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SCIENTIFIC THOUGHT
IN THE
TWENTIETH CENTURY

An Authoritative Account
of Fifty Years' Progress in Science

EDITED BY

PROF. A. E. HEATH, M.A.

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INTRODUCTION

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I

TO do full justice to the rapid advances in scientific thought during the first fifty years of the twentieth century nothing short of an encyclopædic volume would suffice. Something less ambitious might, however, be more serviceable to the general reader. Odious selection is inevitable; but if we adopt a particular standpoint from which to view developments during this period, it is just possible (provided our position be well chosen) that we may catch a fairer view of the wood, even if all the trees are not included in the landscape.

In that hope, upon which the plan of this book is based, we take our stand. We intend to look at the scientific thought of our century as an age whose distinguishing characteristic is " the growing-up of discovery." The concepts and methods used by men of science have been made more precise and explicit, with consequent increase in speed and effectiveness. This has largely come about through radical criticism by both scientific workers and students of methodology.

It is understandable, therefore, why the opening contribution to this volume is by a philosopher, Prof. Ayer, who gives us an account of the manner in which those who reflect upon scientific work see that " second order " considerations are involved. We use a different

type of object-language in the sciences themselves from that in which we " talk about " science. Prof. Ayer indicates how analysis becomes necessary if we are to evade the puzzlements into which a less delicate use of language involves us. Conceptual symbolisms of all kinds need critical evaluation.

In the contribution which follows the same kind of rigorous analysis is applied by Prof. R. A. Fisher to one particular method in scientific work: namely that of statistical inquiry which has assumed supreme importance in all fields: from physics and astronomy to genetics and sociology. He shows how the development of statistical methods has led to its taking on a steadily more objective character, and summarizes the advances made by saying that they enable us to conduct experimental and observational inquiries so as to maximize the information obtained for a given expenditure of effort. In actual fact (as Prof. Medawar says in his contribution to this volume) it is Prof. Fisher himself, in his *Statistical Methods for Research Workers*, who has done the lion's share in devising the great variety of statistical methods required by biologists.

The rather different techniques needed in Sociology are dealt with by Donald G. Macrae in his contribution on this subject. The bare bones of statistical returns have to be clothed in the flesh and blood of human inter-relationships; and any prediction based on such returns must be looked at in the light of the fact that awareness of the prophecy may change people's attitudes. Another aspect of this sort of problem is stressed by Dr. Alice Stewart in her contribution on Social Medicine. She says that, when the major problems of preventing wastage of life have been solved, the same outlook and the same methods could be applied to increasing health; but in this phase of its

activity we have to be on our guard against a possible pitfall. All statistical work involves the determination of " normal " values. Since the normal of any biological feature is nothing more than the most frequent measurement at a given moment, it is easy to slip into the mistake of supposing that the value corresponds to what is natural or " right " for the species. False standards of what is possible can be produced in this way.

On an entirely different, and more fundamental, level Prof. Llewellyn Jones in his article on Physics describes the interesting and even exciting developments in the related topic of Probability. He shows how this has changed enormously our conception of physical laws.

II

So far I have been stressing the more sophisticated character of science in our own time ; but it is useful to remember how very brief a period has passed since man's first achievement of that balanced form of reflection which we call scientific. It is little more than three centuries since he attained the capacity of combining a passion for brute facts with the power of abstract generalization. Yet even in this short period it is possible to recognize that each age has possessed its own peculiar scientific flavour. Thus there is a certain disconnectedness in the science of the sixteenth century. Discoveries were the result of a sort of spasmodic inventiveness. In the seventeenth century instances of inter-connection and co-operation multiply. Results in one field are used in another—as when Galileo, hearing of the invention of the telescope, constructed one and turned it immediately to astronomical use. The increasing co-operative and cumulative character of science is reflected in the appearance during

this period of the great European Academies of Science, and explains why this " century of genius," as Whitehead called it, is so important for future ages, since it enabled the ground to be cleared of the age-long accumulation of untested systems which cumbered it. Individual men working in isolation could not have coped with the enormous labour of putting such systems to the test of experiment.

In the eighteenth century the method of balanced reflection, which had already been applied so brilliantly to mechanics, was directed to various physical forces and to the field of chemical fact. In the nineteenth century it was the turn of biology and the realms of mind and society. An interesting point about these developments is that they had to be fought for at each stage. Every extension to new fields was hailed on its appearance as a blasphemous or indecent intrusion of scientific method into regions not susceptible to such treatment. These old battles had already been won when the twentieth century dawned. What followed was something quite different.

III

After 1900 there were of course rapid refinements in techniques. Even in a science like Geology, whose " heroic age " was the nineteenth century, this has happened; there has been more than consolidation of what had already been achieved. New modes of attack on geological problems have appeared, as Prof. Leitch shows us. In this and many other parts of the scientific field new methods have often resulted from a kind of cross-fertilization between different sciences. Biochemistry is a typical instance. Another, described by Dr. Richter in his contribution on Neurology, is the immense strides forward which have been made in

psycho-neurology by the fusion of neural physiology and electronics with psychology.

What I had in mind in speaking of an entirely new temper in the science of the past fifty years was not, however, these various developments in techniques. It was that, in our age, earlier certainties (even in science itself) have been undermined. There was a period when physics was the beau-ideal of all scientific explanation. After that it was the turn of evolution to be regarded as providing the essential basic conceptions. At this point Time came seriously into the picture. The search for an eternal mathematical order was ousted from the centre of the stage by exploration of developments in time. At the opening of this century misgivings as to what was fundamental were further intensified by the work of radical innovators like Einstein and Freud. Recent advances in Astronomy, combining nuclear and stellar physics, force us (as Sir Harold Spencer Jones indicates) to alter our whole view of the nature of the universe.

The upshot of these developments is that we have become increasingly appreciative of revolutionary changes in our ideas, and thus more sceptical of the lasting applicability of any particular ordering scheme. This more bracing scientific atmosphere means that we are nowadays less inclined to hanker after one single type of conceptual explanation capable of dealing with all fields of human experience. Instead, we are readier to accept *the autonomy of each science* within its own field. There are important consequences of this attitude. To these we will now turn.

IV

The central figure in the great drama of scientific thought is, of course, man. We seek to understand the

universe into which he is born; his nature; and the social habit and cerebral equipment which have enabled him to develop means of communicating with his fellows and to transmit his capitalized experience to succeeding generations.

Science, like man himself, is a composite structure arranged in a hierarchy of levels; each level being built upon what is lower in the hierarchy, and each giving rise in turn to higher levels. At the lowest level man is a material object. If dropped from a height he will fall with the same acceleration as a lump of lead. He is also that kind of complex material aggregate which we call a machine: he is, for instance, a heat-engine whose output of energy can be precisely calculated in terms of the calorific value of the food he consumes. In addition he is an animal. He functions in the ways characteristic of living matter; and physiologists exist to describe such functioning. Man is also the end-product of a series of related animals: hence the need for wider biological studies, comparative and genetic. Beyond this level his development is clearly dependent upon the fact that he is a herd-creature, a social animal; so psychological, anthropological, and sociological studies are necessary.

It took a long time before this "thinking animal" could reach that level of reflective impersonality which enabled him to admit these levels in himself. He didn't like being called matter, animal, or savage—even if these were essential lower bases for his slow but incomplete approach towards becoming *homo sapiens*. Faced by unpalatable truths he took refuge in evasions, like Prof. Balmy in Sam Butler's *Erewhon Revisited*, who said, when cornered, "A spiritual enlightenment from within is more to be relied upon than any merely physical affluence from external objects."

It is the opposite difficulty which has beset men of science. They are enormously intent upon rendering the world intelligible. In consequence when a type of ordering conception achieves success at one level of the hierarchy they are inclined to presume that it must apply to other levels also. Then, because the sciences of the lower levels are naturally older and more mature than those of the higher, there is a tendency to explain away higher levels in terms of lower: psychology is nothing but physiology, physiology nothing but physico-chemical mechanism, and so on.

The more realistic temper of our century makes this line of thought less attractive. We look at the different levels and say to ourselves (in the words of a very different Butler, the good eighteenth-century Bishop), "Everything is what it is and not another thing." This is no falling away from admission of the stern truth that higher levels could not exist without the lower, of which they are the product; but each level is taken at its face value.

V

This acceptance of autonomy in the sciences is expressed very clearly throughout this volume. Dr. Kent's article on Chemistry brings out the specific contribution of that science, on its own merits, to both higher and lower levels. But it appears most strikingly in Prof. Medawar's contribution on Zoology. He attacks both vitalists and mechanists—the former because they revert to "holistic" views without adequate evidence; and the latter because they "inflate a pragmatic rule of laboratory conduct into an article of faith." He states the case for the autonomous character of biological studies as follows. We need not fear that physics and chemistry can ever dispossess biology of

its subject-matter. The problems of biology centre round biological conceptions, and these are simply " the *contextual* concepts of that rank of the natural order that happens to form the subject-matter of biology."

In psychological studies the same stand is made. Behaviourism should be regarded as a methodological device, not a creed. In his account of the recent history of Psychology (an account made all the more fascinating from the fact that Sir Cyril Burt himself was at the storm-centre in most of the developments he describes) he says : " In spite of the criticisms levelled against it in the early days from philosophers on the one side and physiologists on the other, general psychology appears at last to have established itself as a distinct department of natural science."

In similar vein Prof. Meyer Fortes inveighs against the simple comparative procedures of nineteenth-century anthropologists, and shows how these have been replaced by intensive analytical methods based on direct observation. Man is qualitatively distinguished from all living species by the fact that he lives in organized social groups. This fact, the result no doubt of the anatomical and physiological features distinctive of man, may well be the outcome of biological evolution. " But," he adds, " with the establishment of modern man on the earth there began the processes of historical development; and these belong to a different order of natural events from those of biological evolution. They involve changes in culture and social organization only, not changes in the biological heredity or even the basic social capacities of man." The new frame of reference for anthropological studies " will have to be worked out on the model of the experimental natural sciences, not on the evolutionary speculations of the founders of anthropology."

VI

There is one further lesson to be learnt from the development of scientific thought in the twentieth century: namely, that it is the chief glory of science that it is never finished. You go to myth not science for certainties. There will always be difficulties to overcome, and strange new regions of thought to explore. In his contribution on Genetics Dr. E. B. Ford gives evidence against the view that the reason why Mendel's work had to wait until this century for recognition was due to the publication of his papers in an obscure journal. They were in fact read by his contemporaries. The trouble was that men's minds were not ready for them. Men, even men of science, have tended to cling with the tenacity of despair to the security of systems. In this connection I like the remark of Hecate to the witches which is quoted by Dr. Hawksley at the end of his summary of advances in General Medicine: " And you all know security is mortals' chiefest enemy."

That is why plastic adaptability, which has been the mark of science during the past fifty years, is a sign of increasing maturity. We are now prepared to face the most radical revision of our conceptions, even of those which threaten our most cherished convictions about human nature. Scientific thought is, as I said earlier, beginning to grow up. If in the future we should find it possible to extend this adult attitude to the social as well as to the natural environment of man, then we might well be able to face our national and international difficulties with greater hopefulness.

THE PHILOSOPHY OF SCIENCE

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I

A SCIENTIFIC treatise consists, in the main, of statements, and of these statements the majority will be in *object-language*; for scientists use words primarily to make assertions about objects. There may indeed be scientific works which are wholly of this character. But most commonly it will be found that the treatise also contains statements which are expository in a different sense. They may, for example, be explanations of the use of certain terms, or comments on the logical relationship of other statements in the treatise, as when it is said that two different theories are, or are not, incompatible, or that one set of statements is evidence in favour of another. And these statements which refer not directly to the subject-matter of the science in question but to its concepts, or to other statements, can be described as belonging to *meta-language*. Strictly speaking, therefore, they do not form part of any scientific theory: for a scientific theory finds its complete expression in the object-language. As statements of meta-language they belong rather to the philosophy of science. It is not the case, indeed, that all statements about statements in a meta-language are philosophical. But the converse does obtain, at least for the philosophy of science. It is true of all its statements that they are meta-linguistic. What makes them philosophical is not easily defined, but it will be found that they all deal in some way or other with questions of meaning and of evidence.

An example of the way in which philosophical and

scientific questions are related is afforded by the quantum theory. This theory contains equations whose solutions refer to states of a physical system; the equations enable one to calculate with a certain degree of probability the transitions from one state of the system to another. That the predictions which it yields are statistical is an integral feature of the theory. But now it may be asked whether there is a possibility of turning these statistical predictions into causal predictions; and this may be construed as a philosophical question. It is a philosophical question to ask whether the Principle of Indeterminacy expresses only a limitation of our knowledge or whether it reflects the nature of the facts. When it is put in this way, this question does not indeed appear to be a question about language, but it is so none the less. For the answer to it depends on our deciding whether there is a meaning in saying that a particle has at any given instant a definite position and a definite momentum. Those who hold that the meaning of physical concepts is dependent on the operations which enable them to be applied will argue that there is no sense in ascribing to a particle a definite position and a definite momentum at one and the same time: for the processes that yield a precise measurement of its position and those that yield a precise measurement of its momentum are mutually exclusive. Consequently, those who take this view will maintain that the Principle of Indeterminacy reflects the nature of the facts. On the other hand, those who hold that the meaning of statements about the positions and momentum of particles is independent of the processes of measurement by which they are determined may very well regard the Principle of Indeterminacy as expressing a defect in our knowledge. They may argue that a particle does in fact have at any

given instant a definite position and a definite momentum; and that we cannot say *a priori* that there is no means of discovering them. Now, this is a philosophical dispute: the disagreement is not about the use of the quantum theory, but about its interpretation. It need not, however, be purely philosophical. A physicist, like Einstein, who holds that the theory yields only " an incomplete description of a real situation," hopes to be able to subsume it under a wider, or deeper, theory, in which perhaps the principle of causality is re-established. Thus, to the difference in philosophical outlook there may correspond a difference in scientific expectation. This does not mean, however, that the philosophical question can be settled on the scientific level. Even if a theory were forthcoming which yielded what a classical physicist would regard as a " complete " description of the physical situation, the operationalists would not thereby be refuted. For they could argue that what was understood by the " physical situation " depended on the method of describing it. Thus the quantum theory would, in their view, furnish " complete " descriptions of the situations to which it referred. In the new theory the meaning of the terms would be different: they would be associated with different operations. It would, therefore, be legitimate to say that the situations which the theory described were not the same. Neither need this be inconsistent with the assumption that the new theory incorporated the other. For all that would be required is that it should account for the same phenomena. And a difference in what is conceived as a " physical situation " need not be matched by any difference in the phenomena. The distinction may arise only at the level of interpretation.

For a second example we may turn to psychology.

The concept of the unconscious mind has a use in psycho-analytic theories. The psycho-analyst knows what tests to apply in order to decide whether his patient has such and such an unconscious memory or belief; and his statement that the subject's behaviour is due to this or that unconscious factor is a scientific statement. But then it may be asked whether, as Freud thought, the Unconscious literally exists, or whether it is a fiction which serves only to facilitate the description of overt behaviour. Further, it may be asked in what sense and to what degree one can explain people's behaviour by referring to the Unconscious. And these are philosophical questions. In the first instance, we have to decide what we are going to mean by saying that something does, or does not, literally exist. Because we have an established use for the word " existence " in certain contexts, as for example in the case where the perception of a real physical object is contrasted with an hallucination, we are tempted to assume that the question whether something or other exists is always a question of fact. But this may not be so. The question may be verbal, in the sense that it turns not on any doubt about the nature of the facts but on the choice of ways to describe them. Thus, between the Freudian who insists on the reality of the Unconscious and the behaviourist who explains away the Unconscious as a convenient symbol, there may be no difference of opinion in regard to anything that is observable. Neither will they necessarily disagree about the ways in which the relevant phenomena are linked together. The point of their dispute may simply be that one wishes to speak of the reality or existence of events or processes which are pictured as lying " behind " the phenomena, while the other wishes to confine the application of these words to

what can be observed; and here the dispute may be further complicated by a difference of opinion as to what is to be reckoned as observable. It may not then greatly matter which decision is taken. There is no harm in extending the use of words like " existence " and " reality," so long as the different senses in which different types of " objects " may be said to exist are not confused with one another; and equally there is no harm in putting such things as the unconscious mind into the category of fictions, so long as one is clear about the scientific purpose which such fictions serve. What is important philosophically is the way in which statements about unconscious processes are related to the statements about behaviour by which they are tested. And to discover this we have to investigate the uses to which the statements about the Unconscious are actually put.

In both these examples, and in many others that could be given, the philosophical questions which arise depend upon the special characters of the scientific theories to which they are related. It would be idle to try to evaluate the Principle of Indeterminacy without considering the part that it plays in the quantum theory: a philosopher who disregarded the procedures of psycho-analysts could have little of interest to say about the unconscious mind. Nevertheless, the way in which a philosopher deals with such special theories will depend also on the attitude that he takes towards certain more general questions. Thus, as we have already seen, it makes a great difference to the philosophy of physics whether one accepts the claim that the meaning of physical concepts is given by the corresponding operations of measurement: different views of the Unconscious will be taken by those who adopt different criteria of reality. So, in various contexts,

it may have to be decided under what conditions a scientific statement is to be regarded as a convention: to what extent and in what sense it is required of a scientific hypothesis that it should be verifiable; what is meant by saying that one hypothesis, or theory, is better established than another; what is the logical status of inductive reasoning. These, and other such, questions do not relate to any particular scientific theory: the object-language to which they refer is not that of the sciences themselves but that in which we talk about the sciences. Accordingly, they may be treated as meta-linguistic questions of the second order. What makes them important is that they are concerned with the basic canons of the philosophy of science: the answers to them determine the ways in which all scientific theories are philosophically discussed.

