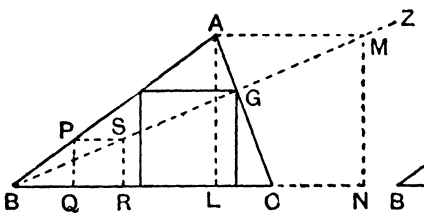

Fig. 22

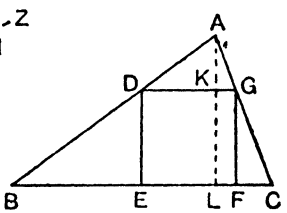

Fig. 23

boy would see that the application was straightforward, and he would proceed with his synthesis at once. He would gather from the proposition that he might draw any square whatsoever with its base in the base of the triangle and a corner on one of the other sides of the triangle, say the square PQRS or ALNM (when the side AL conveniently coincides with the altitude of the triangle). By joining BS and producing towards Z, or by joining BM; and either allowing the square PQRS to expand, the point S running along BZ; or allowing the square ALNM to contract, the point M running along ZB; the point G in AC is determined, and the rest follows at once. (Fig. 22.)

But suppose the boy did not know the general principle of which the rider is an application. An analysis would be essential, involving perhaps a number of trials. Unless the boy had previously done something to similar triangles and to proportional division, he would fail to obtain the solution. But in any case he would probably first ask himself, how is the point G to be determined in AC (or D in AB) in the completed figure? (Fig. 23.) Once

he had decided that similar triangles might give him the clue, he would not improbably follow up the fairly obviously direct course:

$$\frac{AG}{GC} = \frac{AK}{GF} = \frac{AK}{DG} = \frac{AL}{BC},$$

that is, the line AC is divided at G (or AB at D) in the ratio altitude/base. He can now set out his work synthetically, in its usual deductive dress.

4. Although the analytical method of approaching a problem applies to other branches of Mathematics generally, the method will vary materially, according to the particular circumstances. In teaching how to solve, for instance, problems producing algebraic equations, a great deal depends upon the plan adopted for making a pupil see how to apply certain general principles to all particular cases. The pupil must be taught not only how to set down his data systematically, but how to search for the given alternative conditions whereby the equation can be formulated. Take, for instance, the following well-worn problem producing a quadratic: *What is the price of eggs per dozen when two less in the shilling's worth raises the price one penny a dozen?* Pupils who have been well taught and have had a fair amount of practice with easy examples, solve this problem offhand; pupils who have been taught without insistent reference to general principles find it a little difficult.

The well-taught pupil will see at once that he has—

(1) To consider (*a*) the price per dozen eggs, and (*b*) a shilling's worth of eggs.

(2) To discover a relation between these two things. (An intelligent pupil may see the relation at once; a slower pupil will have to take the intermediate step of finding the cost of a single egg.)

(3) To formulate an equation either between

(i) Numbers of eggs for a shilling; or,
(ii) Prices of eggs per dozen; or,
(iii) Prices of one egg.

He therefore tabulates his data and the consequent derivative relations, and then equates, as follows:—

First method:—

	First Conditions.	Alternative Conditions.
Price of eggs per dozen	x pence	$x+1$ pence.
$\therefore$ Price of one egg ...	$\frac{x}{12}$ "	$\frac{x+1}{12}$ "
$\therefore$ No. of eggs for 1*s.* ...	$\frac{144}{x}$	$\frac{144}{x+1}$
Hence the equation: $\frac{144}{x} = \frac{144}{x+1} + 2.$		

$\therefore x = 8.$

or, Second method:—

	First Conditions.	Alternative Conditions.
No. of eggs for 1*s.* ...	x	$x-2$
$\therefore$ Price of one egg ...	$\frac{12}{x}$ pence	$\frac{12}{x-2}$ pence.
$\therefore$ Price of a dozen eggs...	$\frac{144}{x}$ "	$\frac{144}{x-2}$ "
Hence the equation: $\frac{144}{x} = \frac{144}{x-2} - 1.$		

$\therefore x = 18.$

or, Third method:—

	First Conditions.	Alternative Conditions
Price of one egg... ...	x pence	y pence.
$\therefore$ Price of a dozen eggs...	$12x$ "	$12y$ "
$\therefore$ No. of eggs for 1*s.* ...	$\frac{12}{x}$	$\frac{12}{y}$
Hence the equations: $12x = 12y - 1$; and $\frac{12}{x} = \frac{12}{y} + 2.$		

$\therefore x = \frac{2}{3}.$

and so with other possible methods, *all based on the same general principles.* The whole procedure of tabulation is virtually mechanical, and the only point that ought, ordinarily, to require elucidation on the part of the teacher is the derivation of the step (*a*) number of

eggs for a shilling from the step (*b*) the price of a dozen eggs; or *vice versa*. A few preliminary "mental" exercises would probably suffice to make this point clear.

5. Of course, the successful solving of problems will depend, to a considerable extent, upon the amount of previous practice in problems of a similar, if simpler, kind. We could not, for instance, expect a boy to attack the following problem, until he had learned, by working easier examples, to formulate an equation involving angular measurements as determined by the usual initial and final positions of the hands of a watch.—*Suppose it is now between ten and eleven o'clock, and that 6 minutes hence the minute hand of a watch will be exactly opposite to the place where the hour hand was 3 minutes ago. Find the time.*—The real difficulty here would probably consist in the drawing of a satisfactory figure, considering the number of angles that are involved and the close proximity of the positions of the hour hand now, three minutes ago, and at ten o'clock; and a hint would probably be necessary as to the means of discovering the approximate present position of the minute hand. Clearly from the question this position must be somewhere in the neighbourhood of a quarter past the hour. Hence a figure may now be drawn upon the blackboard, and the angular measurements briefly discussed. Any further help ought to be unnecessary, except perhaps a reminder that in this type of problem the equation is usually formulated by breaking up the composite angle in two different ways.[1] The boys must not lose sight of the general principle in the particular problem, and must be made to apply that principle themselves.

6. Or another problem: *From the top of a mast of a ship, which is 88 ft. above the sea, the light of a lighthouse which is known to be 132 ft. high can just be seen. How far is it away?*—As this is a simple and direct application of Euc. iii, 36, the teacher's work ought to be confined to the elucidation of the fact that the mast and the lighthouse are virtually prolongations of two radii of a great circle. If the class wanted more help than this, they would probably not have realized the full purport of Euc. iii, 36.[2]

[1] If the teacher found it absolutely necessary to formulate the equation himself, for instance, $\frac{x-3}{12} + 50 = x + 6 + 30$, he would afterwards insist on the boys giving one or two alternative forms of equation, using the same figure

[2] The ordinary approximation method of the textbooks (on problems involving the Dip of the Horizon) would, of course, be dealt with *after* the general principle had been fully grasped. The simplification of the usual formula might then well be given to the class as a separate and distinctive problem.

7. Or a harder problem[1]: *A cylindrical vessel is filled with water. With what velocity must it be whirled round its axis that half the water may be thrown out?*—Obviously this problem requires the solver to be familiar with the following principles:

(1) If a cylindrical vessel of water revolve about its axis, the cavity formed in the fluid will be a paraboloid.
(2) In order that any particle of water on the surface of the paraboloid may remain at rest relatively to the ground, the particle must revolve, in a horizontal plane, with a velocity which would be acquired by a body falling through a space equal to the abscissa of the point.
(3) The content of a paraboloid "inscribed" within (and therefore touching the base of) a cylinder is equal to half the content of the cylinder.

He ought then to be able to see that—

(1) The quantity of water thrown out will always be equal to the paraboloid thus formed;
(2) The greater the velocity of the cylinder, the greater will be the quantity of water thrown out, i.e. the lower will the vertex descend;
(3) The cylinder must be whirled with such a velocity that the vertex of the paraboloid may descend till it just touches the bottom of the cylinder.

The solution at once follows, viz., that the velocity must be equal to that acquired by a body falling freely through a space equal to the height of the vessel.

The problem was given as a rider to a class of students who were already familiar with Principles 1 and 3, and to whom Principle 2 had just been demonstrated in connection with problems on centrifugal force. Only one student was successful; the others had not quite grasped Principle 2, which was then demonstrated again, and several other students got out the solution successfully. The real point of interest was the teacher's attitude towards his students; he took care to see that they were thoroughly familiar with the necessary general principles, but he positively refused to help them in applying these principles.

8. The writer was once much struck by the mathematical work

[1] See Carr's edition of Newton's *Principia*, Sections I-III, p. 161.

of a class of boys in a French school. It was remarkable how little help the teacher responsible seemed to give, yet the boys showed an unusual facility in attacking problems. They were at that time doing solid geometry and were not only quite familiar with the content of Euclid, Book XI, but had done some fairly advanced work in orthographic projection, including exercises in interpenetration. The Headmaster asked the writer to give them some kind of problem, and accordingly Lewis Carroll's 63rd "pillow problem" was proposed:—

Given two equal squares in different horizontal planes, having their centres in the same vertical line, and so placed that the sides of each are parallel to the diagonals of the other and at such a distance apart that, by joining neighbouring vertices, eight equilateral triangles are formed. Find the volume of the solid thus enclosed.

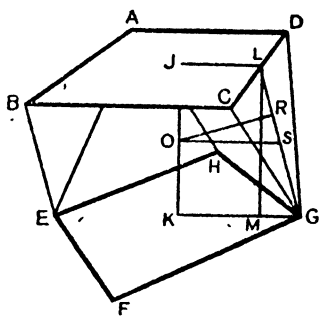

Fig. 24

Only one boy was able to draw a manageable figure, and he was then made to draw it on the blackboard. During the next three minutes no boy made any progress; then the master went to the board and put in JK, and by a question or two got some of the boys to point out that the solid was built symmetrically about JK as an axis. They were, however, still baffled, and eventually they were given a further hint, the master indicating that the midpoint O of JK might be made a possible starting-point for the dissection of the solid. Almost immediately three boys saw there were eight similar pyramids having the equilateral triangles for their bases, and their vertices at O; and it now soon came out that the solid contained still two other pyramids, the bases consisting of the two original squares, and the vertices being at O.

The whole class now saw that the problem resolved itself into finding the volume of one of the former and one of the latter pyramids.

They decided first to find the volume of ABCD.O, and were told to let each side of each square be 2 units in length. Almost without exception they quickly found the length of JK by finding LM from the triangle LMG (LG $= \sqrt{3}$ and MG $= \sqrt{2} - 1$; therefore LM $= 2^{\frac{1}{2}}$). Thus JK $= 2^{\frac{1}{2}}$ and therefore JO $= \frac{1}{2^{\frac{1}{2}}}$. Hence the volume of ABCD.O $= \frac{4}{3 \cdot 2^{\frac{1}{2}}}$.

They now took in hand the pyramid CDG.O. The area of the base CDG was obvious, but the perpendicular distance OR from the vertex to the base puzzled them for a time. Eventually two boys suggested the same expedient, viz., the drawing of OS parallel to KG. Thus the triangles ORS and LMG are similar, and as OS is known (being half the sum of JL and KG), OR can be found and $= \frac{\sqrt{2}+1}{2^{\frac{1}{2}}.\sqrt{3}}$. Therefore the volume of the pyramid CDG.O $= \frac{\sqrt{2}+1}{2^{\frac{1}{2}}.3}$. The volume of the whole solid is now easily estimated, and is equal to $\frac{8.2^{\frac{1}{2}}(\sqrt{2}+1)}{3}$.

In a class of twenty-three boys there were fifteen correct solutions. The times varied from fourteen to twenty-one minutes. No other help was given than that indicated. It will be noticed that the boys' solution is really neater than Lewis Carroll's own, though of course the actual key to the boys' solution was provided by the teacher. And the large-scale figure on the board was made very clear by a few suggestive shading lines.[1]

Briefly, then, scientific method applied to the teaching of solving mathematical problems means the method of discovery, the method of induction, the method of analysis. The pupil is taught to realize at the outset what the problem *gives* him by way of working materials, and what the problem requires him to *do*. The method does *not* mean placing the pupil in the attitude of a passive listener, of one who merely follows out the working of the teacher's mind. The teacher's work is limited to giving the pupils hints and clues, and these by means of suggestive questions rather than directly. The pupil is taught *how to discover* the method of analysing problems; his problems are not analysed for him unless he finds the task, in a particular instance, hopelessly beyond his skill.[2]

[1] Cf. "Lewis Carroll" (C. L. Dodgson), *Pillow Problems*.