II

One of the achievements of the analytic movement in philosophy during the last fifty years has been to dispel the mystery which was thought to attach to the propositions of formal logic and pure mathematics. It was generally agreed that these propositions were necessarily true, but the difficulty came in explaining how they could be known to be so. Why, after all, should the world be logical? How can we be certain that the laws of logic will never come into conflict with the facts? The answer is that it makes no sense to say of the world either that it is or that it is not logical. The only thing that can properly be described as logical, or illogical, is an inference from one statement to another. Logical inferences are those that we make in accordance with the laws of logic; and the laws of logic are rules for carrying out such inferences.

They cannot come into conflict with the facts, because in themselves they say nothing about facts. By applying laws of logic we can derive one true statement from another: but logic alone will never tell us that any descriptive statement is true. We have to learn that from experience. All that logic can tell us is that if some descriptive statement, or set of statements, is true, then some other descriptive statement must be true also. But suppose that the derived statement turns out to be false? In that case, either the deduction is invalid, or else the original descriptive statement is itself shown to be false. For in committing ourselves to the truth of a descriptive statement, we are committed also to the truth of everything that follows from it. But can we not decide not to be so committed? It is true that if we accept a given set of statements and at the same time reject one or other of their logical consequences, we shall be holding contradictory beliefs. But why, it may be asked, should we avoid self-contradiction? We are apparently assuming that the world is such that no pair of contradictory statements can both be true. But what right have we to make this assumption? And if we do make it, are we not thereby admitting that the truths of logic do depend upon material facts? For, on this view, it is the nature of the facts that makes contradictions inadmissible.

The answer to this is that there is no need to make any such assumption. The law of non-contradiction excludes the combination "p and not $-p$" where "p" stands for any proposition that you please. Its justification is that, given the ordinary interpretation of the negation sign, the expression obtained by combining a proposition with its contradictory has no factual content. It is not that the world happens to be so

constituted that one or other of two mutually contradictory propositions must be false. It is that *we so frame our language* that the conjunction of a proposition with its contradictory has no descriptive use. This does not mean, however, that the law of non-contradiction is itself to be construed as a descriptive statement about the customary use of words. By treating " not (p and not $-p$) " as a law of logic, we lay down a condition for the use of the negation sign: we make it a rule that, whatever statement we may choose to replace " p," the result of inserting it in the formula " p and not $-p$ " is not to be counted as describing any possible situation. This rule does indeed correspond fairly closely with the way in which negation signs are ordinarily used; but while this may be its pragmatic justification, it is not the source of its validity. What makes the law of non-contradiction valid is simply its status as a primitive, or as a derived, proposition in the system of logic in which it occurs. We might have a logic which disobeyed the law of non-contradiction in the sense that it allowed the formula " p and not $-p$ " to yield true propositions in certain cases. We might even have a logic in which there was a law of contradiction in the sense that " p and not $-p$ " occurred in it as a premiss, or as the conclusion of a theorem; and in that case we should have as good grounds for regarding this law of contradiction as valid as we now have for regarding the law of non-contradiction as valid. This sounds paradoxical, because one thinks of the negation sign as retaining the meaning that we ordinarily give it: and it is clear that if we interpret the negation sign in such a way that the law of non-contradiction is valid, the law of contradiction cannot be valid too. But the point is that the meaning of the negation sign is itself

determined by the " laws of logic " into which it enters. In so far as we abandon or modify the law of non-contradiction we prescribe a different use for the negation sign; we alter the meaning of negation. There is no question but that we are free to do this if we choose. The only question is whether it would be a useful procedure, whether a language which conformed to the principles of this unfamiliar logic would be equally convenient for the description of facts; and the answer to this question would depend to a considerable extent upon the character of the facts themselves. But to say that the facts may make it preferable to adopt one system of logic rather than another is not to say that the truth of any logical statement is established empirically. What guarantees the application of a truth of logic to the world is that no statement which conflicts with it is allowed to be even a candidate for empirical truth. As Wittgenstein put it: " It used to be held that God could create everything except what was contrary to the laws of logic. The truth is that we could not say of an ' unlogical ' world how it would look." [1]

The same considerations apply to the propositions of mathematics, whether or not it be the case, as Russell and Whitehead have tried to show,[2] that mathematics is formally reducible to logic. To some writers on the philosophy of science it has seemed mysterious that mathematics should play the part it does in the formulation of scientific laws; and it has even been suggested that the ability of scientists to make such a profitable use of mathematics indicates that the world was created by an expert mathematician. But the fact is that by means of pure mathematics alone

[1] *Tractatus Logico Philosophicus*, 3, 031.
[2] *Principia Mathematica.*

we can learn nothing about the world. The use of mathematics in science is to make a transition from one non-mathematical proposition to another. That some descriptive statement is true is an empirical, and never a purely mathematical, discovery; what we can learn by using mathematics, just as by using logic, is that if some descriptive statement is true, some other descriptive statement is true also. Thus it is for experience to decide whether, and in what domains, it is possible to give a physical interpretation to the axioms of Euclidean or Riemannian geometry. What is certain *a priori* is that if the interpretation is consistently carried out, whatever phenomena are found to satisfy the axioms will also be found to satisfy the theorems. And the reason for this is simply that if the phenomena in question turn out not to satisfy the theorems, all that is proved is that we were mistaken in supposing that they satisfied the axioms: we were mistaken in supposing that we had given the system of geometry a valid physical interpretation. For the criteria by which it is decided that such an interpretation is valid is that all the propositions of the system become empirically true. Thus, we may regard a certain trio of physical objects as forming the three end points of an Euclidean triangle; and having done this we may infer that the sum of the angles of this triangle is a hundred and eighty degrees. But when we come to measure the angles we may find that their sum is greater than a hundred and eighty degrees. It may then be suggested that our measurements are faulty; but if a repetition of them, or the use of other methods, yields the same result, this hypothesis, although it is not logically refutable, is likely to be excluded. What we should most naturally say in such a case is that the triangle was not Euclidean. This does not mean that Euclidean

geometry is itself in any way invalid; as a formal system its validity is independent of any matter of fact. All that is shown is that it is inapplicable to this particular case. In the same way, if we find that some physical process of addition—for example, the mingling of certain measured quantities of liquids—does not yield such a total quantity as the laws of arithmetic would lead us to expect, we do not conclude that those laws have been refuted. We say that the physical process in question is not a correct interpretation of mathematical addition. There is a sense, therefore, in which mathematical calculation can never lead us astray. It may indeed lead us to a false result; but that shows only that we started from false descriptive premisses. The use of formal logic and pure mathematics will not preserve us from scientific error: but we make it a rule that for whatever errors we may fall into, logic and mathematics themselves are not responsible. If the race is lost they are deemed not to have entered. And it is to this that they owe their necessary truth.

Our reason then for saying that the propositions of logic and mathematics are not empirical is that no experience is allowed to confute them: and by the same criterion we may divide the statements which pass for scientific laws into the two classes of empirical hypotheses and conventional truths. In general, it is not possible to tell merely by the form of a statement into which of the two classes it falls: we have to consider the use that scientists actually make of it. Moreover, since the meaning of words is not unchanging, a sentence which at one time expresses an empirical hypothesis may, with the growth of scientific information, be turned into the expression of a necessary truth. Thus, that the chemical composition of water is H_2O was at first an empirical discovery, but no

doubt there are many people for whom it is now a matter of convention: they would refuse to give the name of "water" to anything that was not so compounded. But what of heavy water which has the composition $_2H_2O$? Is it not really water? The answer is that you can say what you like. If heavy water really is a species of water, then it is not a necessary truth that water is H_2O. If this is to be a necessary truth, then we may find it advisable to invent another name for "heavy water." There is nothing at issue here except a question of verbal convenience.

The problem is not, however, always so simple as this. Consider, for example, the Newtonian law that the acceleration of a body is equal to the force which is acting on it divided by its mass. Is this an empirical hypothesis? The difficulty here is that it is not immediately obvious what we are to understand by "mass" and "force." If "force" is defined, in a way that is not unknown in text-books, as the product of mass and acceleration, then our law is plainly analytic: it is a straightforward consequence of the definition.[1] To give it empirical content we have to find some way of measuring force which is independent of the way in which we measure mass and acceleration. It would be generally conceded that this can be achieved in terms of counterpoise. To quote Prof. Bridgeman, "this crude concept," that is, the notion of force conceived in terms of muscular sensation, "may at once be put on a quantitative basis by substituting a spring balance for our muscles, or instead of the spring balance we may use any elastic body, and measure the

[1] This is not to say that the concept of "force" is altogether arbitrary. Its introduction is based on the empirical fact that the relationship between mass and acceleration is constant. This point is brought out in Poincaré's analysis to which reference is made below.

force exerted on it in terms of its deformation." [1] To generalize this sketch of a definition, we have then to " extend the concept of force, and say that any body accelerated is acted on by a force, and the magnitude of the force is defined as that which would have been necessary to produce in the same body the same acceleration with a spring balance in empty space." It is here assumed that all that is required for a definition of force is the specification of a procedure by which forces may be measured. And this assumption may be granted, with the proviso that the interpretation of such measurements, and even the possibility of making them, is usually bound up with a particular theory: so that there is a danger that the specification of the measuring process may itself involve the concept which the measurements are intended to define. But let us ignore this difficulty in the present context and allow that force can be defined without circularity by an operational method which does not rely upon the law that the acceleration of a body is equal to the force which is acting on it divided by its mass. Even so, it is not yet established that this law is an empirical hypothesis; for, while the concept of acceleration may be held to present no difficulties, it is by no means an easy task to disentangle the concept of mass from that of force. The suggestion made by Henri Poincaré, whose analysis I am following in the choice of this example,[2] is that by treating the equality of action and reaction as a convention, we can reach the conclusion that when two bodies A and B are acting upon one another their masses are inversely proportional to their accelerations; and this would be operationally sufficient if we could assume that A and B were each

[1] *The Logic of Physics*, pp. 102 ff.
[2] Vide *La Science et l'Hypothèse*, pp. 119–129.

subjected to no force but that exerted by the other. But since this assumption is not justified, we have to assume instead that the acceleration of A can be decomposed in such a way that we can distinguish the contribution of B, or rather of the force exerted by B, from that of other bodies; and this, as Poincaré shows, involves further assumptions concerning which it is at least questionable whether they can be experimentally verified. The result of this investigation therefore seems to be that we can treat this law of acceleration either as a necessary truth or as an empirical hypothesis; but that if we do interpret it in such a way that it becomes an empirical hypothesis, we shall probably find that we are then committed to treating certain other laws as *a priori* conventions. What is subjected to the test of experience is the Newtonian system as a whole: and within the system it is a somewhat arbitrary matter which elements are taken as conventional. We can always save one part of the system at the expense of another; we can even preserve the system as a whole, in the face of adverse observations, if we choose to make enough *ad hoc* additions to it. But there may come a point, as in the case of the Newtonian system itself, when such a course is found too inconvenient to be acceptable. Even so, the old system is seldom totally discarded. Its laws may still be regarded as valid for a special set of cases, and may even, with suitable amendments, be retained as necessary truths.

The thesis of conventionalism has perhaps been taken to its furthest point by Eddington. His basic principle, to which I see no call to take exception, is that " the physical universe " is to be defined as " the theme of a specified body of knowledge."[1] In other words,

[1] *The Philosophy of Physical Science*, p. 3.

instead of taking the notion of physical reality as primitive, and regarding the statements of physicists as more or less accurate descriptions of it, we are to start with the statements of physicists and conceive of " physical reality " only as *that which these statements describe*. This does not mean, of course, that every statement which occurs at any time in physics is bound to be true. It is simply that to talk of the physical universe as having such and such a character is equivalent to asserting a given set of propositions. If these propositions come to be rejected, then our conception of the physical universe is to that extent altered. We must not say that the physical universe itself is altered, because it is a convention that truth is timeless. To say that a proposition is now false is to imply that it always was so and always will be so, allowing, naturally, that the proposition may itself include a specific reference to space and time. But while it is strictly incorrect to say that the physical universe is that which is described by the physics of any given period, since that would formally exclude the possibility that the physics in question should turn out to be false, it is, nevertheless, true that there is no other way of characterizing the physical universe than by putting forward a set of physical statements; and to allow that these statements may after all not describe the universe correctly is to allow that grounds may arise for replacing them by others. It follows that the meaning we attach to " physical reality " is not independent of our conceptual system. Eddington himself employs the analogy of a fishing-net, where a fish is defined as anything that the net can catch. Thus what is counted as a fish depends not only on external facts, but also on the structure of the net. In this way it becomes a question how much of our physical knowledge is subjective, in

the sense that it is derivable from our choice of certain methods of description without any reference to the empirical facts. Eddington's own answer to this question is that it is very largely subjective. He even claims to be able to deduce from purely *a priori* premisses the number of protons and electrons in the universe. How far his claims are justified is a matter of dispute. There are logical grounds for holding that some of them are not. But what is more important in the present context is that, even if they can be justified, they do not, as he appeared to think, oblige us to accept an idealist philosophy. His results are obtained by taking certain principles to be conventionally true and by specifying certain processes of measurement as determining the reliability of physical observations. From this it follows, analytically, that no physical statement, which is itself consistent, can ever contradict these principles, and that no physical fact can be observed which does not conform to these processes of measurement. But what is yielded by this procedure is at best a definition of " physical statement " and " physical fact "; and it can hardly be maintained that the choice of these particular definitions is a " necessity of thought." Other definitions are conceivable, and might very well be adopted if our experience showed that these had outlived their usefulness. For it is a matter of fact, which cannot be determined *a priori*, that the use of a particular conceptual system is adequate for the description and prediction of the course of our experiences. So long as it is our only vehicle of description, we cannot describe anything except in terms of it; but this is not to say that it will always prove sufficient for everything that we may wish to describe.

The fact is that, with a little logical ingenuity, one

can turn any universal statement into an *a priori* truth. But while this device can make any scientific law secure from refutation, it does not genuinely add to the security of scientific knowledge. For the effect of making universal statements analytic is to widen the connotation of the terms which figure in them; so that more is required to establish that any given object is covered by the term. Thus, what the universal statements lose in empirical content, the corresponding existential statements gain. Suppose, for example, that we make it a convention, instead of an empirical hypothesis, that chimpanzees are Rhesus negative. In that case we can be quite certain that if anything is a chimpanzee it will be Rhesus negative, but it will then be correspondingly more doubtful whether any given animal really is a chimpanzee. For the grounds on which we might previously have satisfied ourselves that some animal was a chimpanzee are now insufficient; the condition of its being Rhesus negative has been added to them. If this condition proves not to be satisfied by animals which do satisfy the other criteria, we cannot say that any general hypothesis has been refuted; but, what comes to the same thing, we may find ourselves obliged to re-define our terms. Accordingly, the question is whether we prefer to have a vocabulary which allows for the establishment and rejection of general hypotheses, or whether we prefer to camouflage this procedure as an alteration in our use of language. If we had reason to suspect that such alterations would be frequent, the second of these alternatives would hardly seem attractive; so that it is likely to be chosen only at an advanced stage of a science. It is, therefore, not surprising that the thesis of conventionalism should first appear in the philosophy of physics. In the biological sciences it does not play a comparable role.

III

If a scientific statement is to be a candidate for material, as opposed to purely formal, truth, it must be capable in some way of being tested by observation. But here a distinction must be drawn between statements which are directly and those that are only indirectly testable. Thus, it would ordinarily be held that statements about hereditary traits, the deflections of compass needles, the overt actions of a man, the movements of certain microscopic bodies could be tested by direct observation, whereas statements about genes, the intensities of magnetic fields, a man's unconscious mental states, the movements of sub-atomic particles could be tested only indirectly. But how are we to decide into which of these categories a statement falls? It has been suggested that we are to say that a statement is directly testable if, and only if, the presence, or absence, of the state of affairs which it describes can, in suitable conditions, be determined by sense-perception, when the term "sense-perception" is understood to include memory and feeling. But the objection to this is that the truth of every scientific statement is in the end decided by sense-perception; we have still to distinguish the cases in which the decision is immediate from those in which it is not. In practice, the distinction seems to turn upon our use of language. We speak, for example, of perceiving tracks in Wilson cloud-chambers, but not of perceiving the movements of sub-atomic particles. Consequently, we say that statements about the cloud-chamber tracks can be directly tested, whereas statements about the movements of the particles cannot. But it is always open to us to alter our usage. Since there are recognized procedures for detecting the movement of

sub-atomic particles, there is no reason why, when these procedures are carried out, we should not say that the particles are perceived. In the same way, we could make it significant to say that we perceived a person's unconscious state and not merely his behaviour, or that we perceived not merely the deflection of compass needles and the like, but also, by such means, the properties of the magnetic field. Thus the distinction between what is directly and what is only indirectly testable appears, on this showing, to be very largely arbitrary.

Nevertheless, a relative distinction can be drawn which is *not* arbitrary. Whether or not we are able clearly to delimit the class of statements which are to be regarded as directly testable, we can at least say that some types of statement are more directly testable than others. For if we ask how a given statement p is to be verified, we may find that the only means we have of verifying it is through the verification of certain other statements q, r, s; on the other hand, it may not be the case that there is no means of verifying q or r or s except through the verification of p. And in such conditions we can say that p is less directly testable than q, r, s. It is in this sense that statements about the movements of sub-atomic particles are less directly testable than statements about marks on photographic plates, or that statements about unconscious mental states are less directly testable than statements about overt behaviour. In general, we may say that q is more directly testable than p if the occurrence described by q is a manifestation of the occurrence described by p, whereas the occurrence described by p is not a manifestation of the occurrence described by q. And if there are any statements in a given language of which it is true both that they describe observable occurrences

and that no other statements in the language describe occurrences which are manifestations of the occurrences that these statements describe, then they are of all the most directly testable. It is usual to call statements of this sort the basic statements of the language in question.

Since the only way in which any statement is actually verified is by someone's having some sensory experience, and since a sensory experience is not itself a manifestation of any other sort of occurrence, it follows that in any language which contains the means of formulating purely sensory statements, those statements will be basic. The concept of a purely sensory statement is, indeed, not free from difficulty. What is here to be understood by it is a statement which refers only to what is sensibly given at any particular moment: a statement which philosophers would characterize by saying that it described a present sense-datum. It is a feature of such statements that they have no extrapolative implications: if they are used correctly in the situations to which they refer, there can be no question of their not being true. Thus they can achieve a security that other empirical statements lack, but they do this at the cost of being uninformative: so much so that many philosophers regard them as degenerate cases. Such statements can always be introduced into a language, but it is not essential that they should be. A language which does not contain them may still be equipped to express all the propositions of any given science. If it is a descriptive language it must contain some statements which are counted as directly testable, but these statements may refer to physical objects rather than to sense-data. We understand them when we know what observations we have to make in order to be justified in asserting

them: and for this it is not necessary that we should be able to describe such observations otherwise than by making these statements themselves.

Whatever statements are taken as basic, there remains the problem of their relationship to statements which are less directly testable, statements which do not in the same way refer to what can be observed. The question is whether, in addition to the objects which are referred to by the basic statements, we are also required to postulate the existence of objects to serve as designata for statements of a higher order, or whether these higher-order statements can be regarded as referring to the same objects as the basic statements, albeit in a more complicated way. To some extent, I have already dealt with this question when discussing the status of the Principle of Indeterminacy and of the Unconscious Mind: and I then pointed out that it was a question of language rather than a question of fact. In certain cases we are not inclined to use a realistic terminology: for example, few people would wish to say that a political society had any reality apart from the individuals who composed it. But there are also cases in which the choice of a realistic terminology is the more natural. It would, for example, be generally held that sub-atomic particles did literally exist. It seems paradoxical to say that they are only scientific inventions, that they do not have as good a title to reality, even if it is not the same title, as the phenomena by which they are manifested. And the realist attitude is strengthened if sense-datum statements are taken as basic. For there is great reluctance to allow that all the impressive discoveries of science are, in the end, nothing more than complicated descriptions of actual or possible sequences of sense-data.

Now, as we have seen, there is no call to say that the

realist attitude is mistaken. We can extend our use of the word " existence " to any level that we please. But, *ex hypothesi*, the only way we have of establishing the existence of any higher-level object is through its manifestations. Were this not so, it would not be a higher-level object; the statements which referred to it would themselves be basic. And if we say that the existence of such an object is to be inferred from its manifestations, we must remember that the only way in which this inference can possibly be vindicated is by the appearance, in the relevant conditions, of further manifestations. Thus, the work that any higher-level statement does is always that of correlating phenomena which are more directly described by statements of a lower level. From the deflection of a compass needle I may infer the existence of a magnetic field: in stating this conclusion I am indeed doing more than merely re-describing my single observation of the needle: the reference to the magnetic field is intended to explain why the needle behaves as it does. But what the explanation consists in is the linking of this phenomenon with various other electro-magnetic effects, namely all those that are conventionally associated with the presence of magnetic fields. The inference which is made to look like the penetration of a disguise, the reading off from a mask of the features which it veils, turns out to be a passage from one mask to another. And this shows that the analogy of veiling is not here in place: for it is significant to talk of masks only when there is at least a possibility of removing them.

Nevertheless, it may also be misleading to say that what is meant by the higher-level statement is simply the set of its more directly verifiable consequences. For this suggests that higher-level statements can actually be translated into lower-level statements,

which is not, in general, the case. In most instances, the content of the higher-level statement is richer than that of any given set of lower-level statements; they may be held sufficient to establish it, but even so they are not necessary. Some other set would have done the work as well: and this in itself makes translation impossible. There is, however, a sense in which the phenomenalists, who wished to reduce everything to sense-data, may be allowed to have been right. The greater richness of the higher-level statements does not consist in anything other than their power to generate further lower-level statements. A theory is not just a shorthand description of the observations which it explains; but they alone give it its "cash-value," and there is a sense in which it is correct to say that it refers to them. In the same way, it is a mistake to insist, as the positivists once did, that every scientific concept should be explicitly definable in terms of what could be observed; but it is only to the extent that it is connected with what can be observed that any concept has a scientific use.

IV

The truth of basic statements is established directly by observation. It may also be established indirectly through the connection via a higher-level hypothesis of one basic statement with another. The truth of higher-level statements is established indirectly via the basic statements. Since these higher-level statements are not in general deducible from the basic statements which serve to establish them, their election to truth is in a sense provisional. Being empirical statements, they are not formally demonstrable: and the fact that they do not follow from the basic statements

means that they are always liable to be confuted. There remains always at least a logical possibility that further observations will be unfavourable to them. The same applies to universal statements at all levels: for it is a truism of logic that a universal statement is not entailed by any finite number of particular statements. There is no formally valid process of inferring from " some " to " all."

A conclusion which is often drawn from this is that no empirical statement, other than a direct record of a sensory experience, is ever certain. And if it is made a rule that a statement is to be considered certain only when it is either formally demonstrable, or a direct record of sensory experience, or else deducible from a finite number of such records, this conclusion is valid. But all the information that it gives is that there are higher-level empirical statements. To say that they are not certain is, in this usage, merely to say that they have the character they have. It is at best to call attention to the way in which they differ logically from necessary statements on the one hand and from a certain type of basic statement on the other: so that once we have given an account of what these differences are, the reference to certainty tells us nothing further. Moreover, there is a familiar sense in which it is proper to say that these empirical statements can be certain. They are correctly said to be certain when their truth is established beyond reasonable doubt. And in so far as the statement that they are all uncertain tends to obliterate the distinction between those that are and those that are not established beyond reasonable doubt, it is, I think, to be avoided, even though it is, in the sense intended, necessarily true.

In the same way, it is misleading to say that all empirical statements, with the possible exception of a

certain type of basic statement, are only hypotheses, or that they are only probable. We have a use for the word " probability " in the mathematical calculus of chances and in statistical studies, and in these uses the term can be adequately defined. But it is doubtful whether there is any satisfactory way of defining probability in the sense in which it is simply contrasted with certainty. Or rather, what would serve as such a definition is better described as a logical analysis of confirmation. What is required is a specification of the conditions in which the truth of one statement counts as evidence for the truth of another. This was, in fact, the task that Mill undertook when he set forth his canons of scientific method. His treatment of the problem was too narrow, and even within its limits not entirely satisfactory: nor do I think that anyone has yet succeeded in producing a wholly adequate solution of it. But the recent development of semantics offers at least a more promising approach.

If this problem were solved, we should be provided with a logical reconstruction of scientific procedure. We should not, however, have been given a reason why scientific procedure is to be trusted. If we are asked why we count one statement as evidence for another, our answer can be only that they are related in such and such a way, and if we are then asked why their being related in such and such a way should make one evidence in favour of the other, the only answer we can give is that that is what we understand by " evidence." For example, the fact that a generalization has been verified in a particular instance bestows some credit on it: it gives us some reason to believe it. Such evidence may be only weak: or in certain circumstances, it may be very strong. That depends upon the character of the generalization, and also upon

the conditions in which it has been tested. But the position of the sceptic is that he does not see why such a verification, or indeed any number of such verifications, should count as evidence at all. All that has been shown, he will object, is that a certain generalization has held good in the past: but is that any proof that it will hold good in the future? The problem of induction is not merely, as is sometimes thought, the problem of justifying inferences from the particular to the general. It concerns the validity of all non-formal reasoning. The question which it raises is whether we are entitled to accept anything short of formal demonstration as a ground for rational belief. What we are required to give is a proof that what is taken to be good evidence really is good evidence.

The advantage of putting the question in this way is that it makes it easier to see that it can have no answer. For in what could such a proof consist? Certainly, our conception of what constitutes good inductive evidence is not arbitrary. It canonizes the procedures that we have found to be reliable. But the trouble with the sceptic is that he will not accept this sort of answer. He wants a guarantee that what has hitherto been reliable will continue to be so. And then the best answer we can give him is that the assumption that a method which has hitherto been reliable will continue to be so has itself been reliable hitherto. But this again is an inductive argument: and because it is an inductive argument it does not remove the sceptic's doubts. Finally, it becomes clear that *nothing can remove them*. The sceptic's challenge is unreal because there is no conceivable way of meeting it. There is nothing that he is going to count as proof.

This is not to say that our standards of proof are sacrosanct. It is only that without some common

standard no argument is possible, as can be shown by an example. There are certain gamblers who make an eccentric use of what they deem to be the law of averages. Relying on the *a priori* truth that long runs are improbable if the chances are equally distributed, they wager on their being broken. Observing at roulette that red has come up several times in succession, they make this a ground for betting on black. Suppose now that someone were to apply this system to nature as a whole. He would assume that the fact that a generalization had been verified in a number of particular instances, so far from being evidence in its favour, was actually evidence against it. The law of gravitation, he would say, has had a very long run : I am therefore justified in expecting it now to fail. Could we prove such a man was wrong ? The answer is that we could easily prove it, but only by appealing to a standard of proof which he explicitly rejects. We can give our reasons for believing in whatever law it is that he questions, but he thinks that they are bad reasons. He thinks, indeed, that they prove just the opposite. And there the matter rests. All we can do is wait upon the event.

This does not show that the use of scientific method is irrational. On the contrary, it sets our standard of rationality ; what is irrational is not to use it. But in making it our standard we are taking a decision for which, in the nature of the case, no further reason can be given. And this means that no further reason need be looked for. The problem of induction is harmless just because it is insoluble.

STATISTICS

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Early Sources

ALTHOUGH the rapid progress and wide applications of statistical methods during the last thirty years have made the subject one peculiarly associated with the twentieth century, it is yet to be noted that its roots go back to the Renaissance, like that other great branch of applied mathematics, mechanics or dynamics; that Pascal and Fermat were contemporary with Galileo and Newton, and that the laws of chance were developed in relation to the problems of gaming almost simultaneously with the deterministic mechanical laws, which for some centuries seemed to menace the world with an iron necessity.

The two important tributaries, developed before the twentieth century, to the modern science of statistics and to the corresponding educational discipline are, on the one hand, the theory of probability, developing through de Moivre, Laplace, and Poisson on the mathematical side, through Thomas Bayes, Boole, and Venn on the logical, and reaching out to practical applications in life assurance and demography as the aristocratic pastime of gaming fell into moral disrepute.

A second parallel development, almost independent of the first, came to be known as the theory of errors. Here the emphasis was not upon uncertainty of expectation, but upon the practical business of summarizing and digesting a considerable observational record, of the kind which systematic observations in astronomy or the routine of surveying were already beginning to produce. Gauss's name is particularly to be associated with this development, the importance of which for

mathematical studies is none the less great, although the academic mathematician, developing the subject didactically, is all too prone to lay his emphasis on the more formal development of the theory of probability.

Galton and Statistical Biology

A man who, towards the end of the nineteenth century, played a peculiar part in precipitating modern developments was Francis Galton. A man of means and, had he chosen, of leisure, Galton made his name early in life as an African explorer. In 1869, evidently reacting eagerly to his cousin Charles Darwin's evolutionary theory, he had written *Hereditary Genius*, one of the most remarkable books of the century, and in it had demonstrated how apparently intangible concepts, at first vaguely apprehended, can be made quantitative and relatively precise by the collection and adequate presentation of statistical data. Throughout his life this possibility evidently exercised a fascination on his mind. In a crude way he attempted to collaborate in discussing the numerical results of his cousin's experiments with plants. He tried his hand at the statistical expression of meteorological phenomena, and, towards the end of his long life, armed with much experience, but without adequate mathematical technique, he became convinced that quantitative, and particularly statistical, methods were needed to consolidate Darwin's ideas, and to give confidence to their practical application. In Karl Pearson he found a man of boundless confidence and ambitious energy, and, with the sympathy of W. F. R. Weldon and his wide biological knowledge, Galton believed that a solid foundation could be built for a timely advance in the method and theory of biological research.

So far as Pearson's work is concerned, the immediate

outcome was the appearance from 1894 onwards of a series of extensive memoirs entitled *Mathematical Contributions to the Theory of Evolution.* In all there were twenty-six of these, the first twelve appearing in the *Philosophical Transactions* of the Royal Society. The title chosen must be taken to represent rather Galton's hopes than Pearson's performance, for Pearson was here exploring his own general concepts in mathematical statistics, such as skew-frequency curves, contingency, and the rather numerous statistics to which, without distinction, he applied the term correlation. These developments were accompanied by extensive mathematical tables to facilitate their use. Mendel's laws are discussed in the twelfth memoir, but only to be dismissed as inadequate.

A more enduring consequence was the foundation, in 1901, of *Biometrika: A Journal for the Statistical Study of Biological Problems.* For many years this handsomely produced quarterly was undoubtedly the centre of development of mathematical statistics in this country. It accepted papers from outside Pearson's laboratory and included some of the most important advances of a period of rapid progress. In building up the high reputation of this journal Pearson's labours as editor were constant and indefatigable and constituted the greater part of the scientific activity of his later life.

Although in following his own bent Pearson undoubtedly wandered far from Galton's intention, yet he may be regarded as ploughing the ground in preparation for later developments. That the huge mass of his writings have now little value must be ascribed to two circumstances—first, that his mathematics were on the whole clumsy and lacking in penetration and, second, that without the power of self-criticism he was unable and unwilling to correct his numerous errors or to

appreciate the work of others, which would have been of the greatest assistance. He seems to have regarded observational material principally as a means of illustrating his *a priori* concepts, not as a means of correcting them or as providing problems of interpretation in which statistical methods might be of service. He seems to have felt a contempt for the work previously done in the theory of errors and to have known little about it. He certainly regarded the skew-frequency curves he invented as improvements to be substituted for the normal law of errors.

The Emergence of Modern Statistics

It was in the study of experimental data that the characteristics of modern statistics first began to display themselves, notably in that series of methods which are known as the tests of significance. The logical situation behind these is exceedingly simple, though it has been seriously obscured in recent times by the elaboration of a highly sophisticated mathematical background. This, however, is unnecessary, and all the practical progress that has been made was achieved without its aid. In general, if, in connection with a given observational record, a hypothesis is considered which is well defined in the sense that from it can be derived definite expectations, we may use the observations to test whether these expectations have been realized, or whether, on the contrary, the observations depart so far from expectation in some relevant respect that the hypothesis under consideration must be deemed to be contradicted by the data, and must be abandoned. In the latter case the deviations from expectation are said to be significant, while in the contrary case, if the observations are such that with reasonable probability they might have arisen on the

hypothesis under test, this hypothesis, though not proved, has at least so far been confirmed, and, pending further and more stringent observations, may be accepted. Obviously this process of comparing observational data with more or less vaguely conceived hypotheses has been subconsciously inherent in experimental work from the time of its inception. The hypotheses conceived, however, by the active experimenter are more or less fluid, and his judgment from the results is personal and subjective. The possibility of an exact test of significance relative to a properly defined hypothesis, and to its necessary consequences in the frequencies of the different observational records which might have arisen from it—the possibility of an objective test appropriate to a given class of cases, and specifying the level of significance of the judgment imposed by the data—is of quite recent origin, and was first clearly exemplified by a paper published in 1909 by W. S. Gosset, writing under the pseudonym " Student." [1] One feature in particular should be borne in mind when modern tests of significance are spoken of as exact; namely, that the observational material is assumed to be finite, and may consist of only a small sample. Realism and practical applicability obviously require this. Yet it was the greatest obstacle to clarity of thought to pass from the vague and necessarily inexact approximations of " the theory of large samples " to a computational procedure appropriate to real cases in which the sample is finite. It is characteristic of this difficulty that when methods appropriate to " small " samples were developed, the protagonists of earlier methods should have exerted themselves to stress the smallness of the sample as a ground for disparaging the

[1] " Student " (1908). *The Probable Error of a Mean.* Biometrika, vi, pp. 1–25.

conclusions which could be based upon it. The study of sampling problems appropriate to finite, and therefore sometimes small, samples was inseparable from the development of exact and objective tests of significance, and the underlying mathematics consisted almost wholly in the realistic examination of such problems.

Student's Test

The particular test to which Student's name has been attached concerns the precision of the mean of a sample of observations, such as a series of analyses of random samples of a bulk, the average value of which is of interest. The kind of information which such a series of analyses can give may be made clear by dividing the whole series of possible or hypothetical values of the unknown average into two classes, one of which is contradicted by the data, while the other is consistent with it, at a defined level of significance, or of stringency of the est. Such a subdivision is achieved by a test of significance by which we can determine whether or not any chosen hypothetical value is rejected by the test. Since the hypothetical value chosen is known we may subtract it from each observation and reduce the problem to that of testing whether a given sample from a normally distributed population is or is not consistent with the population having zero for its mean.

If the population sampled were of known variance it was well known that the sampling distribution of the mean of any number, N, of observations was a normal distribution with the same mean, and the variance divided by N. The probability of our mean exceeding by chance the value observed would then be easily calculable, and we might fix our level of significance by saying that deviations were acceptable if they fell within the limits fixed by definite values of the prob-

ability of falling outside these limits—*e.g.*, a not very stringent but convenient level of significance would be fixed by choosing a deviation of a magnitude which would be exceeded in one direction or the other just once in twenty trials. This comes to 1·96 standard deviation. The method therefore in use in the century before the appearance of Student's paper was to obtain as good an estimate as possible of the variance of the population sampled and to apply this calculation on the assumption that the value estimated was in fact correct.