[2] Those mathematicians who have never yet suspected the stability of the foundations of their subject would do well to read Mr. Berkeley's *Mysticism and Mathematics*. Mr. Berkeley tells a story of a friend who dropped pure Mathematics because it demanded "a low cunning" of which he was destitute! He himself finds in the orthodox exposition something inimical to straightforward thinking, and slyly suggests that "the modest avowal of incompetence on the part of those who are unable to follow it is at once a tribute and a sacrifice to intellectual integrity"!

BOOK V

FURTHER APPLICATIONS

Introduction

The first three chapters of this section deal with a type of problem different from those hitherto considered, for each is concerned with an estimate of values alien to science. But, as before, that estimate has necessarily to be based on facts, and at first sight it might therefore seem possible to weigh those facts, to verify them, to construct hypotheses to cover them, and to arrive at something like an objective judgment.

But the facts available in such cases do not admit of this precision of treatment. A conclusion can only be more or less probable. The cardinal difficulty is to arrive at a conclusion which is entirely unprejudiced. Even the most unprejudiced person has at least one basic prejudice: he leans to conservation or he leans to progress; and at least to that extent his judgments are apt to be warped.

A perfectly honest thinker who is a robust partisan, be he historian, theologian, philosopher, or politician, may find it difficult to give a scrupulously just judgment in a matter within his own department. His judgment can hardly escape being tinged with subjective colouring.

But there is an element of prejudice more subtle than mere partisanship.

We commonly take our opinions from the accepted beliefs of the society in which we move, or the party or church to which we belong, from custom, from tradition, from social environment, from any source except that of careful independent thought. The memory thus becomes a storehouse of conventional ready-made opinions, and these gradually harden into irrational convictions.

It may sometimes happen that the irrational cause of an opinion we hold can clearly be traced to party feeling or to self-interest, and with an effort we may eliminate such a prejudice from any question involving the consideration of human values. But most of our irrational convictions have become so much a part and parcel of ourselves that we are almost unsuspicious of them, we are certainly

unaware of their irrationality, and we never dream that by them we are held in the bonds of a prejudice which colours our every decision and action. Can we in such circumstances form a scrupulously just judgment? In any case, personal conviction is not necessarily proof. Conviction may outrun proof or may fall short of proof. Subjective conviction is no true criterion of objective certainty.

Belief admits of all degrees of intensity, from the subjective feeling of certainty, through degrees of probability, to doubt and suspension of judgment, and again through degrees of improbability and disbelief.

For all these reasons it is evident that different people will almost inevitably come to different conclusions when they undertake to examine problems involving human interests, and this despite the admitted facts and principles which form the common jumping-off ground.

If, in attempting to solve a problem of this kind, the reader becomes conscious of the slightest feeling in favour of a particular solution, the reasons for the feeling should be sought and examined, and the feeling suppressed. The essence of an inquiry conducted according to the principles of the methods of science is that, throughout, the investigator remains not only intellectually cold but personally indifferent to any judgment which the facts he is examining may compel him to deliver.

CHAPTER XLIII

The Cause of the Decline and Fall of the Roman Empire

1. *Necessary Facts and Principles for preliminary consideration.*
   (1) General. (2) Historical. (3) Scientific. (4) Progress and Decadence
2. *The Case of Rome:*
   (1) The Main Facts from Roman History.
      (i) General. (ii) The Early Empire's Sources of Strength. (iii) Apparent Moral Weaknesses. (iv) Admitted Sources of Real Weakness.
   (2) Hypotheses as to the Decline and Fall.
      (i) Political. (ii) Economic. (iii) Sociological.

1. FACTS AND PRINCIPLES COMMONLY ACCEPTED

(1) General

1. There is a danger of an unreasoned worship of the past, and of an unjustified pride in the present. It is unjust to measure ancient values by modern standards.

2. The phenomena of decay and death still call for explanation, even in the organic world.

3. In the fall of States, we must distinguish between what the rulers did and what they failed to do.

4. There is no foundation for the fatalistic theory that a people *must* decline. It is a false analogy to compare the rise and decay of a nation with the successive phases of a normal human body.

5. Great calamities no doubt contribute to the fall of a State, but the main factors leading to the fall are usually obscurer.

6. The influence which a superior civilization may have in advancing an inferior one is not likely to produce self-support

unless its character is in harmony with the temperament and innate capacities of the inferior people.

7. Certain groups of nations are apparently capable, of their own initiative, of a certain measure of civilization and no more. Others seem incapable of either creating a civilization of their own, or of preserving unaided a civilization imposed on them from without. The kaffirs and negroes were undisturbed for centuries, but failed to move forward.

8. The "struggle for existence" between nations is really the struggle during peace-time. Under our normal environment, reaction is almost automatic; but given new circumstances in an unusual environment, then the prompt action which leads to success in the struggle depends on classified experience, trained foresight, and developed brain-power.

9. National solidarity is unlikely to be real unless the co-operation of citizens is effective. The counting of heads is a crude process, but the representative system based on this process is the best devised so far.

10. Forms of political organization seem to matter little. *The* thing is the quality of the human stuff. Decay follows the decline of natural superiority. What man *does* is the essential factor in each civilization. The great formative interests of man's mind are the things that matter.

11. Civilization does tend to impair valuable human qualities. The migration of the best stock from the country to the towns results in more leisure and more amusements; the abuse of the leisure leads to deterioration. Striving ended, decay follows. Further, the progress of civilization means a wealth of new customs and beliefs, and there is frequent failure of a wise choice amongst them. The consequences are obvious.

12. History shows clearly that civilization is an intermittent and a recurrent phenomenon.

(2) Historical

1. Can we trace the possible common factors leading to success and leading to failure in the great civilizations of the past and of the present? One unmistakable element of strength may be seen in all progressive political societies—solidarity of some kind; for instance in France there is a feeling of equal opportunity and of even-handed justice; in Germany, a pride in achievement and in

scientific organization; in the United States, a confidence that energy will bring a sure reward.

2. Moral instability is characteristic of both ancient and modern Greeks. But beware of inferences from this fact: the modern Greeks are presumably predominantly of Slav stock.

3. Greek thinkers tended to idealize the immutable as possessing a higher value than that which varies.

4. The strength of the Romans was not in their originality, or in constructive or inventive powers, but in their powers of adaptation and assimilation.