A good, and, indeed, the best possible, estimate was available in Bessel's formula

$$s^2 = \frac{1}{N-1} S(x - \bar{x})^2,$$

in which x stands for any observation of the series, $\bar{x}$ for their mean or average value, S for summation over all the values of the sample, and N for the number of observations. Consequently, the permissible limits at the 5 per cent level of significance would on this method be given by

$$\pm \frac{s}{\sqrt{N}} (1{\cdot}96).$$

The important thing to notice here is that the statement that s^2 is the best possible estimate of the unknown variance is not the same as the statement that the variance is in fact equal to s^2. The estimate s^2, like the mean value of $\bar{x}$, will be subject to errors of random sampling, and will sometimes be greater and sometimes less than the true value. It is not obvious, and was not obvious to the mathematical traditions of the nineteenth century, in what way this circumstance should affect our conclusions.

What Student perceived was that although the

sampling distributuion of $\bar{x}$ must depend on the unknown variance of the population sampled, yet that of the ratio

$$t = \bar{x}/s\sqrt{N}$$

must be independent of this unknown variance. Its distribution would depend only on the known size of the sample N, and Student set himself to determine what its actual distribution might be. Using an extremely crude mathematical approach he arrived in fact at the correct distribution, and showed that the frequency was not proportional to

$$e^{-\frac{1}{2}t^2},$$

which would have justified the traditional method, but rather to

$$\left(1 + \frac{t^2}{N-1}\right)^{-\frac{1}{2}N},$$

a distribution for which the frequencies of extreme values fall off much less rapidly than for the normal distribution.

Student gave a simple table for evaluating the probability that t should fall outside any chosen limits. Since then fuller tables have been made available, so that it is easy at any chosen level of significance to find the corresponding limiting values of t. For ten observations, for example, the 5 per cent limits are not $\pm$ 1·960, but $\pm$ 2·262.

It will be noticed that the use of Student's criterion completely removes all doubt or embarrassment due to the size of the sample being finite and the estimate s^2 not exactly equal to the true variance. It is in this sense that the modern tests of significance are spoken of as " exact," in contradistinction to the approximate tests developed without this refinement and commonly

referred to as derived from "the theory of large samples."

This example has been set out in full, not only because its point has been missed in many expositions, but because it displays very simply the intimate integration of the whole class of advances which make up modern statistical methods. It was mentioned in passing that Bessel's formula supplies the best possible estimate of variance available from the data, and this at once raises a series of questions—How is it known to be the best? In what sense is it the best? What criteria exist for judging one estimate to be better than another?—questions which it is the function of the Theory of Estimation to answer.

Again, Student could have had no success without recognizing and solving a problem of a type at that time almost untouched—namely, the problem of the exact sampling distribution of quantities calculable from samples. These problems of distribution looked exceedingly formidable in Student's time, and although since then many have been solved it is still true to say that where there is doubt or difficulty it is usually because the relevant problem of distribution has so far baffled analysis.

Thirdly, if we express Student's t in the form

$$t = (\bar{x} - \mu)/s\sqrt{N}$$

where the unknown mean, μ, of the population sampled has been introduced explicitly, it is seen that t, the sampling distribution of which is known merely from the size of the sample, is expressible as a function jointly of the observed quantities $\bar{x}$ and s, and of the unobservable parameter μ. It constitutes an example of what have been called pivotal quantities, by which we can pass equally to probability statements about $\bar{x}$

and s for given μ, or about μ, given $\bar{x}$ and s. These latter are known as statements of fiducial probability, to avoid confusion with the statements of inverse probability, which are possible when certain *a priori* knowledge is available—*e.g.*, as to the frequency with which different values of μ shall occur in a bulk, such as has been sampled, but which cannot be derived without presuming (or assuming) such knowledge *a priori*. It is the quality of fiducial probability that it is available in the absence of knowledge *a priori*, and as an obvious consequence that its meaning or logical content must be different from that of any statement of inverse probability. Its validity is derived from the complete independence of all unknown or hypothetical elements, which the pivotal distributions enjoy.

Problems of Distribution

A class of mathematical problems which came into prominence with the initiation of more critical methods of statistics are known as problems of distribution. From the observational data, whether frequencies or measurements, quantities can be calculated aimed at representing properties of the distribution sampled. In the case of measurements there are characteristically symmetric functions of the observations. The whole class of functions calculable from the data are known as " statistics." If we know, or are given by hypothesis, the distribution of each individual observation, and know also that these several distributions are independent of each other, though expressible in terms of one or more unknown parameters, it is theoretically possible to deduce the sampling distribution of any chosen statistic, so as to determine the relative frequency with which it would fall within specified limits were the sampling process repeated indefinitely from

like material. Although this theoretical possibility is obvious, such problems appear at first sight to be extremely difficult of explicit solution. Their exact solution in an increasing number of important cases has supplied a powerful stimulus to the demand for exact tests of significance, and to the development of an adequate theory of estimation.

Speaking generally, problems of distribution can be approached by three methods. The first, which the subject owes to somewhat similar researches in statistical mechanics, consists in the geometric representation of the sample by a point in generalized Euclidean space, having as many dimensions as there are observational values. The entire sample is thus represented by a single point, and the frequencies with which samples of all possible kinds will appear in the random sampling process is represented by a frequency density in the manifold space employed. For any chosen statistic, if we assign it a particular value, the samples which will lead to this value will fall generally on a continuous sub-space, so that the generalized space may be regarded as subdivided in a series of slices by chosen values of the statistic. The total frequencies found in these regions gives the frequency distribution of the statistic in question. In simple cases, for example, space may be divided by a series of parallel planes, or by a series of concentric hyperspheres, and the frequency distributions, such as those of $\bar{x}$ and s^2 in Student's problem, may be found with little difficulty. This geometric method has indeed been found more successful than any other, and to it are due the majority of the exact solutions now known.

A second method is to proceed by " mathematical induction " from a sample of given size to a sample containing one additional observation or set of observa-

tions. If the distribution is known for a sample of size N, it may be possible to infer, knowing that the next observation is independent of those that precede it, the distribution for size $N + 1$, and so to obtain the general formula.

A third method, using a more advanced type of mathematical analysis, is based upon what is known as the characteristic function. It has been widely discussed and is of great theoretical simplicity, but so far has not led to the resolution of difficulties recalcitrant to other methods. If x is a variable quantity of known distribution, then for any real value of t it is demonstrable that the average value of e^{itx} exists mathematically, and has an absolute value not greater than unity. Regarded as a function of t this is known as the characteristic function. Given the characteristic function, the corresponding frequency distribution may be inferred by an inverse process involving integration.

Now, although the distribution of a given statistic calculable from a sample of observations offers a problem requiring some penetration and insight, yet it is possible without consideration to write down formally an expression for its characteristic function in terms involving a finite number of processes of integration. Consequently, by using the inversion mentioned above, we have here a method of the utmost generality for solving problems of distribution.

The Theory of Estimation

During the long period in which the exact sampling distributions of the statistics commonly in use were unknown and thought to be unattainable in practice, the properties of such distributions could not be used as a means of comparing the merits of such different methods of statistical reduction as might suggest them-

selves. Indeed, the belief was expressed that it was impossible to compare the merits of two methods of estimation without reference to the particular purpose for which the estimates were required, and that the notion of a best estimate was only a subjective illusion.

As soon, however, as statistics are regarded as having knowable sampling distributions, a number of comparisons suggest themselves as representing unquestionable *desiderata* in any quantity used in estimation. In the first place it will usually be true for any appropriate statistic, that, as the sample from which it is drawn grows larger and larger, its range of distribution will grow narrower and narrower without limit, so that it is said to converge in probability, in the sense that there is a limiting value T_∞, such that the probability of the absolute value of the deviation of the statistic from this limit exceeding any chosen quantity ϵ, however small, shall tend to zero. If this is true, then the limiting value will depend only on the population sampled, and will necessarily be some function of the parameters by which this population is characterized. The estimate is said to be consistent when the statistic T is used as an estimate of this particular function of the parameters and not of any other. We may say, then, that T is a consistent estimate of a parameter θ if in large samples it converges in probability to the limit θ. Although the use of inconsistent estimates is not unknown in some special cases where the error involved is exceedingly minute, yet I believe the desirability of satisfying this criterion has never been challenged. In what follows it will be assumed that the estimates spoken of are consistent estimates.

In all cases of practical importance it is found that as the sample is made larger without limit, the form of the distribution of the estimate about its limiting value

tends to assume that of the normal law of frequency of error discovered by Laplace and Gauss, and that the variance of the distribution then falls off inversely to the size of the sample. In other words, if v is the variance of the distribution and N the size of the sample, the product Nv tends in general to a finite limit. Evidently this limit measures in an inverse sense the precision of the estimate under discussion, at least in large samples. The second criterion that has been developed, known as the criterion of efficiency, is that this limit shall be as small as is possible. Statistics satisfying this second criterion as well as the first are said to be efficient. Since the minimum of the limiting value of Nv is directly calculable from the data of any problem, it is comparatively easy to test whether any proposed estimate is efficient, and if not to recognize that it must be capable of improvement. The recognition of this criterion swept away the claims of methods of estimation which had been most confidently advocated about the beginning of this century. The utility of this advance was the greater since it was shown that an efficient estimate in single or simultaneous estimation could always be obtained by the method of maximum likelihood, and that, starting with any consistent method, routine calculations would give the numerical values with whatever precision might be desired. On the practical plane our second criterion provides, therefore, a comprehensive solution of the problem of estimation.

On the theoretical side the problem at this point is still far from closed. Many different efficient estimates may be proposed. These will, indeed, tend to equivalence as the samples are made larger, but for any finite sample they will differ in value. They will differ also in the form of their error distributions, and evidently

one may be preferred to another. The analysis, however, of the criterion of efficiency points the way to a further means of discrimination. It has been pointed out above that the limiting value of the product Nv in large samples has a minimum which does not depend on any chosen method of estimation, but on the sampling properties of the data. This property specifies in an inverse sense how valuable each unit of the original data was for the purpose of estimation in view. In fact, the reciprocal of this minimum limit is defined as the quantity of information provided by each unit of the data, or in simple cases by each original observation.

Now, given the exact sampling distribution in a finite sample of any proposed statistic, it is equally possible to ascertain the quantity of information which a single value of this statistic will provide, and to compare this quantity with that provided by any alternative statistic derived from a sample of the same size. The notion of efficiency, based originally on the limits for large samples, may thus be extended to the comparison of statistics derived from finite samples. It may be shown that the amount of information in an estimate can never exceed the finite amount of information supplied by the original data, consequently the criterion of efficiency when extended to small samples implies merely that the loss of information, if any, incurred in the process of statistical reduction, shall be made as small as is possible.

A particular class of statistic was early shown to satisfy a condition more stringent than that of the criterion of efficiency—namely, that no loss of information whatever is incurred. In such cases the statistic is said to be sufficient. What this means in practice is illustrated by a further property of sufficient statistics—namely, that if T is a sufficient statistic and

T' any other statistic whatever, then the simultaneous distribution of T and T' in random samples shall be such that for any chosen value of T the distribution of T' shall be wholly independent of the value of the parameter θ to be estimated. In other words, as soon as we know the value of T, then the value of T' is completely irrelevant to the estimation of θ, and supplies no information whatever about it. Since, in the case of sufficient statistics, this is true of all alternatives which can be proposed, we are using our words consistently when it is said that the sufficient statistic contains all the information about θ originally present in the data. When sufficient statistics exist thay can be found by the method of maximum likelihood, and in such cases we have a method of estimation which is satisfying not only practically but also theoretically.

It will be noticed that sufficient statistics do not exist in all cases, and that their existence depends on the functional form of the problem. To complete what is here said on the theory of estimation, it should, therefore, be added that in a number of cases in which sufficient statistics, in the strict sense, do not exist, yet it is possible to render estimation exhaustive, and to avoid all loss of information, by the use of what are known as ancillary statistics, which possess the properties that while they themselves have random sampling distributions independent of all unknown parameters, yet that they, together with the estimates obtained by maximum likelihood, form an exhaustive set. The use of ancillary statistics does not alter the estimate arrived at, but does alter its sampling distribution, and thus supplies additional information which, without their use, would be lost.

The logical form in which the information supplied by observational data about unknown and hypothetical

parameters or about observations not yet actually made, should be expressed, deserves close attention. In the early work of Thomas Bayes, published 1763, a serious attempt was made to bring the problem within the scope of the theory of probability, so that on the basis of the observations, statements of probability could be made as to the value of the hypothetical parameters. It appears that, in general, it is not possible to set the results accurately within this framework of ideas. Bayes' approach was accepted somewhat uncritically by Laplace as the basis of his theory of inductive inference, but from the middle of the nineteenth century a succession of writers, beginning with Boole, pointed out that this approach is open to criticism and leads to contradictions. These criticisms appear to be unanswerable, and the theory of inverse probability, originating with Bayes, is now almost universally abandoned. The existence of pivotal quantities having the properties explained above, with distributions independent of all unknowns, and expressible in terms both of observable statistics and of one or more of the unknowns, does, however, allow probability statements about these unknowns to be inferred from the known distribution of the pivotal quantity. Statements derived in this way are known as statements of fiducial probability, and the corresponding distributions of the parameters as fiducial distributions. In practical applications these are often referred to as providing confidence limits.

Information and Experimental Design

The development of the precise quantitative notion of the quantity of information transformed the statistician's task in two ways. It had been his business to make the most of the data available. So long as grossly defective methods of doing so were current, and recom-

mended by the highest authorities, it was natural that precise or reliable results obtained should stand to the credit of the statistical method employed; in the contrary case, if no tolerably precise results were obtainable, the statistician presented his work with a somewhat apologetic air. When, however, it became manifest and easily demonstrable that any finite body of numerical data contained only a finite amount of information, and that methods of extracting either the whole, or very nearly the whole, of this had been made available, it became obvious that so far as precision was concerned, the statistician as such had no responsibility. His task was fulfilled, if he correctly assessed the amount of information, and presented his results embodying the whole of it relevant to the questions under discussion. At this stage his task resembles that of a chemist making an assay, and it would be absurd for him to be ashamed if the assay is low, or elated should it prove to be high. The precision of the results and their value for all purposes are inherent in the body of numerical data originally available.

Relieved of one responsibility, statistical work is in the position to undertake another. The methods by which quantitative information is assessed reveal also to what particular features of the observational record its limitations are principally due. We are in a position to estimate how many observations and of what kind would be needed to give results of any required precision. It is this very fundamental change of outlook that has turned the attention of so much modern statistical work in the direction of the design or planning of observational records, and of experimental arrangements. Most modern text-books of statistics deal now explicitly with some aspects of experimental design with reference, in particular, to the needs of

quantitative biology. The history of the art of experimentation is, of course, a long and complicated one, and although it is now most readily apprehended by the student through the exposition of formal principles, yet these have come relatively late in its historical development. The driving force and urge to improvement has lain in the diverse enterprise of thousands of workers exercising their curiosity and ingenuity in overcoming particular difficulties. In such fields as biology, agriculture, psychology, it early became evident that the technique of experimentation traditional in the so-called exact sciences of physics and chemistry was not sufficiently penetrating. Exploration of knowledge in the biological fields required in particular *controlled* experiments—*i.e.*, experiments in which it is not the absolute values of the observed quantities which are of primary interest, but comparisons between these. The principles of the complex subject of biological control were, however, very hazily understood and, for more than a generation, the word " control " was used in an almost reverentially mystical attitude.

Need for control arises from the experimenter's consciousness that he is ignorant of innumerable causes which may affect his experimental results. It is for this reason that controlled experimentation was first developed and refined in sciences which did not think of themselves as " exact." If plants are grown in a greenhouse, the experimenter is aware that the conditions in which they are grown may not be the same as those prevailing in other years, or in other greenhouses. He prefers, therefore, to rely on comparisons between groups of plants grown together in the same greenhouse. If he wishes to know whether the addition of an element, such as boron, to the solution in the water culture affects the growth of plants, he will make

sure not only that a number of his plants are treated with boron, but also that a number grown in parallel with them shall be without this addition. The object of his experimental comparison is to make sure that if a significant difference does appear in the growth of the two groups of plants, it shall properly be ascribed to the addition of boron ; and also, what is a different problem, to make sure that if boron is in fact capable of affecting appreciably the growth of the plants, his experiment shall be competent to detect this effect. Obviously, also, if there are many different questions in the mind of the experimenter, more complex systems of control may need to be elaborated. Although traditionally a particular group of experimental units such as the plants receiving no boron have been spoken of as " controls," in contradistinction to the other groups of plants in the experiment, yet in the design of the experiment, and in the treatment of the results, no special role is played by this control group, for all should be thought of interchangeably as controlling one another.

Naturally, from the beginning of controlled experimentation a great deal of care has been given to render the circumstances and treatment of the different groups of experimental units closely alike in all respects save those under test. It has only more recently been realized that differences in environment between units assigned to the same treatment are equally of vital significance to the success of the experiment, and to rendering it capable of an unequivocal interpretation. At its simplest this general principle arises from the fact that we can only judge of the significance of the difference in performance of groups of units treated differently by comparison with similar differences in performance between units treated alike This being so, it became obviously imperative for the validity of the comparison

that the physical conduct of the experiment shall ensure that the very same uncontrolled causes of diversity which affect units treated differently shall equally act between units treated alike. The modern experimenter, therefore, takes care that nothing in his experimental arrangements shall allow unknown causes of diversity to affect either group of comparisons without equally affecting the other. The most general means to effecting this is by what is called randomization.

A simple example may be used to show what randomization amounts to. In one of his experiments Darwin compared the growth rates from germination of two groups of maize seedlings, one from self-fertilized and the other from cross-fertilized parents. Darwin arranged his seedlings in pairs, one from cross-fertilized and the other self-fertilized, growing each pair side by side in the same pot. He took the greatest care that these two plants should be comparable in, for example, the date of germination. It is probable that all the seedlings from self-fertilized plants were in a line along one side, whereas those from cross-fertilized plants were in a parallel line along the other side of the pots, perhaps one line was on the east and the other on the west. Since it is not known *a priori* which side would be favoured, owing, for example, to differential insolation, or to the convection currents circulating in the greenhouse, the comparison between one group and the other was fair or unbiased, in that neither was known to enjoy any advantage from such causes. On the other hand, such causes are not known not to exist, and if they exist, they will affect all the plants in one line differentially from their opposite numbers in the other line. There was, therefore, in Darwin's experiment the possibility of a cause affecting differentially comparisons

between the different groups of plants, without affecting equally the differences between the relative performances of the different pairs. All that the statistical examination of the results can ascertain is that the difference between the two averages of fifteen plants each is rather greater than could be ascribed to chance, judging from the variation among the fifteen differences in the fifteen pairs of plants. The statistical analysis does not, therefore, exclude the possibility that this apparently significant result was really due, in part at least, to the sites assigned to the two treated groups.

If, however, randomization had been applied in the experimental design, the members of each pair would be assigned to the west or east side of their pots at random, as by tossing a coin. Any significant difference between the performances of the two contrasted groups would then be known to be ascribable to the effect of self-fertilization. Randomization is a device designed to ensure that the laws of chance used in testing the significance shall be validly operative in the physical conduct of the experiment.

To summarize briefly the steps by which statistical studies have gained their present level of usefulness, we may say that during the present century we have learnt

(i) To conserve in its statistical reduction the scientific information latent in any body of observations.

(ii) To conduct experimental and observational inquiries so as to maximize the information obtained for a given expenditure.

In accomplishing these tasks of immediate utility, it has incidentally furthered the task of experimental

science by incorporating the vast amorphous body of research experience, which has been accumulating for centuries, in principles of scientific inference which can be taught to students. The art of adding to natural knowledge by experimentation is no longer the " mystery " of a craft, but, in a sense in which it has not been before, part of the heritage of a scientific education.

ASTRONOMY

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ASTRONOMER ROYAL

UNTIL the close of the eighteenth century astronomy had been concerned mainly with the study of the positions and motions of the celestial bodies. Observation of their physical features was limited to the mapping of the Moon's surface and to spasmodic observations of sunspots. In the latter part of the century, the researches of William Herschel began to open up many new lines of investigation ; for the first time the stellar universe was studied *per se*. He obtained the first rough ideas of the structure of the Galactic System, published long lists of nebulæ and star clusters, speculated on the nature of nebulæ, proved that many close double stars were physical binary systems, and made the first attempt to establish a photometric system. During the nineteenth century there were further developments : the first determinations of stellar distances were made ; the spiral structure of some of the nebulæ was discovered ; the periodicity of sunspots was established ; double-star astronomy was placed on a firm foundation ; the application of the spectroscope to the celestial bodies began to provide information about their physical nature and, in particular, about their composition ; helium was discovered in the Sun, while still unknown on the Earth ; a start was made with the classification of the spectra of the stars ; the gaseous nature of many of the galactic nebulæ was established ; stellar spectra were used to provide information about the line-of-sight velocities of the stars.

But, despite these advances, when we look back at the state of astronomical knowledge at the dawn of the twentieth century, it seems very primitive. The

distances of only a few stars were known with certainty, and the realm of observation was in large measure limited to the near vicinity of the Sun—within, say, a few thousand light-years. It was not known whether the spiral nebulæ were members of our Galaxy or were island universes in space beyond it. There was no certain knowledge of the physical nature of the stars, nor was anything known of their age, of their evolution, or of the source of their energy.

The revolution in outlook that has been brought about during the past fifty years has resulted in large measure from the application of photography and from the construction of large reflecting telescopes. Photography began to be applied to astronomy in the last decade of the nineteenth century. It has many advantages over visual observation. It effects an enormous saving in time at the telescope, faithfully recording in a relatively short time a vast mass of detail. The photographic plate stores up faint impressions, so that by increase in length of exposure fainter and fainter objects are recorded, until a limit is reached when the plate is fogged by the natural brightness of the night-sky. A single photograph can give the positions and magnitudes of many thousands of stars, or the positions and intensities of hundreds of lines in the spectrum of a celestial object. Comparison between photographs of the same star-field made at different times gives information about the changes that have occurred in the interval—the movements of stars and other objects, and their changes in brightness. By using emulsions of different types, information can be obtained about the colours of stars, and hence about their temperatures. Photography has, in fact, become a most powerful and indispensable tool in astronomy, and has almost entirely superseded visual observation.

In order to explore space to remote depths, telescopes of great light-gathering power or, in other words, of large aperture are essential. The first important advance in this direction was made by William Herschel, who constructed reflecting telescopes with mirrors of speculum metal, his largest being the 40-foot telescope of 48 inches aperture. The great reflector erected in 1845 by Lord Rosse at Birr Castle, which had a 72-inch speculum mirror of 54 feet focal length, was the largest of this type ever made. Large disks of speculum metal are difficult to cast and to figure; being liable to tarnish, they require frequent repolishing and refiguring. Because of these disadvantages and because improvements in optical glass were making the construction of increasingly larger achromatic objectives possible, the reflecting telescope fell into disuse, and for the greater part of the nineteenth century was supplanted by the refractor. There was a progressive increase in the size of refracting telescopes, culminating in 1897 with the 40-inch refractor of 63½ feet focal length of the Yerkes Observatory. A telescope of this size is unwieldy in use and limited in its applications.

The discovery by Liebig of a simple chemical process for depositing a thin film of silver on a glass surface, and the development by Foucault of methods of figuring and testing glass disks, made the silver-on-glass mirror possible. As glass is easier to cast and to figure than speculum metal, the way was prepared for the construction of mirrors of greater aperture than any hitherto used. The modern large reflecting telescope with glass mirror belongs essentially to the present century. Its perfect achromatism makes it superior to the refractor for many purposes, while it has the advantage of flexibility, as observations can be made at

the prime focus, or at the Cassegrain focus, or at the coudé focus, thus providing a wide range of equivalent focal lengths.

At the beginning of the century the largest mirror which had been made of glass had a diameter of 60 inches. In 1918 the 100-inch reflector of the Mount Wilson Observatory was completed. Then, in 1927, the great project for the construction of a 200-inch telescope was embarked upon ; the Hale telescope, as it has been named, has just been brought into use. Meanwhile five other telescopes with apertures from 69 inches to 82 inches have been brought into operation, and a further five are planned or are under construction, including a 120-inch telescope for the Lick Observatory and a British 98-inch telescope, to be erected at the new site of the Royal Greenwich Observatory at Herstmonceux. A satisfactory rate of progress in the provision of telescopes of great light-gathering power, suitable for the observation of faint objects and for obtaining the spectra of stars with high dispersion, is therefore being made. In recent years a film of aluminium, deposited by vacuum distillation, has replaced the silver film as the reflecting surface, having a higher reflecting power than silver in the ultra-violet and being free from liability to tarnish.

At the same time a new lease of life has been given to telescopes of moderate aperture, particularly to the refractors of medium size, which were the standard instruments at many observatories fifty years ago, by the development of photo-electric methods of observation. The application of electron multipliers has given a great increase in sensitivity, and has made it possible to determine stellar magnitudes with very high accuracy down to a low limit of magnitude. In this particular field of work, the photo-electric cell gives an accuracy

superior to that obtainable with the photographic plate.

The conventional reflector with a paraboloidal mirror, when used for direct photography, has a very limited field of good definition, coma becoming large at a small angular distance from the axis. A great step forward was made in 1930 by Schmidt, who showed that a large field of good definition can be obtained by using a spherical mirror in combination with a clear glass plate in the plane of the centre of curvature of the mirror, the plate being figured aspherically so as to correct the spherical aberration of the spherical primary. The focal surface of this combination lies on a sphere whose radius is the focal length, so that the photographic plate must be constrained to fit this surface. The Schmidt telescope has proved admirably adapted to the study of stellar distribution, the structure of the Galaxy, and the obscuration of light from distant stars by interstellar matter, and is being widely used.

Improvements in photographic emulsions have been a great help to the astronomer. The increase in the speed of plates has made it possible to shorten exposures, or, in other words, it has effectively increased the apertures of telescopes, while emulsions of special types have greatly extended the range of wavelengths that can be photographed. Plates sensitive to infra-red light make it possible to photograph giant red stars at very great distances, while for the analysis of spectra the increased range of spectrum that is available has proved to be of the greatest value.

Perhaps the most striking development in astronomy during the past century has been the great increase in knowledge about the scale of the Universe. The first accurate determinations of stellar distances were made in the latter part of the last century with the heliometer,

a slow and tedious process. The application of photography reduced the time at the telescope, simplified the reduction of the observations, improved the accuracy of the results, and made it possible for determinations of stellar distances to accumulate rapidly. The distances of several thousands of the nearer stars have now been measured. For stars more than about 750 light-years away, the parallactic angle upon which the determinations are based is so small that it is comparable with the uncertainty of its measurement; the individual distances then cease to have much value, though collectively they are useful for statistical purposes. An extension to much greater distances has been made possible by an indirect method, which determines the intrinsic brightness of a star by means of certain features in its spectrum which are sensitive to luminosity, the criteria that are used being calibrated by means of stars whose distances have been directly measured. Knowing the star's intrinsic brightness, its distance can be inferred.

A still more powerful method for determining the distances of remote objects has been based on the special class of variable stars called Cepheids, after the type star Delta Cephei. The variation in brightness of these stars is caused by a regular pulsation, the period of which may be only a few hours or may be a few weeks. In 1912 Miss Leavitt found that the periods of Cepheid variables in the Large Magellanic Cloud were closely correlated with their apparent magnitudes; as these stars were all at effectively the same distance, this meant that there was a correlation between the period and the intrinsic brightness. The zero point of the relationship is fixed by means of the nearby Cepheids whose distances are known. The relationship can then be used to infer the intrinsic luminosity of any Cepheid

variable merely by determining the period of its light variation. This result is of great importance, as all the Cepheid variables are highly luminous stars, which are visible at very great distances. The dimensions of the known Universe at once became greatly enlarged, for distances of remote objects in which Cepheid variables could be detected, such as star clouds in the Milky Way or globular clusters, could be inferred with a relatively small uncertainty.

The method involves one assumption—that distant stars are not dimmed by any interstellar matter or, in other words, that space is transparent. For any dimming will reduce the apparent brightness of the star and the distance inferred from the intrinsic brightness will be too great. It has been proved in recent years that interstellar space contains matter consisting of both atoms and of small dust particles. The dimming by this matter, whose density is far below that of the most perfect vacuum which the physicist is able to produce, has been estimated in various ways. The first estimates of distance inferred from the observations of Cepheid variables have required some correction because of this dimming, but the method is now believed to give reliable results.

Two important conclusions soon followed from the use of Cepheid variables as standard beacons. For the first time it became possible to form a reasonably accurate estimate of the size of the Galaxy. The special class of objects called " globular clusters " made this possible. These clusters all contain a number of Cepheid variables, enabling their distances and positions in space to be determined. Instead of being strongly concentrated towards a plane, as are the stars and interstellar matter in the Galaxy, they are distributed more or less at random throughout a spherical

volume which is co-extensive with the Galaxy. The conclusion is reached that the Galaxy has a diameter of about 100,000 light-years, the Sun being at a considerable distance—about 30,000 light-years—from its centre. This excentric position of the Sun explains the peculiar distribution of the globular clusters, most of which are found in one half of the sky. Earlier investigators from Herschel onwards had concluded that the Sun was at the centre of the Galaxy ; the reason was that obscuration by the dust clouds limited visibility in all directions in the Milky Way to the same distance. Because the globular clusters are not concentrated towards the Milky Way they can be seen in spite of their very great distances, as their light does not have to pass through a great length of obscuring matter.

The second important result which came from the study of the Cepheid variables was the proof that the spiral nebulæ were extra-galactic systems, settling a long controversy as to whether they were or were not galactic objects. Observation had shown that their distribution over the sky was not uniform, their numbers decreasing steadily towards the galactic plane, which, it was argued, proved their galactic nature. The discovery of Cepheid variables in several of the nearer of these objects enabled their distances to be derived. The distances were so great that there no longer remained any doubt that these nebulæ are far beyond the bounds of our Galaxy and that they were, indeed, island universes in space. The reason why the observed numbers fall off towards the Galaxy is that those in low galactic latitudes are dimmed or completely blotted out by the obscuring matter near the galactic plane. The nearest of the external galaxies, the Great Nebula in Andromeda, proves to be at a distance of 750,000 light-years.

Further information about the Galaxy was obtained from the study of the motions of the stars in the vicinity of the Sun. In one direction the stars are found to be moving faster than the Sun, and in the opposite direction to be moving slower. The Galaxy, in fact, is rotating, but not in the way that a solid body, such as a cartwheel, rotates. It rotates under the gravitational attraction of its parts, and primarily of a massive central nucleus, so that the rate of rotation increases towards the centre of the system and decreases in the opposite direction. The centre is in the direction of the Sagittarius star cloud, the brightest region of the Milky Way. At the distance of the Sun, the period of rotation is about 200 million years and the velocity about 150 miles a second. The total amount of matter in the system can be estimated; the mass proves to be about one hundred thousand million times the mass of the Sun. About half of this material consists of discrete stars, the other half consisting of the atoms and dust particles which form the interstellar matter.

The next step was to determine the distances and dimensions of the island universes. The distances of the nearer systems can be directly determined by means of the Cepheid variables which are found in them. Their dimensions prove to be of the same order as those of our own Galaxy. Like it, they are in slow rotation, enabling estimates of their masses to be obtained, which are in general agreement with that of our Galaxy. For systems which are so remote that individual stars cannot be observed in them, distances were estimated on the assumption that the galaxies are all of the same size. When measures were made of the line-of-sight velocities of many of these systems, the surprising result was found that they are all receding from us and that there is an extremely close proportion-

ality between the distances and the velocities of recession. Whatever the interpretation of this result may be, it has enabled the distances of very remote systems to be inferred with very considerable precision from the measures of the displacements of the lines in their spectra.

The picture which is thus obtained of the Cosmos—the Universe of Universes—is a truly awe-inspiring one. The most distant universes which have been observed are so far away that their light takes about 500 million years to reach us. While the light has been on its long journey, the Earth has seen great changes. Races of animals have appeared, passed through their evolution, and disappeared, giving place to new types, which have in turn disappeared. The face of the Earth has been changed many times, as mountains have been uplifted, gradually denuded away by the action of wind, rain, and water, to be followed by new uplifts. The light had almost reached the end of its long journey before man appeared on the Earth. In the volume of space accessible to present observation, it is estimated that there are about a hundred million of these great universes, and at the extreme limits of the observable region there is no evidence that they are thinning out in their numbers. The new 200-inch telescope will enable space to be probed to a distance of a thousand million light-years ; it will be of great interest to await the results of this extended survey.

The observed red-shifts of the extra-galactic nebulæ, interpreted as velocities of recession, have given rise to the theory of the expansion of the Universe. At this stage our investigations become tied up with the properties of space and the cosmological results that follow from the theory of relativity. According to the views of Einstein, space is held in equilibrium by the matter it contains. With the right amount of matter,

the total curvature in the whole space is just enough to close it up into a universe that is finite though unbounded. But Lemaître has shown that this arrangement could not be permanent, for it is unstable. Such a universe must start either to expand or to contract. An alternative suggestion is that the interpretation of the displacements to the red of the lines in the spectra of the extra-galactic nebulæ as velocities of recession may not be correct; but that the energy of the quanta of light may steadily and uniformly decrease during their long journey through space. There is a hope that the observations with the 200-inch telescope may make a decision between these alternatives possible.

Observations during the last half century have not merely furnished us with a picture of the structure of the Universe; they have also provided a pretty good understanding of the nature of a star. Knowing the distance of a star and its apparent brightness, its intrinsic brightness can be determined. From measures of its colour, the surface temperature can be inferred; as the star radiates very much like a black body, the brightness per unit area of its surface is determined by its temperature, and hence the total surface or the size of the star is found. The mass of a star can be derived only through the gravitational effects which the star exerts, and so it is from the twin systems—the visual double stars, the spectroscopic binary systems, and the eclipsing binary systems—that information about the masses must be obtained. The observed data—mass, size, temperature, and luminosity—must be related to one another by theoretical considerations.

The development of a theory of the internal constitution of the stars was due primarily to Eddington. A star is held together by its gravitational attraction, but opposing this is the pressure of the radiation

streaming outwards from the intensely hot interior of the star, and tending, like a mighty rushing wind, to blow the star apart. Eddington developed the mathematical consequences and compared them with observation. It was known that some stars were of great size and of low mean density—the giant stars—and that others were of small size and with mean densities greater than that of water—the dwarf stars. Eddington expected that the perfect gas laws would be obeyed in the former stars, but not in the latter. He found, on the contrary, that the dwarfs fitted the theory just as well as the giants, so that the gas laws were obeyed by them also, despite their high densities. The explanation, when he suggested it, was obvious: the temperatures within the stars are so high (about 20,000,000° K. at the centres) that every single atom is completely ionized—all the electrons are stripped from the nuclei. A mixture of nuclei and electrons can be compressed to densities much greater than that of normal matter in the solid state and the gas laws will still hold.