5. Who and what were the "Romans"? Contrast 200 B.C. and A.D. 200.

6. In discarding mediæval *naiveté* and superstition, the renaissance thinkers turned their thoughts backward, and adopted many of the prejudices of Greek philosophy. Not until the seventeenth century was there a general rebellion against antiquity.

7. Printing, gunpowder, and the compass were three inventions unknown to the ancients; they changed the world profoundly—its literature, its warfare, its navigation; they were incomparably greater in their effects than the work of politicians.

8. The fall of a great empire did not necessarily imply extinction, or even decadence, but often a mere transfer from one autocrat to another.

9. Each people that has attained a high civilization appears to have done so on the basis of the intellectual and moral qualities of the races which entered into its composition.

10. The diminution of national power is no proof of national decadence. The cases of Holland and Venice are examples. Even Spain at the end of the seventeenth century was exhausted rather than decadent.

11. Has Western civilization produced the nearest approach to ideal man? What of the civilizations of India and China?

(3) Scientific

1. In their criticisms of human achievements and possibilities, historians now recognize the necessity of considering the teachings of science, especially in biology, evolution, heredity, eugenics, and sociology.

2. Scientific discovery and industrial achievement is a great new social force. The change of the setting of civilized life during

the last 100 years is not due primarily to politicians, philosophers, theologians, or soldiers, but to men of science.

3. The laws of heredity have still to be formulated, but they are evidently inexorable in their working. It is known that genius is rarely, if ever, transmitted; but ability tends to persist for three or four generations, though not always in the same form.

4. Can the principle of "natural selection" be applied to communities as well as to individuals? "The survival of the fittest" is a hateful idea to democracy, but apparently it is inevitable as long as human nature remains what it is. Can this be changed?

5. The lesson of eugenics — the necessity of the upkeep of beneficial qualities. The one thing that matters is the quality of the human stuff, and therefore the development of the best stocks and the restraint of the unfit are essential. But democracy objects.

6. Education and general environment are now known to be inadequate to raise superior strains from inferior stocks. Ability differences are inevitable.

7. The great object of science is the progress of mankind. Science therefore desires the free use of the social ladder to ensure constant additions to the best strains. Socialism seems to make a mistake in proposing a closed system and the suppression of the individual, who is to be a cog in a perfect machine. Incentives to enterprise by the best stocks are thus weakened, and progress is checked.

(4) Progress and Decadence

1. Progress is the directing idea of humanity. Are we justified in expecting, from the teaching of history and evolution, a steady and indefinite advance, even though, physically and mentally, men have remained much the same in all known ages? Advance seems too slow to be discernible much under 100,000 years. No satisfactory answer is possible; our knowledge of national and social psychology is insufficient. Our descendants are likely to be born into a world as different from ours as ours is from that of palæolithic man. Finality of progress is probably a psychological illusion.

2. Still, progress is presumably limited in any given age by the limitation of human faculty at that age. And may not progress come to a temporary pause, if not to a final stop, some day, if incentives to investigation are discouraged?

3. Any progress in knowledge and material comforts is always

obvious. But can our efforts ensure progress towards a higher civilization?

4. Progress implies (1) the development of public well-being, (2) the development of individual life. The latter is the main factor in a "higher" civilization.

5. The hypothesis of man's moral and social perfectibility does not seem to be supported by acceptable evidence.

6. Two factors combine to make each generation what it is. (1) Physiological inheritance, which imposes a limit on variation; (2) acquired qualities, non-inheritable, due to physical and psychical conditions of environment: these qualities probably determine progress or decadence. If progress is arrested, probably either the external physical conditions have become unfavourable, or the psychical conditions (opinions, beliefs, &c.) have become hardened and non-progressive.

7. When reaction against recurring evils grows feebler, enterprise slackens, and degeneration sets in.

8. The more obvious causes of decadence have themselves to be explained by causes more general and more remote.

9. But it seems probable that the real causes of national decay can be given only by a sociology which has arrived at scientific conclusions on the life-history of different types of society. Sociology has not, however, yet been placed on a scientific basis.

> *Hypothesis.*—A rise in civilization is initiated by the biological blending of two races; this gives the blended stock a new energy which carries it upwards; after a time this effect is exhausted, and a decline sets in (Flinders Petrie). The qualities of the people become inadequate to support the increasing complexities and ever higher demands of the civilization. Fundamental danger: the deterioration of the innate qualities of the population.

2. THE CASE OF ROME

The reader will find it useful to prepare sketch-maps representing the extent of Roman possessions during successive centuries, then to draw two graphs, ordinates being erected at century intervals, one to represent the increase and decrease of the area of the empire; the other to represent, roughly, but as nearly as the known facts permit, the rise to and fall from the pinnacle of a great civilized State. A comparison is instructive.

(1) The Cardinal Historical Facts to be Considered

(i) General

1. *The Republic.*—Citizenship in ancient Rome: inequalities, concessions. The old nobility of birth succeeded by a new order, presumably a blend of efficient strains and an aristocracy of merit. No representative system, and voting power nullified by distance. The Senate really ran the State, and possessed great power after the struggle with Carthage. Challenge of the popular party.

Change in economic position. In early days the agricultural system of small holdings thrived as the result of the personal energy of the stolid and patient Roman people. Scientific agriculture learnt from Carthage; then followed large estates, slave labour, and economic revolution.

With the mastery of the rich Mediterranean world, opportunities came for amassing wealth. Provinces exploited. Energy directed to money-making. Dwindling away of tough peasantry. Party violence, rival ambitions, revolution, the army supreme.

2. *The Empire.*—The dropping away of the disguises of monarchic power. The great frontier armies the source of Imperial strength. The dying off of the old nobility; their places taken by promoted financiers and adventurers who became patrons of hordes of parasitic dependents. Serious lowering of moral standards. Bureaucratic machine gradually filled up by Greek freedmen: consequences. Definite signs of retrogression before the end of the second century.

Reconsideration of policy of great estates because of diminishing supply of slaves. The tenancy system.

Growth of barbarian power beyond the frontiers. Internal and external disasters of third century. Rapid decline of agriculture. Progressive moral degradation of the upper classes.

The orientalized autocratic monarchy: a further lease of life for the empire. Permanent division. Disintegration of the western half. Survival of the eastern (non-Romanized) half until the coming of the Turks.