Eddington's theory provided an explanation of the paradox of the companion of Sirius and of the other stars in the small group of so-called " white dwarfs." When the mass and the size of the faint companion of Sirius were determined, it was found that the mean density of the star was about one ton per cubic inch, which, being many times greater than the density of platinum, seemed absurd. But when we conceive of a mixture of nuclei and atoms being jammed closer and closer together, densities far higher even than that of the companion of Sirius are possible. The properties of matter in this state are of great interest in theoretical physics.

One important result from Eddington's investigations was the derivation of a theoretical relationship

between the mass and the luminosity of a star. Eddington collected all the relevant observational data and found that they conformed to it; in other words, if the luminosity of a star is known, its mass can be assigned, and conversely. This result has an important bearing on the question of the evolution of the stars. The well-known Russell–Hertzsprung diagram, connecting the spectral types and luminosities of stars, had been believed to indicate an evolutionary sequence; it was thought that a star started as a red diffuse giant, contracted under its gravitational attraction, becoming hotter and bluer, but that after a certain stage had been reached, further contraction caused a progressive cooling and reddening, accompanied by increase of density. The mass-luminosity relationship tells us that a red giant is much more massive than a red dwarf. As, according to the theory of relativity, mass and energy are synonymous, a star must continually lose mass as it radiates and the rate of loss of mass can be calculated. The Sun, for instance, is losing mass at the rate of about four million tons a second. If, then, a red dwarf has been formed by evolution from a red giant, it must have lost the greater part of its mass in the process. Has such a decrease in mass occurred?

In this connection, the time-scale of stellar evolution is of importance. There have been diverse views on this question. Twenty years ago a controversy was still in progress between the advocates of a " long " time-scale (of about a million million years) and those of a " short " scale (of about ten thousand million years). Opinion has gradually converged in favour of the short time-scale: the evidence in support of it has been derived from the study of moving clusters of stars, of the statistics of binary stars, of the approach to equipartition of energy among the stars, and of various other

phenomena. In ten thousand million years the proportion of its mass lost by a star is quite small. Hence the Russell–Hertzsprung diagram does not give any information about stellar evolution ; it merely indicates a locus of steady states. The age of the Earth and the ages of meteorites, determined by the radioactive method, are found to be a few thousand million years, and therefore of the same order as the ages of the stars. We can find no evidence of anything in the Universe older than about ten thousand million years. What is the meaning of this? We can imagine that the Universe consisted originally of matter distributed practically uniformly. There was then no change, or at the most an almost infinitesimally slow change, so that time, as it were, stood still. Under such conditions time has no meaning. Eventually condensations of matter gradually formed and changes began to take place, which developed at a rapidly increasing rate. It is to that epoch that our estimates of the ages of the stars and of the Universe must be referred.

At the beginning of the century nothing was known about the source of stellar energy. The old view of Kelvin and others that the Sun obtained its energy from gravitational attraction gave a lifetime far too short for the geologist to accept. The discovery of radioactivity seemed at first to provide a possible source of energy, but that, too, proved to be inadequate. Atomic energy was the only source that could maintain the radiation of a star for thousands of millions of years. There were two main alternatives to be considered: annihilation of matter by the fusion of protons and electrons might occur; it is conceivable that a star could then in the course of its lifetime radiate a large part of its mass; or there might be a building up of helium out of hydrogen, which can result in a loss of

mass of less than one per cent. The temperature at the centre of a star is not nearly high enough for annihilation of matter to be possible, and it is now accepted that the main source of stellar energy is the building up of helium out of hydrogen atoms. The mechanism of the process by which this occurs has been made clear by Bethe. The helium atoms are formed by a cycle of nuclear reactions in which both carbon and nitrogen are involved; the atoms of these elements behave as catalysts and emerge from the cycle unaltered. The conclusion that the mass of a star has not appreciably changed during its evolution is in harmony with the results already described.

Though much can be inferred about the interior of a star by mathematical investigation, the features that can be studied spectroscopically are merely superficial. There is a great variety in the spectra of different stars, but the majority of stellar spectra can be arranged in a single continuous linear sequence, correlated with the colours and temperatures of the stars. Though the change in the spectrum from one point in the sequence to the next is slight, the spectra of the red stars are totally unlike those of the blue stars. The physical explanation of the close relationship between the temperature of a star and its spectrum was given in 1921 by Saha, who applied the principles of thermodynamics to the process of ionization. The degree of ionization depends mainly on the temperature, but also to a much smaller extent on the density; there are consequently slight spectral differences between the spectra of giant and dwarf stars of the same temperature, providing, as already explained, a powerful method for determining the luminosity of a star.

By the detailed analysis of the spectrum of a star information can be obtained about the relative abun-

dances of different elements. There is found to be a close similarity in composition between the stars and the Sun. Hydrogen and helium are by far the most abundant of all the elements; all the other elements together amount only to about one per cent of these two. There is also a close similarity in composition between the Sun, the Earth, and meteorites, for the metallic elements; but the Earth and the meteorites are deficient in hydrogen and helium; these light elements have escaped away into outer space, because the gravitational control was not enough to hold them. The interstellar matter seems also to have a composition similar to that of the Sun and stars. This suggests either that the stars were originally all formed out of the same primeval cosmic matter, or else that the relative numbers of atoms of different kinds is the result of some process of atom building, which has either reached the same stage in the various stars or has possibly attained some final and stable state.

Astrophysical observations have greatly helped in the theoretical interpretation of spectra. In laboratory investigations spectral lines associated with transitions from a metastable state of an atom—a state in which an atom will remain for seconds or minutes before emitting radiation—are not observed, because the energy is removed from an atom in such a state by collision long before the atom is ready to offload it. Such lines are accordingly called *forbidden* lines. But in many celestial objects the density is so low that the atoms can exist in metastable states. When spectral lines which had not been observed in any terrestrial source were first obtained in celestial spectra, they were attributed to unknown elements, following the precedent of the discovery of helium in the Sun before it was found on the Earth. Thus the gaseous nebulæ were supposed to

contain an element *nebulium*, and the Sun's corona to contain *coronium*. With the advance in the knowledge of atomic structure it was realized that there were no gaps in the atomic series to be filled by unknown elements, and therefore that the lines of unknown origin must be produced by atoms of known elements under unfamiliar conditions. The nebulium lines are now known to be forbidden lines of doubly ionized oxygen, while the coronium lines are forbidden lines of highly ionized atoms of iron, nickel, and calcium.

The knowledge of the spectrum, structure, and brightness of the corona had been slowly gained during the few brief moments of totality when the Sun was completely eclipsed, by expeditions sent at great trouble and expense to distant parts of the world. The corona is so faint that its light is drowned by the light scattered by the atmosphere in the near vicinity of the Sun. The study of the corona was revolutionized when Lyot proved that by suitable methods it was possible to observe it without waiting for an eclipse ; the principle is to make observations from a high altitude, above the dust layer, so that only molecular scattering occurs, and to use an instrument of special design, in which an artificial eclipse is produced by obscuring the Sun, special precautions being taken to exclude scattered and diffracted light. It is important that the optical components should be of the highest perfection, with freedom from scratches, bubbles, striations, and other defects, and with high polish. With the aid of the coronagraph, Lyot was able to discover new lines in the spectrum of the corona and to study its changes of structure in relation to other solar phenomena.

A new branch of astronomy has been opened up in recent years, which might be called micro-wave astronomy. It is found that the Sun is sending out

electromagnetic radiation on wavelengths from a few centimetres up to about 15 metres. From time to time there is a great increase in this radiation, lasting usually from about 50 to 60 minutes. This increase coincides with the appearance of a bright eruption or flare on the Sun. The phenomena of these flares have been intensively studied during the past fifteen years, because of the associated effects on radio-transmissions; the development of the flare coincides with a sudden radio fade-out on short-wave radio transmissions over the sunlit hemisphere of the Earth. The radio waves are normally reflected back to earth by the ionosphere; at the time of an intense flare there is a great increase in ultra-violet emission; the ionized layer is lowered, and the radio waves are absorbed in the denser atmosphere instead of being reflected. Accompanying the flare, electrically charged particles are shot out from the Sun with a speed of about a thousand miles a second; if they encounter the Earth, about 24 hours later, they come under the influence of the Earth's magnetic field, spiral inwards, and give rise to displays of the aurora and to magnetic storms.

Sudden bursts of radiation, of very brief duration, are also observed. These bursts do not occur simultaneously on all wavelengths in the wave-range involved. They no doubt originate at different levels in the solar atmosphere, but further investigation is required to elucidate their origin.

Besides the electromagnetic radiation from the Sun, it is found that radiation of a similar nature is coming from certain regions of the Galaxy. The precise source of the radiation is still uncertain because of the limited resolving power of the equipment used. From some regions the intensity of the radiation is constant; from other regions it varies rather rapidly. The

evidence at present available indicates that the radiation comes from localized regions, possibly from either individual stars or nebulæ.

Radio methods have proved valuable in the observation of meteors. A strong radar reflection is obtained from the ionized cap of air in front of a rapidly moving meteor. The advantage of the use of radar is that observations can be made in cloudy weather or in the day-time, when visual or photographic methods cannot be used. Meteors which are too faint to be observed visually can be detected by radar. The method therefore opens up the possibility of adding considerably to the knowledge of meteoric phenomena.

While there has been, as we have seen, a remarkable addition to our knowledge of the stars and of the Universe during the present century, more has been learnt also about our fellow members of the solar system. The size of the system was extended by the discovery of a new planet beyond Neptune, which has been named Pluto, in 1930, while six satellites of Jupiter, a fifth satellite of Uranus, and a second satellite of Neptune have been discovered in the course of this century. We have learnt much about the conditions on the other planets and about their atmospheres. It has been found that Venus is devoid of oxygen and of moisture, but has an atmosphere that contains a great quantity of carbon dioxide; this seems to indicate that there has been no development of plant life on Venus. Mars has been proved to possess a tenuous atmosphere, which contains small amounts of moisture and of carbon dioxide, but oxygen has not yet been detected. Seasonal changes in colouration and changes in the appearance of some areas from one season to another strongly suggest that there is some sort of vegetation, which, by the nature of the reflections from it, appears to resemble

lichens rather than more highly developed forms of plant life. The major planets have very extensive atmospheres, which are rich in methane and, for Jupiter and Saturn, in ammonia also, but which contain no free oxygen, nitrogen, or carbon dioxide; hydrogen and helium must be present in great abundance, the strong gravitational pull of the giant planets having prevented the escape of these light elements. It seems that, apart from our Earth, Mars is the only planet on which any sort of life can exist, and there it seems to be almost on the verge of extinction.

Many theories have been propounded to explain how the solar system was formed. None of them is without its difficulties. Several are of the catastrophic type, which suppose that the planets have been formed from matter that initially constituted a part either of the Sun or of a supposed former companion of the Sun; they all fail to explain how matter drawn out from the interior of a star, under great pressure and at extremely high temperature, could condense and aggregate into planets before it had dissipated away into space. There are other theories which suppose that the planets were formed by the gradual evolution of a primordial system. A theory of this type, recently advanced by Weizsäcker, which emphasizes the importance of hydrodynamical considerations, appears to be the most promising of all the theories of the origin of the solar system, though it is not altogether free from objections. The problem of how our solar system was formed still remains a challenge to astronomers. Until it is solved it must remain largely a matter of conjecture whether planetary systems are the exception or the rule; and therefore also whether life is widespread throughout the Universe or whether it occurs only under a special combination of circumstances.

PHYSICS

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Introduction

NINETEENTH-CENTURY physics had seen the growth of our knowledge of matter in the simple gaseous state and also the coalescing of our ideas of the nature of light and of electric waves in the electromagnetic theory. On the other hand, physics in the twentieth century has been characterized by the growth of our understanding of matter in the solid state and of its fundamental structure. This resulted from the investigation of the atom and the discovery of a large number of fundamental particles from which the whole of the material world appears to be built. With the examination of the action and behaviour of these particles has come a new knowledge about the laws obeyed by them—laws which have now been called the basic laws of science. The scope of this investigation ranges from the very small, on the sub-atomic scale, to the extremely large on the cosmological scale: from the cold of absolute zero to the extreme heat of atomic explosions. The discovery of these new laws has provoked what can only be described as a revolution in scientific thought itself, and concerns ideas which we all tend to take for granted as a matter of common sense: these are our ideas of *space* and *time*, *cause* and *effect*, and of *determinism*. This great change has been brought about by the development of atomic physics in the new era opened up by those three great discoveries at the end of the last century: of X-rays by Röntgen in 1895; of the electron by J. J. Thomson in 1897; and of radioactivity by Becquerel in 1898. In fact, twentieth-century physics really started in 1895,

a year which clearly separated the new from the classical in physics.

The new radiation, called X-rays, was shown to be excited when cathode rays (electrons) were stopped by matter—a process rather analogous to the emission of a sharp sound when a stream of lead shot strikes a hard floor. The rays manifested themselves by exciting a fluorescent glow on materials like zinc sulphide, and this effect stimulated inquiry into many aspects of phosphorescence. Later, in the hands of Bragg, X-rays were to become a most important tool in the investigation of the crystalline structure of matter and the structure of complex molecules. It was the discovery of a sort of phosphorescent effect produced in a uranium salt which led Becquerel to the conclusion that the uranium atoms were actually emitting a radiation of their own. This phenomenon was called radioactivity, and in the investigation of this, Madame Curie discovered radium.

By noting that the emanations from radium discharged an electrometer in the vicinity, Rutherford found rays of two types : a strongly ionizing type which were absorbed in a few centimetres of air—the α-rays, and a more penetrating but less-active type called β-rays. Later, in 1898, Villard discovered a third type of extremely penetrating rays which he called γ-rays. By bending α- and β-rays in electromagnetic fields, Rutherford established that α-rays were, in fact, streams of helium atoms, each carrying two positive charges, shot out of the radium at prodigious speeds : the β-rays he found to consist of streams of extremely fast electrons. Results such as these led Rutherford to the view that the phenomena of radioactivity were actually manifestations of the natural disintegration of radium atoms.

Sir J. J. Thomson's famous experiments demonstrated the existence of fundamental units of electricity —electrons—which were constituents of all atoms, and they were followed by an enormous amount of work carried out in attempts to elucidate the properties of these electrons. By bending streams of electrons (cathode rays) in electric and magnetic fields, J. J. Thomson had also found the ratio of the electron charge $(-e)$ to its mass m, so that m could be calculated when e was known. The values found showed that the electron was an incredibly light particle (10^{-28} gm.); in fact, there is no lighter particle known. Now, J. J. Thomson had calculated on the electromagnetic theory that moving electrons appeared to be associated with an extra mass, because when the electron moved energy had to be added to the associated electromagnetic field. Further, if the radius of the electron were about 10^{-13} cm. this additional mass would be enough to account for all the measured mass, so that on this view there would then be no necessity for attributing to the electron any intrinsic mass, like that of an ordinary body. J. J. Thomson also established that the anode rays from a discharge tube were positively charged particles which were always associated with atomic masses; hence the smallest positive particle was the positively charged hydrogen atom called the proton. This fact was in some ways disconcerting, for to an otherwise symmetrical scheme of things it introduced a characteristic and fundamental asymmetry.

Later Rutherford and Soddy showed that the emission of an α-particle meant the loss of two units of positive charge, while the emission of a β-particle meant the loss of one unit of negative charge. As these changes correspond to the formation of atoms

with different chemical properties radioactivity thus consisted in a natural transmutation of the elements. Soddy also established that the weight of an atom did not precisely determine its chemical properties—*i.e.*, its exact position in the periodic table—as might be deduced from the approximate periodic classification which had been set up by chemists. Thus there existed atoms of identical chemical properties, yet having different weights ; such atoms are called isotopes, and, strangely enough, the existence of some such particles had even been suggested by Sir William Crookes as long ago as 1886. The α- and β-rays from radium proved in the hands of Rutherford and his associates a most powerful tool for investigating atomic properties. These particles were high-speed projectiles which could be used to find how atoms behaved in collisions. However, these particles were so small that they could not possibly be seen by ordinary light (light waves are too long and would sweep over such particles just as ocean rollers sweep over small rocks). On the other hand, the paths of destruction produced by these high-speed atomic projectiles as they crashed their way through a gas could be made visible, and this was done by an ingenious device introduced by C. T. R. Wilson in 1911. Wilson knew that water vapour in a gas tends to condense into little droplets, provided some nuclei are present. In the atmosphere of industrial districts, dust usually occupies this role, and the result is a formation of mists or fog. In a dust-free gas, however, charged gas atoms (ionized atoms) can also act as nuclei : consequently, when an atomic projectile, say an α-particle, shoots through a gas, ionizing the atoms in its path, the ions produced can act as nuclei on which water drops can form. If these drops are now suddenly illuminated and photographed,

a record is then obtained showing the track of the ionizing particle as a thin line. In other words, these tracks are the vapour trails formed on the debris produced in the path of atomic projectiles.

This Wilson chamber, by producing records of the tracks of colliding particles, enabled not only the tracks of colliding particles to be examined, but also any atomic changes which might result from such collisions to be readily demonstrated. Consequently, this was an instrument which had a most profound effect on the development of atomic research in the twentieth century.