(ii) The Early Empire's Sources of Strength

1. Conditions were entirely conducive to the happiness and contentment of the people during the earlier days of the empire, especially during the first half of the second century.

2. Greek culture was fostered; wealthy men were generous; there were few wars of aggression; there was a general advance in humanitarian ideals; there was an anxious seeking after spiritual truth; education was held in esteem; physical culture was cared for; law was placed on a scientific basis.

3. The Mediterranean area was a source of great wealth.

4. There were no disintegrating influences of national sentiment in the provinces. The absolutism and bureaucracy of the imperial system seemed to satisfy that sentiment.

5. There was a lack of cohesion of the enemies over the borders.

(iii) Apparent Moral Weaknesses

1. Institution of slavery. But this was concomitant with the rise as well as with the fall of the empire. It was common to many ancient states. The resulting evils were great, but, under the empire, they were diminishing evils.

2. Gladiatorial shows. But ostentatious brutality is common enough with conquering nations, and is not unknown even amongst those which are peaceful (Britain and Spain, for instance). Such survivals of barbarism seem to signify little.

3. Gratuitous distribution of corn to urban mob. But even if this did demoralize the mob at Rome, Rome was not the empire.

4. There were many morally detestable and politically pernicious aspects in the social system. But some of these improved rather than grew worse.

(iv) Admitted Sources of Real Weakness

1. Race suicide was common amongst the best stocks: an aversion to matrimony and a mystical admiration for celibacy seem to have been real. Hence there was a progressive decay of the native Italian stock. Depopulation was made good by an inflow of adventurers and manumitted slaves from all parts of the empire, all of doubtful moral worth.

2. The consequential increase of poverty.

3. The empire gradually lost its power of assimilating aliens and barbaric elements, and became too feeble to absorb or expel them. These elements were therefore an increasing peril.

4. The needs of the bureaucracy compelled it to resort to crude socialistic experiments. The people became disheartened because of

the serious encroachments on their freedom, and their resilience was at an end.

(2) Hypotheses as to the Cause of the Decline and Fall

(i) FIRST HYPOTHESIS: cause mainly *political*.—The Romans failed to invent a political system suited to develop the peoples they had conquered.

It is true that there was no representative system, and that no popular judgment was possible; also that, although the bureaucratic machine was efficient, the people had no real part or lot in the government.

But it is almost an axiom that, as long as the human stuff remains sound, political institutions count for little.

Case *not proven*. At most the cause suggested by the hypothesis was contributory only.

(ii) SECOND HYPOTHESIS: cause mainly *economic*.—The revolution in agriculture led to a decay of the sturdy peasantry, and was a prime cause of the ultimate poverty.

It is true that poverty became extreme, and it could hardly be otherwise in view of the crushing burdens of taxation.

But there does not appear to have been any very striking change in the general economic position as the empire went down under the barbarian attacks.

Case *not proven*. But the cause suggested by the hypothesis was no doubt a contributory cause of a serious kind.

(iii) THIRD HYPOTHESIS: cause mainly *sociological*.—There was a progressive deterioration of the human stock, especially the Italian stock. The influx of foreigners and of manumitted slaves had too great a dilutive effect. This was aggravated by the race-suicide of the best strains.

Case hardly proved, but the hypothesis possibly does suggest the *main* cause of the decline and fall.

It seems impossible to put forward an entirely satisfactory hypothesis. To lay the blame on a generation or two of the upper classes is certainly arbitrary. It seems probable that the main causes, whatever they were, were operating steadily through the

course of centuries. That race-suicide was one main cause is probable, but first as an effect and then as a cause.

But, if the Roman plant withered, the root appears to have remained sound, for after a long winter it grew up again, stronger and sturdier than ever, in the form of modern Europe. It is, however, unsafe to say that arrested progress and perhaps decay are always merely a winter rest. In the case of Rome there may have been some obscure temperamentally antagonistic elements between the Roman system and the Western peoples. Perhaps the system was too Oriental for them.

Be that as it may, the Christian Church remained even if the empire fell. So did fragments of Græco-Roman culture.

Books for Reference:

1. *The Idea of Progress.* J. B. Bury.
2. *Decadence.* A. J. Balfour.
3. *The Roman Fate.* W. E. Heitland.
4. *National Welfare and National Decay.* W. M'Dougall.
5. *The Revolutions of Civilization.* Flinders Petrie.
6. *National Life.* Karl Pearson.
7. *The Legacy of Rome.* Editor, C. Bailey.

CHAPTER XLIV

Is Pope's Verdict on Bacon Justified?

The answer to this question involves an estimate of the worth of Bacon's personality and character.

1. Bacon's Intellectual Endowment

(1) Intellectually, Bacon was the outstanding figure of his time, and he ranks with Shakespeare and Newton.

(2) The search for knowledge was the absorbing passion of his life.

(3) He was a writer of matchless prose.

(4) He was an historian, and he was richly endowed with qualities which make an historian.

(5) He had eminently sound views on law reform, and he was a great judge.

(6) He was a philosopher who inaugurated a new epoch:

(i) He exposed the futilities of crude theories either based on unsifted observations of nature or originating in apriorism.

(ii) He was the first to realize the tremendous possibilities of man's conquest over nature, and was also the first to see that the principles of pure science must be established before applied science can make much headway.

(iii) He taught men of science to see the fundamental importance of inductive methods of inquiry, and it was the methods he suggested that Harvey, Newton, Galileo, and Kepler used in their researches.

(7) He possessed remarkable political insight. He was the first to recognize (i) the great possibility of England's future; and (ii) the importance of (α) colonies, (β) command of the sea. He also recognized that the House of Commons must be supreme.

2. His Personal Character

On the one hand:

(1) He was a charming companion.

(2) He possessed great breadth of view, he loved justice, and he loved good government.

(3) He felt it a paramount duty to do all in his power to further an increase in knowledge, not only in the interests of his country but in the interests of mankind generally.

On the other hand:

(1) He lacked that personal force which is a necessary element in the equipment of any successful public man.

(2) His relations with his political associates and political rivals do not place him in a very attractive light.

(3) He was not a man of great courage. At that time it was not an uncommon thing for successful public men to end their days on the scaffold. Bacon's first patron, Essex, was executed; his second, Buckingham, was assassinated; his contemporaries in the field of philosophy, Descartes, Galileo, Bruno, were all persecuted. Not improbably it was these things which made Bacon such an extremely cautious man; he seldom took a strong line. He has

been called a "pleaser" of men. He won them by appearing to fall in with their humours and to yield to their wishes.

3. The Specific Charges made against Him

(1) He was a sycophant and place seeker.
(2) He was a party to the torture of a clergyman named Peacham.
(3) He betrayed his friend and patron, Essex.
(4) When judge he accepted bribes from suitors.

4. Replies to the Charges

(1) It is definitely known that from 1586 to 1618 he was continually pressing for employment and advancement. His letters abound in complaints of neglect, in attacks on his rivals, in compliments and flattery, in details of his qualifications, in accounts of his services.

But it is unjust to judge him by present-day standards of conduct. In his day, flattery, high-sounding compliments, intrigues for place and position, were the common custom. Even in these days, wire-pulling is rampant in high and low places alike. The language is different now, but the intention is precisely the same.

(2) Peacham refused to say if he had had accomplices in the writing of a sermon criticizing the king, whereupon the Archbishop of Canterbury, the Chancellor of the Exchequer, and other high officers of state issued a warrant to examine him under torture. Bacon was then attorney-general, and took part in the examination.

At this time it was by no means an uncommon practice to examine under torture persons charged with State offences. There are thirty-eight known cases in the reign of Queen Elizabeth. Whether legal or not, it was used, and in attending the examination of Peacham, Bacon merely followed the custom of his time. Even a present-day lawyer would be shocked if it were suggested that he should concern himself with the effect of the engines of the law on individuals.

(3) Bacon accepted a brief for the prosecution of Essex, who was found guilty of treason and executed. In after years Bacon published an explanation of the part he took in the trial. His main points were: (i) Essex had engaged in a treasonable conspiracy to subvert the government; (ii) it was the ordinary duty of a principal legal adviser of the crown to take part in such a serious prosecution; (iii) loyalty to the crown must take precedence of personal friendship.

Thus Bacon's own reply to the charge is adequate. Essex was a traitor and deserved a traitor's fate. No doubt a serious ethical question is involved in the clash of duties—duty to a friend and duty to the State. But the friend had committed a crime that could not possibly be pardoned, and Bacon followed the course that the higher duty demanded.

(4) The last charge—that he was a corrupt judge—is the most serious. Previous to the attack on him in Parliament, it had never been suggested that his judicial decisions had ever been other than scrupulously fair. It was universally recognized that his intellect was of the kind that loves to be true and right for the pleasure of being so.

It is known that he was extravagant, was always more or less hard up, lived beyond his means, and frequently borrowed money; and his own letters explicitly admit that he did accept gifts from suitors. All this no doubt tended to sap his moral strength, never perhaps very great.

But:

(i) There is no proof that he ever took money to pervert judgment, or that he ever gave an unjust decision.

(ii) Although, however, he never sold his judgments, he did allow the continuance of, and himself shared in, the mischievous custom of accepting subsequent presents for service. The power of such a custom was too strong for a character prone to be pliant to circumstances.

And it has to be borne in mind:

(i) That in the Court of James I the very atmosphere was charged with the taint of gifts and bribes.

(ii) That Bacon was almost stunned at the disapprobation of the two Houses.

(iii) That Bacon had at various times subjected to mortifying defeats his great rival Coke, who now saw his chance in the Commons and promptly seized it.

Bacon admitted that the practice of receiving gifts, in which he had shared, was reprehensible in the extreme, and he felt that, although his judgments had always been scrupulously just, yet he was morally guilty of the charges brought against him.

In a book published in 1668, *The First Part of Youth's Errors*, there is a letter by Thomas Bushel, Lord Chancellor's seal-bearer

up to the time of Bacon's fall, to a Mr. John Eliot, apparently also a servant of the Chancellor, which seems to go far to establish Bacon's innocence. Bushel suggests that his master was sacrificed to appease public clamour.

In his *Discoveries*, Ben Jonson wrote of Bacon, "All who were great and good loved and honoured him". Such testimony is not lightly to be brushed aside.

Intellectually, Bacon was probably a scrupulously honest man. Morally, he was most certainly not a bad man. But he did not possess the courage necessary to rescue himself from the rather sordid psychological environment in which he lived. He was too yielding to circumstances.

Pope's pronouncement must be held *not proven*. Macaulay's strictures seem equally to be undeserved.

Books, &c., for reference:

1. *Was Bacon the "Meanest of Mankind"?* Lord Riddell.
2. *Francis Bacon* (speech at the unveiling of the Bacon memorial, 1912). Lord Balfour.
3. *Works.* Ellis and Spedding.
4. *Bacon.* R. W. Church.

CHAPTER XLV

Is there a Criterion of Excellence in Æsthetic?

1. *Some Elementary Principles of Æsthetic.*
2. *Hypotheses as to Æsthetic Excellence.*

Some Elementary Principles of Æsthetic

1. The fine arts include sculpture and architecture, painting, music, and poetry. Phidias and Wren, Rembrandt, Beethoven, and Shakespeare, all created; and each expressed himself in his chosen medium, whether marble or paint, words or musical sounds.

In the sculpture of the Greeks, in the Gothic cathedrals, in the paintings of the Italian masters, in the symphonies of the German composers, in the tragedies of Shakespeare, there is a universal language which all the ages may read. Such great works were not produced by dilettanti in easy consultation with a wraith conjured up from a rosy mist, but by intense enthusiasm and intense labour.

2. The greatness of a work of art depends, in no small degree, upon the extent to which it not only embodies the expression of the spiritual character of the age and makes a universal rather than a local, individual, or momentary appeal, but also touches the very foundations of human character. Great works of art endure because of this depth of appeal.

3. The ordinary visitor to a picture-gallery, guide-book in hand, is often a pathetic figure doing a duty, really bored and tired. The ordinary visitor to a cathedral who discourses on periods and styles, corbel-tables and moulded mullions, is probably displaying a little intellectual vanity. In neither case is there much æsthetic appreciation, hardly any emotional response. So it is with the coldly critical connoisseur; he rather prides himself on his steady pulse as he stands in front of a great masterpiece, or as he attends to the unfolding of the culminating scene of a great tragedy. Æsthetic pleasure sometimes actually diminishes as the knowledge of art increases.