One of the first great results of the use of this instrument in the hands of Rutherford and Geiger was to show what sometimes happened when the fast and massive α-particle collided with an atom. The cloud-chamber tracks showed that while in most cases an α-particle crashed through the atoms in a very thin metal foil almost in a straight line, there were rare occasions when the track of the α-particle was deviated through a large angle, and in some cases the direction of motion was almost reversed. Rutherford has himself described this discovery as quite the most incredible event which had ever happened to him in his life. It was almost as incredible, he said, as if you fired a 15-inch shell at a piece of tissue paper, and the shell came back and hit you. He at once realized, however, that this backward scattering must be the result of a collision of an α-particle with an atom, and could have been produced only if the whole mass of the atom were so highly concentrated that the α-particle could approach to within incredibly small distances of the atom. This conclusion gave Rutherford the idea of the nuclear atom—an atom in which practically the whole mass was concentrated at a minute central

nucleus carrying a positive charge, and around which the negative electrons must somehow be disposed, probably in orbits on an analogy with the planets rotating round the sun. Then, by considering how close α-particles could approach a nucleus, Rutherford estimated the radius of the nucleus and the charge it carried. It turned out that the size was extremely small (about the size of an electron), and the charge carried on the atoms of the various elements was approximately proportional to their weights.

The Rutherford Nuclear Model. These achievements now provided the facts upon which was built a theory of the full structure of the atom. The outstanding problems to be explained related to the nucleus and to the extra-nuclear arrangement of circulating electrons. For the nucleus, it was necessary to account for its generally stable structure, and also for its instability in the case of radioactive atoms which emitted the α-, β-, and γ-rays; the emission of these rays took place for no obvious reason, and the law of emission appeared to follow the laws of chance rather than any strict mechanical rules. As for the rest of the atom, the edifice of electron orbits must be stable, and also be able to account for the emission of the radiations observed (spectra). As the experimental data about protons, electrons, α-rays, β-rays, the nucleus and its charge and mass appeared to be at that time fundamentally simple, it was thought that the development of a complete theory of an atom could be obtained by applying the known and simple laws of mechanics to these fundamental atomic particles. Even so, it was realized that the task was likely to be a laborious one, if only because the spectra emitted by some atoms were extremely complex; for example, iron emits over

50,000 spectral lines in the visible spectrum. Nevertheless, it was thought possible to account for these spectra somehow on the basis of the mechanics of the two fundamental particles: electrons and protons. In fact, the age-old question of the nature of matter and the century-old question of the nature of electricity appeared to be nearing its answer.

Since the mass of the electron may be neglected compared with that of the proton, the mass of the atomic nucleus must be equal to that of all the protons in it. But as the nuclear charge is much less than that of all the protons, there must therefore be a certain number of electrons present in order to neutralize some of the positive charge. The fact that the nucleus contains electrons and protons is in accordance with the phenomena of radioactivity, in which α-particles (composed of four protons) and β-particles (electrons) are sometimes emitted from heavy nuclei.

Now, very exact measurements of the masses of various atoms showed that these were less than *exact* multiples of the proton mass. This mass defect is explained, however, by one of the two non-classical theories introduced into atomic physics in the early years of the century, namely, the Theory of Relativity. On this theory, mass can be regarded as a form of energy, and the mass defects then represent the energy changes which occur when the particular nucleus is formed from the constituent parts (protons and electrons). There appeared to be no difficulties presented by this extremely simple and therefore attractive view of nuclear structure. Nevertheless, this theory was later to be rejected as many other elementary particles were discovered.

When, in 1919, Rutherford bombarded atoms of the light elements with the high-speed α-particles, he

found that the atoms were broken up, emitting a proton in the process, and this appeared to prove that the protons must be a constituent of the nucleus. When atoms of nitrogen were bombarded with α-particles, Rutherford found that atoms of *oxygen* were produced as a result of the collisions: this was the first instance of the *artificial transmutation of matter*, and these experiments were a landmark in the history of the investigation of the nature of matter. The energy released (through relativistic conversion of matter into energy) in such experiments is, however, comparatively small because only a few atoms are broken up. It was only when a chain reaction involving all the atoms in a considerable quantity of matter was produced, about twenty years later, that any large amount of energy was released by the break-up of nuclei, and this effect has been demonstrated experimentally in atomic-bomb trials.

The Bohr Atom Model and the New Physics. It was soon realized, however, that any simple structure consisting of an electron rotating in an orbit around a central nucleus could not explain any of the observed facts of atomic spectra on the basis of the generally accepted laws of physics. Maxwell's electrodynamics showed at once that such an electron should emit waves—*i.e.*, light—thus losing its energy, and falling into the nucleus. In order to resolve this dilemma, Niels Bohr brought into atomic theory an idea which was completely foreign to classical physics. He made the basic assumption that inside the atom classical electrodynamics did not apply to an electron, which he then envisaged as circulating round and round the nucleus in a characteristic orbit without radiating or manifesting its presence at all. Then, to account for

radiation by the atom, Bohr, in 1913, postulated that radiation occurred only when an electron changed its characteristic non-radiating orbit; and in order to introduce some rule into this otherwise strange behaviour, Bohr looked to the quantum theory of Planck. This latter theory had actually been proposed many years earlier in 1900, in an attempt by Planck to derive a formula capable of describing the distribution of heat energy among the various wavelengths of radiation emitted by a hot body. Previous attempts to derive a satisfactory formula based on the accepted laws of classical mechanics had failed. Planck had then made a bold suggestion that radiation was not emitted continuously by an oscillating electron, as envisaged by classical electrodynamics (the Hertzian oscillator), but in discrete packets of energy which he called quanta. This theory, like the special Theory of Relativity proposed by Einstein in 1905, was for many years little understood and less accepted, as it was completely opposed to the basic laws of mechanics, which, for 200 years, had so readily fitted the facts of everyday experience.

The theory proved the key to unravelling the complicated tangle of most atomic spectra by reducing them to some sort of order. This was done by abandoning altogether attempts to make the extra-nuclear electrons subject to the general laws of motion of the macroscopic world of everyday life.

Quite arbitrary rules of thumb (selection rules) were introduced to regulate the behaviour of the electron. The key to its motion seemed now to be a set of four numbers, called quantum numbers, whose values and changes were rigidly restricted by the selection rules. Although this whole concept of the arrangement of electrons in the atom was in marked contrast to the

accepted basic laws of mechanics, there was no question whatever of the success of the idea. Not only were X-ray and optical spectra explained, but a meaning was also given to the periodic table, which appeared now as a trivial consequence of a simple rule stating that the same four quantum numbers could not apply to two electrons. The theory had great success not only in spectroscopy: in its later forms it proved very fruitful for understanding the phenomena of magnetism, and also in explaining the properties of metals and the nature of the chemical bond.

During the first quarter of the century a large quantity of experimental data had been accumulated about the behaviour of electrons in the free state—*i.e.*, not bound inside the atom. Townsend had shown some years previously that a certain definite energy was required to remove electrons from an atom (ionization). He also showed that fairly fast electrons in collisions with atoms bounced off them elastically, just like a ping-pong ball in collision with a much heavier billiard ball—a result which was in accordance with classical mechanics if the electron was regarded as a small particle. On the other hand, an effect, discovered about the same time in the early 'twenties by Townsend in Oxford and by Ramsauer in Berlin (that slow electrons could sweep over large atoms of argon almost as if they did not exist), was quite contrary to the idea which regarded an electron as a localized particle. Again, it was an established fact that light waves could be scattered by tiny particles and produce a halo (as, for example, when a lamp is viewed in a mist) and a similar phenomenon was obtained when X-rays were scattered by small bodies (*e.g.*, the atoms in a crystal). However, if a beam of electrons is scattered by small particles (atoms in a metal foil), the halo (diffraction

pattern) is produced exactly as if the electron stream were a beam of waves: such equivalent X-ray and electron patterns were produced by G. P. Thompson in this country and by Davisson and Germer in the U.S.A.

These, and other similar facts are completely inexplicable on the assumption that an electron is a localized tiny particle, obeying the classical laws of mechanics; but they are consistent with the idea of the electron as an extended influence in space just like a wave motion. This latter view is in accord with a revolutionary theory of matter introduced in 1925 by de Broglie, which regarded a particle of matter as associated with a characteristic (perhaps hypothetical) wave motion (ψ-waves) the wavelength of this motion being inversely proportional to the momentum of the material particle—a conclusion which, incidentally, readily explained why diffraction effects became important with slow electrons (low momentum), and could not be observed with larger particles such as billiard balls or bullets of everyday life (high momentum). In this way, a certain duality was attributed to matter; it appeared to possess contrary properties—that of being a particle and also that of being a wave motion.

Now, for many years a corresponding duality had been observed with waves of light and X-rays. Diffraction effects had proved that both light and X-ray possessed the properties of a wave motion: on the other hand, the photo-electric effect (the emission of electrons from a metal on which radiation falls) and the Compton effect (behaviour of radiation as a stream of particles when in collision with electrons) just as definitely indicated that radiation possesses the characteristic of consisting of discrete packets of energy

(photons). It was the great triumph of the new wave mechanics (developed by Schrödinger, Heisenberg, and Dirac) that these two contrary properties possessed both by matter and radiation were to some extent reconciled. This reconciliation, however, was made at a cost, the full significance of which has not until recently been appreciated.

At the time of the introduction of wave mechanics the Bohr planetary atom model of particle electrons circulating in orbits around a nucleus had not only become extremely complicated, but it had also become inconsistent, and it finally failed even to describe the facts. However, it should be noted that the fundamental concept of the Bohr model was based on stationary electron orbits, which from their very nature could never be observed. The final result was that the whole concept was replaced by the wave idea, in which characteristic stationary electron orbits became characteristic oscillations of a function ψ.

The mathematical advantages of this change were enormous, but it was obtained at the cost of losing any clear idea of the fundamental nature of the entities involved (electrons, protons, etc.), whose activity the wave mechanics was so successful in describing. Again, the so-called particle electron could not be tied down so as to make its motion precise. If its speed was known fairly accurately, then its position was vague; if its position was known, then its speed was vague: if the energy of the particle electron was known accurately, then the instant when it possessed that energy could not be known. What was significant about this, however, was that the indefiniteness did not appear to be due to any imperfections in definition, in the theory or in experimental technique; it was apparently implicit in nature itself. If an electron

collided with an atom, it was not possible on the new mechanics to predict accurately its subsequent path, as can be done for large-scale projectiles very simply on Newton's mechanics. In this sense, therefore, the new mechanics is non-deterministic, while Newton's mechanics is strictly deterministic. This result has been expressed by Heisenberg in his *Principle of Indeterminacy*, in which he maintains that the exact speed and position of a particle of matter cannot be measured simultaneously. It will be noted that if the particle idea is rejected, and matter regarded as a packet of waves, then no surprise need be evoked by this principle, as it is quite clear that one cannot localize a wave of definite wavelength, as it is extended throughout space.

Although the precise motion of a minute particle, say an electron, after an atomic collision cannot be specified on the wave mechanics, yet the wave mechanics does give some indication of the probable subsequent motion. This follows by interpreting the electronic ψ function in any small region of space as being related to the chance of finding the particle electron within that region—*i.e.*, if the particle idea is retained, then the laws governing its position and speed are the laws of probability and not Newton's laws. It follows at once from this that if one dealt with a very large number of such particles the probable motion of the assembly can, however, be predicted practically with increasing certainty, in accordance with the established methods of statistics. Since even the smallest particle of matter in everyday life (a speck of dust) is composed of millions of electrons and atomic nuclei, the most probable motion of such an assembly can be deduced on the wave mechanics with, for all practical purposes, complete certainty. On this view, therefore, the strict

determinism which apparently holds in everyday life is, in fact, not a demonstration of the validity of the strictly deterministic laws of Newton : on the contrary, it is a consequence of the fact that one is really dealing with an assembly of very large numbers of fundamental particles. The basic laws which control the motion of such individual particles are the laws of probability, and the laws of motion of the fundamental particle of the universe are not deterministic in the Newtonian sense.

The principle of indeterminacy gives a clue to the reason why quantum effects are not discernible with the gross matter of our everyday life. Such matter is considerably extended in space over distances millions of times greater than the diameter of, say, an individual atom : further, the speeds with which the atoms composing matter at ordinary temperatures are moving are very high. It is only when one deals with very low speeds (nearly zero) on the one hand, or with small distances (atomic diameter) on the other, that the quantum restrictions imposed by the principle of indeterminacy are discernible. If the speeds of the particles examined are high, then quantum phenomena are discernible only when dealing with the minute fundamental particles of nature (electrons, protons, etc.). On the other hand, if one deals with ordinary macroscopic particles of matter (*e.g.*, a drop of liquid) then quantum effects become obvious only if the speeds of the atoms concerned are practically zero, and this is possible only near the lowest possible temperature (absolute zero). This fact is one of the main reasons for the prosecution of low-temperature research in physics today.

At temperatures approaching absolute zero, strange non-classical effects can be produced even with matter

in bulk, and one of the most striking of these is that known as the super-fluidity of helium. This element is the only one which remains liquid at sufficiently low temperatures. If a few drops of this liquid are placed in a small test-tube and the whole cooled nearly to absolute zero, the liquid rises up the sides of the tube and flows out; the liquid refuses to be confined in that small space! This effect is in accordance with Heisenberg's principle. When cooled the speeds of the helium atoms become nearly zero, and therefore their magnitudes become increasingly sharply defined; it follows therefore from Heisenberg's principle that their location must become increasingly more vague; and since the atoms are already confined in a sharply defined small space, they must get out in order to increase the indefiniteness of their location.

Spin and New Particles. A simple application of the same kind of argument as that above shows that, if an electron is regarded as being confined within the minute volume of a nucleus, the Principle of Indeterminacy requires that the electron should possess enough energy to be driven out. Thus, there cannot be any electrons within the nucleus, as envisaged on the Rutherford model. Again, when it is noted that estimates of the size of the nucleus make it about as small as an electron, it is difficult to regard a nucleus as being composed of a number of electrons and protons.

Let us now consider the new discoveries that led to the final rejection of the Rutherford–Bohr atom model —discoveries of new properties of the electron and of entirely new fundamental particles of matter. About the year 1925, physicists had concluded that the intricacies of the fine structure of atomic spectra, and also certain magnetic phenomena, could not be ex-

plained on the basis of the known quantum integers and the selection rules. Some other number was required, but the only one which fitted a reasonable scheme was a half-integer, and this was directly opposed to all the tenets of the original quantum theory. The physical significance of this innovation was settled by Uhlenbeck and Goudschmidt, who showed that the new half-quantum number was associated with the *spin* of the electron about its axis. This new property is a fundamental property of the electron, and is just as fundamental as its charge or mass.

Now, this conception of a spin is not at all an easy one, and it raised fundamental theoretical difficulties of interpretation, especially when the conception of a simple particle electron was giving way to the wave conception in the new mechanics of de Broglie and Schrödinger. Yet there was no question of the importance of this fundamental spin idea, since, by taking this motion into account, the difficult phenomena of the fine structure of spectra fell into place like a jig-saw puzzle. However, it was Dirac who tackled this problem with success by making precise the idea of spin in the new waves mechanics. This immediately had startling results, leading to his forecast of the existence of a *positive* electron. Dirac's equations indicated for an electron the existence of states of negative energy, but these were physically incomprehensible, and this was considered to be a great difficulty of the new theory when applied to ordinary electrons. Dirac consequently suggested the existence of a new particle, the exact opposite of a negative electron—*i.e.*, a positive electron. In order to conserve charge, a positive electron could appear only at the same instant as a negative electron. Dirac envisaged a pair of electrons—one positive and the other negative—

being created at the same time by the materialization of a quantum of radiation (photon) according to Einstein's law of equivalence of mass and energy. Further, the positive electrons possessed a great probability of associating with a negative electron, thus annihilating themselves with the production of high-energy (1 million electron volts) photons. Since in nature there is an excess of negative electrons, it follows that positive electrons have very little chance of independent existence for any appreciable time; this is the reason why they had not been detected previously. This theory was rather sceptically received by the scientific world until experimental verification was obtained many years later, 1931–32, by Anderson in the U.S.A. and by Blackett and Occhialini in Cambridge. About the same time, the existence of still another fundamental particle was established: this was the neutron. Strangely enough, Rutherford himself had suggested as early as 1920 the possible existence of a particle of the same mass as a proton but possessing no charge. The absence of any charge would make it extremely difficult to detect the presence of such particles, but proof of their existence was found in 1932 by the experiments of Chadwick and of the Curie–Joliots. Experiments showed that neutrons were produced when certain atomic nuclei were disintegrated; in other words, the neutron appears to be a constituent of the nucleus. This view is supported by the vast number of experiments carried out during recent years on the production of artificial radioactive atoms. When atoms of an element like phosphorus, for instance, which is usually not radioactive, are bombarded with neutrons, some enter the nucleus and are captured. The new nucleus is now unstable, but later attains stability by ejecting one of the fundamental particles.

The new theory of the constitution of the nucleus based on these facts appeared at first to be just as simple as the older view. The nucleus was to contain only protons and neutrons, the number of protons being sufficient to provide the requisite nuclear charge (Ne) while neutrons were added to make up the atomic weight—*e.g.*, oxygen of atomic number (charge) 8 and mass 16 would have 8 protons and 8 neutrons. This view, however, at once gave rise to two questions: (i) How did naturally radioactive atoms emit negative electrons (β-rays), and some artificial radioactive atoms emit positive electrons ($\beta+$-rays)? (ii) How could these positively charged protons all keep together in such a minute space when their ordinary electro-static repulsion should be enough to drive them asunder?