4. An æsthetic experience is preoccupation with a pleasurable feeling derived from the contemplation of a beautiful object. By the term "contemplation" we imply that the imagination is not inert and passive but is actively at work. The imagination is not a separate faculty; it is just the mind[1] busy with its stores of experience, trying to satisfy a feeling of some kind. The æsthetic attitude thus implies a dual process of contemplation and creation.

5. We may know a great deal about the history, material, composition, construction, value, and the like, of a beautiful object, but, for the æsthetic attitude, all these things are incidentals—builders' requisites for carrying out the artist's design—and are hardly present. The metre of a poem, for instance, is mere scaffolding. Æsthetically, it is the heart and soul of a thing that are essential. Of course the form is important, too, but by form we mean more than spatial shape; we mean all the variations and connections of the constituent

[1] We commonly distinguish between our body and our mind, but instead of "mind" we may conveniently use the more personal term "soul" or "spirit". The antithesis between matter and spirit is clearly justified, even though the two things are regarded as not really separable.

parts and qualities. It is only when our insight penetrates the significance of the form that we appreciate the degree of life and structure that the thing possesses. As a mere mass of cold wet vapour, a cloud may attract us not at all; but, with the sun shining on it, it displays an æsthetic form to which our feeling at once tends to attach itself. As an object reveals more and more of its form, our feeling which is united to it has more to dwell upon, to seize, and to carry off.

6. To the best of his ability, the artist *expresses* himself, in his never too obedient medium; and it is the artist's expression, half revealed, half concealed, in the picture or statue, poem, or symphony, that we try to re-create. In contemplating a work of art, we try to enter the creator's mind, follow the same path as he did, and re-create and re-embody the expression afresh for ourselves. It is our spirit trying to get into communication with, and to fathom the secrets of, the artist's spirit, and we do this through the object.

7. Vitality, character, activity, courage—things like these we cannot read off from colour combinations or from printed works; we have ultimately to arrive at them from our knowledge garnered in the school of life. In a portrait, the artist (unlike the camera) tries to portray the character rather than the features, and regards the man who sits to him as an imperfect picture of the personality within.

8. The artist seems to possess a skill whereby he can extract the spirit of a thing or an event, and embody it more or less successfully in his medium. In the work thus produced, we try to apprehend the beauty to which the artist has given expression; we try to carry off the spirit and re-embody it in our imagination.

9. The thing called beautiful, physically beautiful, is obviously necessary to æsthetic reproduction. But whether beauty is objective or subjective, whether a thing is beautiful in itself or whether it is only our thinking that makes it so, is a much disputed problem. There are times when we see no beauty in things which at other times move us deeply, and we are then inclined to argue that beauty must dwell only in our spirit. But even the cultivated portion of mankind is inclined stubbornly to believe that the beauty resides in the object itself, be the object a rose or be it the sculptured Nike.

10. Croce maintains, however, that a physical thing cannot, in the full sense, possess beauty; that, primarily, beauty implies a mind, and that its physical embodiment is entirely secondary and scarcely essential. But, assuredly, although feeling is necessary to

embodiment, embodiment is necessary to feeling. Admittedly things are not complete without minds, but, equally, minds are not complete without things. The artist cannot operate in the bodiless medium of pure thought.

11. Science shows that our mental picture of the external world is quite inadequate and therefore misleading. We are apt to think of the world of sound as a wonderful symphony of nature. We also think of a beautiful world of colour. But science shows that the universe is really a universe of eternal silence and black darkness, through which surge pulses and waves of matter and æther. We happen to possess a particular type of sense-organ, the ear, tuned to catch from the air only an insignificant proportion of the pulses of matter, and out of them is produced in our minds the sensation of sound. We have another sense-organ, the eye, so tuned as to take in only the one-forty-thousand-millionth part of the known extent of nature's mighty gamut of æther waves, and out of these are produced in our minds the sensations we call light and colour. On such absurdly inadequate foundations are built up not only our whole conception of the external universe but also our whole system of æsthetic—our ideas of beauty, whether of sound, or colour, or form, and all their varied expressions in literature and art. How great is the vanity of dogmatism!

Hypotheses as to Æsthetic Excellence

Art criticism seems always to have been essentially destructive. Does it provide us with criteria of excellence?

First Hypothesis.—*A criterion can be established by the critical analysis of accepted models.*

This view flourished after the renaissance. Criticism then accepted the classical masterpieces as supreme models of excellence, and rules for copying the methods of antiquity were framed accordingly. This applied primarily to literature; antiquity had left no masterpieces in music or painting.

But the attempt to limit æsthetic expression by rules and formulæ was soon found to be futile. We cannot construct a new *Hamlet* by taking the old one to pieces and noting how it is made.

The hypothesis cannot be maintained.

Second Hypothesis.—*A criterion can be based on the consensus of experts.*

The present-day critic concerns himself mainly in pointing out

beauties and in helping us to enjoy them. He teaches us to understand the poet, the painter, and the musician, the ends they aimed at, the models that swayed them, the conventions within which they worked. This seems to be excellent, but, after all, it is just a statement of personal preference. The critic is unable to refer to any immutable principle of judgment, based on the essential nature of beauty. He cannot appeal to some universal canon, and so be in a position to convict as wrong all who differ from him. He can produce no proof that beauty has "objective" worth. He fails to show that A's judgment is right and B's wrong.

True, there are kinds of æsthetic excellence to which we can apply an objective test, an impersonal measurement: for instance, artistic workmanship, technical skill, mastery over material. The pleasure given by the contemplation of these things is undoubtedly æsthetic. The trained critic can pick out the best horseman, the best tennis player, the best dancer, the composer showing the greatest mastery of counterpoint and fugue, the best executant at the piano. But he bases his estimate on knowledge, not on feeling. He is gauging the worth of little more than leather or prunella.

For dexterous versification may not result in poetry; admirable brushwork may express a poor picture. It is when we pass on to qualities we call "beautiful" or "sublime" that any kind of objective test seems to fail. Such qualities seem to possess no existence apart from feeling, and cannot therefore be measured except by the emotions they produce. If our test-question is, "Does this work of art convey æsthetic pleasure?" we are obviously proposing a subjective and not an objective test.

Further, there is no general agreement about things that are beautiful. Differences of race and of age, degrees of culture among men of the same race and the same age, individual idiosyncracy, prevailing fashion, occasion or accompany the widest possible divergence of æsthetic feeling. A picture may attract one man and repel another.

In our dislike of the idea of every man being a law unto himself, we turn confidingly to those who are regarded as authorities in matters of taste. The danger is that we may then assume an admiration we do not feel, and eventually allow our sham professions to become genuine sentiments. We follow the fashion as we do in clothes and in manners.

It is, however, true that there is amongst experts something approaching a common body of doctrine about the literary and

artistic masterpieces of the world. Leonardo, Raphael, Michelangelo, Titian, Rubens, Rembrandt, Velasquez, Turner, are all without hesitation placed in the front rank of artists. Others like Perugino, Teniers, Murillo, and Hogarth, are regarded as painters of great but less extraordinary power. But authorities are in hopeless disagreement when they attempt to give an order of priority to the front-rank men. The colour and vitality of Rubens will appeal to one person, the realism of Velasquez to another, the grandeur and tranquillity of Titian to a third, the mystery of the light and shadow of Rembrandt to a fourth, the perfect draughtsmanship of Leonardo to a fifth, the imagination and vision of Turner to a sixth; and so on. According as these qualities are valued, precedence is given to their exponents. Critics simply do not feel alike in front of the same masterpiece. Where the sentiment of beauty becomes intense, there is no real unanimity of personal valuation. And as with painters, so with musicians and poets.

Again, successive epochs differ vehemently in their æsthetic judgments, as the history of criticism shows. The history of Gothic architecture is a case in point. The original design of a great cathedral was seldom followed by the successive generations who completed it.

And again, the Greek musician was not worthy of a place beside the Greek sculptor and the Greek poet; in fact, the means at his disposal fatally limited his powers of creation, and the ancient accounts of the emotional effects of music are probably much exaggerated. But the Greeks placed their relatively primitive music high among the arts, and thus their scale of æsthetic values must have been very different from our own.

It would appear, then, that expert knowledge counts for little in æsthetic, and there seems to be little reason why men should mould their feelings into the patterns which authority prescribes.

There is a further point. The appeal made to uncultivated minds by what critics would describe as indifferent art sometimes produces intense æsthetic emotion. Consider the school-boy absorbed in some impossible tale of adventure. He is as credulous and as full of excited joy as some ancient Greek king listening to the tales of the wanderings of Ulysses. Does the criticism that the art is poor really make it poor? Or consider the shop-girl's smiles and tears at a popular play; or the Indian squaw's admiration of her newly painted and much befeathered lord. The existence of intense æsthetic emotion in such cases cannot be questioned. We do not

praise the popular play, the popular song, or the popular novel, but these things undoubtedly contribute greatly to the æsthetic pleasures of the multitude. The critic may maintain that the great masterpieces give pleasure of a higher "quality". But how can he prove that they *do*? How can we determine a scale of values?

The profundities, delicacies, and subtleties of a great work of art seem to be discovered only by those who themselves excel as artists, whether they be poets or painters, sculptors or musicians; such men seem to succeed in plumbing depth after depth. A scholar's appreciation is, however, more likely than not, mainly an affair of intellectual understanding, though doubtless of some feeling, too. But because he understands more, does he feel more? Are we not bound to discriminate between intellectual appreciation and æsthetic feeling? "The complete appreciation of a great poet is the last reward of consummated scholarship." Doubtless; but although the intellect may thus obtain complete satisfaction, that satisfaction may yet be lacking in any great depth of æsthetic emotion.

Since æsthetic value seems clearly to lie in the emotion it produces, it is easy to understand why special expert knowledge of æsthetic carries such little weight.

In any case, trained taste is certainly divided by deep differences, and the second hypothesis cannot therefore be maintained.

THIRD HYPOTHESIS.—*A scale of æsthetic values is not possible because art is allied to other great spiritual interests.*

This hypothesis has been put forward as an escape from the difficulty of admitting that the essence of beauty is reducible to individual feeling. An artist is deemed to be more than a maker of beautiful things; he is also a seer, a moralist, a prophet. He is gifted with an insight that penetrates the realities which lie behind the world of shows. Thus art is made (by Ruskin, for instance) not only to create beauty but to teach philosophy, religion, and ethics. But experience does not justify the association of great art with penetrating insight, or good art with good morals. Some of the most beautiful sonnets have been written by men with utterly debased minds. Atheism as well as theism, passionate revolt as well as lofty resignation, all have inspired the creators of artistic beauty.

The third hypothesis cannot be maintained.

FOURTH HYPOTHESIS.—*Beauty having no objective validity, no scale of values is possible. Beauty is expression created by an artist and re-created by the onlooker; it is therefore a communication from spirit to spirit, and its value thus being personal and individual is not assessable.*

The point at issue is what we actually do feel, rather than what the experts think we ought to feel.

The feeling of beauty springs from psychological causes complex and subtle. It is closely concerned with the things in which men most vary—education, experience, beliefs, traditions, customs. And ideals change from generation to generation.

There are two great divisions of feeling. One, the feeling of beauty: this feeling is contemplative, does not look beyond its own boundaries, and does not lead to action (we read Milton for his poetry, not for his theology); the other, the feeling of love: this feeling lies at the root of conduct, always has some external reference, and supplies the immediate motive for all the doings of mankind.

Now, admittedly, love is controlled by no abstract principle, obeys no universal rules, knows no objective standard, disdains logic; we can certainly give no account of the characteristics common to all that is lovable. Need we, then, feel impatient because we can give no account of the characteristics common to all that is beautiful?

Beauty, like righteousness and truth, is a great desire of the human mind. Even the Puritans, when they designed a beautiful heaven, turned their eyes to the forbidden luxury of art.

How came æsthetic emotion to be what it is? To what causal process is it due? We feel that the cause must be *adequate*—that origin must correspond to value.

That being so, we feel bound to rule out mechanism and materialism. It is difficult to see how beauty can be a mere chance product of molecular aggregation. It is equally difficult to see how it was developed in the general process of organic evolution. For how can we show that æsthetic emotion has survival value—that communities rich in the genius which creates beauty and in the sensibilities which enjoy it, *therefore* breed more freely and struggle more successfully than their neighbours? We may freely admit that those who trace the ultimate genesis of beauty exclusively to causes which neither think nor feel, are not involved in contradiction and are not inconsistent. But such an origin seems entirely in-