The first question was answered in a delightfully simple way. It was suggested that in the nucleus there is one kind of heavy particle (a nucleon) which can exist in two different states. When positively charged it was a proton, and when not charged it was a neutron. The neutron can change into a proton by capturing a positive electron or by emitting a negative electron, and a proton can change into a neutron by emitting a positive electron or by capturing a negative electron. This idea helped to restore some of the symmetry lost when the light negative electron and the heavy positive proton were the only known fundamental electrical particles. Continuing with these ideas it was even suggested that there might exist a particle which is a nucleon with a captured negative electron—*i.e.*, a sort of negative proton. If it exists, this has not yet been discovered; these ideas, in a general way, do account for the emission of β- and $\beta+$-rays by nuclei.

The other question, that of the stability of the nucleus, is much more difficult, and could not be answered

without the introduction of some entirely new conception of radiation and the interaction of matter and charges with the electromagnetic field. A kind of analogy was drawn between the change of state of a nucleon producing an emission of electrons (positive or negative) and the change of state of an orbital electron in an atom causing the emission of a photon. On the new quantum mechanics, it was deduced theoretically that two identical electrons near together would be acted on by two kinds of force: one was the classical (Coulombian) force of repulsion, and the other a non-classical (quantum) force of attraction for which no mechanical analogy is possible. This latter force is a consequence of the identity of any two electrons, and appears to arise because the two electrons are exchanging some influence through the medium of their mutual electromagnetic field; it is thus known as an *exchange force*. This force at short distances can overcome the Coulombian force of repulsion, and was seen at once to account for chemical valency and the stability of the structure of molecules; it also explained how the atoms of metals like iron were so strongly held together.

It was by introducing still another new force of this exchange type, on an analogy with electron exchange binding, that the modern theory of the stability of the nucleus was laid. A Japanese physicist, Yukawa, in 1935 proposed the existence of a new type of particle (something similar to an electron but rather heavier) which can be exchanged by a proton–neutron pair, and in that way set up powerful exchange binding forces. The new particles were called mesons, and their weight was calculated to be about 240 electron weights.

Two years later mesons were observed in cosmic

rays, and their estimated weights confirmed Yukawa's calculations. However, more recent work carried out during the last ten years or so, and using the photographic-plate technique developed by C. F. Powell and others at Bristol, has disclosed the existence of many other mesons having a whole range of weights. In fact, it looks as if mesons can lose weight (*i.e.*, decay), as they undergo collisions in passing through matter, and such decay with the emission of fast electrons was first demonstrated by E. J. Williams at Aberystwyth. It is not improbable that the masses of the various mesons may compose a set of *quantized masses* as suggested very recently by Born. Mesons may be positive, negative, or neutral.

Thus it can be seen that the discoveries in physics of the last decade of the first half of the twentieth century tend to be building a much more complex picture of the micro-structure of matter than had emerged at the beginning of the century. The mechanics of the quantum theory, which is quite contrary to the classical mechanics of Newton, is now seen to be applicable to all the fundamental constituents of matter, either in the free state or bound together tightly in the atomic nucleus. All these basic particles thus obey, not strict deterministic laws, but the laws of probability. Since basic mechanical laws represent the relation between our conception of Space and Time, it follows that this conception, too, must undergo revision in accordance with the new quantum ideas.

The Significance of the New Physics. In attempting to assess the significance of modern ideas in physics, it is necessary to trace the development of scientific thought from the time of classical antiquity to the present day. The Greeks tried to form a complete philosophy

embracing all natural phenomena in a mighty geometrical scheme. They based their views of nature on certain preconceived ideas. However, Aristotelian natural philosophy was sterile because nothing could be derived from it to predict quantitatively happenings in the natural world from basic assumptions; yet this philosophy was held more or less throughout the Middle Ages. It was the work of Galileo in Padua University which laid the basis for the rejection of the scholastic view, and in this he was helped by William of Occam's ideas, and by a re-appreciation of the fertile ideas of earlier Greeks (Aristarchus, Archimedes, and Pythagoras). The final wearing away of the Aristotelian view came with Descartes and his followers. Descartes and Fermat found a way of combining algebra and geometry, thus forging a powerful tool for handling the new ideas of modern science which were then being born—the mathematical continuous function, statics, magnetism, electricity, new knowledge of the motion of the planets following the invention of the telescope, the microscope, medicine (circulation of the blood).

Descartes contrived to show that the universe as he knew it would operate under natural laws of motion in the way it actually does, with God acting only as the Prime Mover to set it going. He therefore tried to eliminate spirit from natural phenomena, which occurred, he thought, in accordance with mathematical–physical laws, and in this Descartes succeeded. However, Descartes really wanted to find a universal science which would take in all natural phenomena together with theology and the world of the mind, and yet be acceptable to all, as mathematics was. In this he failed, because the science which developed cut the world, as it were, into two sections: the world of matter and

mechanical laws on the one hand, and the world of the spirit on the other. The great achievement of the Cartesian–Galilean–Newtonian system was its *universality* in its application to the world of inanimate matter. The force of gravity which on the earth pulled an apple to the ground was the same as that which made the moon rotate round the earth, and the same again which held the earth to the sun; and the motion of all matter was found to be in accordance with Newton's laws.

Using these laws, it is possible to calculate the subsequent history of a particle of matter from its properties at any given moment. For example, the path of a shell appears to be precisely determined when once its muzzle velocity is known: the whole motion is the *effect*; and the explosion in the breach is the *cause*. In this sense, therefore, the motion of all matter is apparently strictly deterministic in our everyday world. The shell follows a strictly predetermined path, and what is true for the shell, is true for the earth, the sun, and stars, and, in fact, for the whole material universe, which then, once started, runs on its own like a gigantic clockwork machine, running in space and time according to fixed laws. This achievement of the seventeenth century, resulting in the new outlook which finally freed man from the intellectual chains of the philosophy of the Middle Ages, was probably the greatest in the whole history of human endeavour since the discovery of number. Scholastics of the Middle Ages believed that some human minds could attain incontrovertible knowledge about the real world by deduction from first principles—evolving them out of their own inner consciousness, but this method had been tried and failed. To get knowledge from nature one has to ask nature: that is why science is built upon experiment, from which

most of the intellectual ideas, which proved so fertile both in mathematics and philosophy, have emerged. Controlled quantitative experiments must be carried out, which give results in quantitative form to be handled by mathematical methods. That is the technique which, in three centuries, has proved so powerful and has completely revolutionized the life of man.

The power and success of these new ideas has spread to the whole of science. These basic laws of motion set out by Newton became known as fundamental laws of nature—laws which exemplify the nature of reality: the ideas of *determinism* became firmly established. Whether right or wrong, it should be appreciated that this idea was an extremely fertile one, as it led to new discoveries. Thus, we consider inanimate matter as obeying blind forces in a regular predictable way, giving cause and effect as perfect determinism. In the excitement of these tremendous developments one point was rather ignored. It was this: given the position and speed of a particle on Newtonian mechanics, it can be predicted exactly what that particle will be doing at a later instant. In the same way, however, given the speed and position at the later instant one can calculate just as accurately what the particle was doing at the earlier instant. In other words, on this view, time has no arrow. Everything appeared to apply equally for a positive as for a negative time. Now this was unsatisfactory, as the very simplest observation in everyday life shows. The usual conception of time is that it goes on and on; certain events are irreversible; for instance, if a drop of ink falls into a tumbler of clean water, the ink spreads eventually to colour the whole of the water. No one has ever seen the converse of coloured water slowly clearing to end with clean water surrounding one isolated drop of coloured liquid

(liquids which mix). But there is nothing in Newton's deterministic scheme to say that this is impossible. The ideas of probability and statistical mechanics developed by Maxwell, Boltzmann, and Gibbs solved this dilemma.

This new technique (which also had far-reaching applications in social spheres) had immediate application in physics for the following reason. It was believed in the last century that Newtonian mechanics held right down to all the ultimate particles of reality. To solve the mathematical problem of three bodies was extremely difficult, and for a larger number of bodies it is well-nigh impossible. Now, Newton's success depended upon his treatment of really simple systems of two bodies—*e.g.*, an apple falling to the earth; the moon going round the earth; the earth going round the sun: three bodies could be dealt with only if the effects of the third were very small, and an approximate method was used (Celestial mechanics of Poincaré): it was a very useful method, and it led to the discovery of new planets. However, if one is going to apply Newton's laws to the particles of matter, a very different problem is met. Here one is concerned not with one or two particles, but with millions. A cubic inch of metal or of gas contains about 10^{23} atoms, and to solve this incredible number of equations is impossible. It was in order to deal with this problem that Maxwell and Boltzmann introduced a statistical technique.

The idea is that a collection of, say, millions of atoms can take up an infinite number of different states or complexions. If each particle really obeys Newton's laws, analysis shows that there are millions of complexions which are practically identical; and, further, that the occurrence of these similar complexions is

so frequent, that to our macroscopic everyday eyes the collection of atoms (matter) appears to be in a permanently steady state. Other states are also possible, but as they are so few compared with the stable states we shall be very unlikely actually to see them in a lifetime. Thus, because the probability of a state is very high, it appears to us to be almost a certainty, so that, as far as experiment and observation of large bodies go, determinism still holds. In the case of the drop of ink in a glass of water, the mixed state is far more probable than the unmixed state, and, as time goes on, the mixing is most likely to increase, just as we observe in practice. Thus time is given an arrow.

Now, the consequences of this view were profound, and at the same time disquieting. *Strict determinism* was removed and *very high probability*, or a qualified determinism, substituted. But the keener minds in the last century sensed this, and as they did not want determinism set aside, they thought that if only the full details of the motion of atoms were known, and if all Newton's equations to calculate the subsequent behaviour could be solved, then it would be found that strict determinism held. They believed that probability methods, which were so successful for describing matter in bulk, were in fact only an expediency until the exact laws of the ultimate particles of nature—the basic laws of nature—were found. Thus, if it could be shown that the ultimate particles of matter obeyed Newton's laws of strict causality, the statistical concepts, it was thought at the turn of the century, would then be set aside, except as an expediency for dealing with large numbers.

However, as we have seen, when the behaviour of the fundamental particles of nature is examined, it is found that the basic laws of the natural world are not

the strictly causal laws of Newton, but are the laws of probability—the laws of chance. Instead, therefore, of the laws of statistical mechanics being (as was thought in the nineteenth century) an expedient on account of our ignorance of the ultimate laws—they themselves now turn out to be the ultimate laws. This signifies a change in the interpretation of reality comparable in importance to the Renaissance.

These new ideas are not in accord with our *general conceptions* of what is *common sense*, and if we persist in trying to express them in terms of notions which we have learned to describe as common sense, the new concepts appear meaningless, and it has sometimes been said that modern physics has dissolved into meaningless symbols. Such is not the case. However, interpretation of these new ideas may involve new ideas of what constitutes logic. In fact, during the last fifty years a new idea is being developed, that of a three-valued logic. This owes a great deal to the work of the Dutch mathematician Brouwer, and may be able to deal with the kind of problem in which, for instance, an electron appears to possess contrary properties, being sometimes like a particle, and sometimes like a wave. It is possible, also on this view, that a solution will be found for the problem which Descartes failed to solve: that of *free will* and *determinism*.

There is no doubt that both free will and determinism appear to exist in the macroscopic world: these concepts are quite opposed to each other, and from their natures are mutually exclusive. Bohr has introduced a new *Principle of Complementariness* based on an analogy with the *Principle of Indeterminacy*. While there is no doubt that a particle can have both velocity and position, yet they cannot both be known together. They form complementary functions: both exist but

not together. This is very similar to what is found with the familiar concepts of free will and determinism, as they are mutually exclusive. Bohr extends the idea of complementariness to these concepts of free will and determinism, just as Whitehead and Russell and also Peano extended algebra from symbols for numbers to symbols for concepts, and in this way formed a symbolic logic. It is possible, therefore, that we are wasting our time in trying to solve the problem of the reconciliation of free will and determinism, just as Einstein pointed out forty-five years ago that we were wasting our time trying to find the velocity of the Ether. The problem of ether drift had been attacked by the physicists for nearly a century without success; Einstein solved it by pointing out that we were asking the wrong question of nature, and were therefore trying to find something which was not there. When this point is realized the conclusion is obvious, although at that time it seemed an incredibly difficult step to take. In the same way, it is possibly a waste of time to discuss notions like the beginning and end of the cosmos: applied to the cosmos these ideas may have no meaning.

Thus the new thought greatly extends our intellectual capacity, not only by adding new concepts, but also by excluding all that is sterile and futile. This indicates a new concept of what *understanding* really is, and may free man from the effects of trying to do with his mind that which cannot be done, owing to the nature of thought itself. Attempts have been made, notably by Eddington and his school, and later by Milne and by Bondi and Gold, to reduce the whole scheme of physics to basic *principles of impotence*, from which the whole of science can be deduced. It is then considered by some philosophers that the human mind,

on the basis of such principles instinctively known, can deduce the whole range of scientific facts, without, in principle, carrying out any experiments on the phenomena of the external world. Support for such a view might appear, at first sight, to be provided by the success of recent effort made to co-ordinate physics. For example, the magnificent intellectual edifice known as the theory of the electromagnetic field, which dominated physics for about a century, can be derived, it is considered, from the simple principle of impotence which states that it is impossible to establish an electric force inside a hollow closed conductor by means of a charge outside.

The inter-relatedness of physics is also exemplified by the fact that such an apparently simple basic idea as *spin* (of the electron, earth, a top, the galaxies, etc.) cannot be fully stated without a full discussion of the metric of the cosmic space—*i.e.*, the nature of the cosmos. On this view, electron spin is a direct logical consequence of the symmetry of the cosmos in space and time. If this is correct, then here is a complete unification of experience from the infinitely small to the infinitely large. Again, other basic principles which have been suggested are Milne's *Principle of Impotence*—that one cannot tell where one is in the Universe, and Bondi and Gold's *Perfect Principle*—that one cannot tell the cosmic time. Older principles, such as that of the conservation of energy, turn out to be trivial consequences of these principles. These new principles are definite rules of procedure which enable all that is known to be integrated into one rational scheme of things in a way many times attempted since man began to think, but never before achieved.

On the other hand, it may not, perhaps, be generally realized that the particular way in which we now think

and the particular kind of argument which we regard as satisfactory are themselves the result of experimental experience over the last few hundred years. Without that experience of the external world, there is no evidence to indicate that, even given these new basic principles of impotence, man would in fact reason forward to deduce present observed phenomena in the same way as he now reasons back to these principles inductively from experience. Further, mental processes themselves are now being examined by scientific methods in just the same way as are so-called material phenomena. As Dingle has pointed out, the duality of the Cartesian world is now being simply ignored; it may well be that the processes of the mind will yield to the same techniques of scientific analysis which have made such great advances in co-ordinating the natural phenomena of the physical world.

CHEMISTRY

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WHATEVER their chief interest, there must be very few who have not directly experienced the growing powers of applied chemistry. New drugs and stronger explosives; better dyes and more powerful insecticides; substitutes for rubber, soap, and silk; entirely new industries devoted to the production of plastics and light alloys; improved fertilizers and safer antiseptics; super-fuels and selective weed-killers. There is an endless list of new materials, affecting all who share our technical civilization. These practical advances owe something to the sad stimulus of two terrible wars, to that reckless concentration of effort and finance which only danger can induce; yet advances they remain, of great potential value to a sane society.

In modern times, these material devices mostly result, at short range or long, from the obstinate, unhurried researches of the scientist, from his slow, painstaking improvement of theories and apparatus, of his mental and instrumental equipment. Chemistry is that branch of science which interprets the variety and the variability of material types; and the last fifty years have witnessed notable advances. For their due appreciation we must glance briefly at the chemistry and the chemical problems with which this century began.

Atom and Element before 1900

Chemistry in the nineteenth century was based on two major theories: the definition of an "element" as established by Lavoisier (1789), and the new concept of an "atom" originating with John Dalton (1808).

According to Lavoisier, the elements which compose the multitudinous forms of matter—as a few letters, in varying arrangements, originate lists of a dictionary—were themselves indestructible. They were to be recognized by the practical impossibility of breaking them down to simpler forms. According to Dalton, all matter was composed of atoms, of fundamental particles of varying *weights*. Atoms of any one element were of equal and characteristic weight.

These elemental atoms, structureless, impenetrable, and eternal, rank with the most profitable ideas of scientific history. The search for the elements, the assignment of their atomic weights, inspired a century of devoted research, which extended the systematic knowledge of matter at an unprecedented rate.

By 1870 the labours of such workers as Berzelius and Stas had established the atomic weights of the known elements with great precision. Incidentally, these weights were seldom exactly whole numbers, and an earlier hope (Prout, 1815) that the elemental atoms might all be condensates of hydrogen (at. wt. = 1) was reluctantly abandoned.

It was early observed that though each element had its own characteristics, some still exhibited more resemblances than differences. Chlorine, bromine, and iodine, for example, or calcium, strontium, and barium, behaved as though each triad were variations on a single theme. Each group, for instance, was characterized by a maximum combining power, or valency, towards other atoms. (Chemists visualized atoms of the chlorine group as equipped with seven hooks or couplers, the calcium group with two, and so on.)

Mendeléeff made the most successful effort at classification. Arranging the known elements in the order

of atomic weights, and allowing for some undiscovered members, he exhibited a Periodic Table of chemical character (1869). In its final form, the tabulated elements showed a recurrence of type at every eighth or, alternatively, tenth element: there were nine major types with a valency maximum of zero for the "inert gases," one for the adjacent column, two for the next, and so on. This brought order out of chaos to a remarkable extent. The Periodic Law exhibits notably that "economy in description" which Ernst Mach set as the goal of scientific method.

Some difficulties remained. Among the metallic elements there was a group, the so-called "rare-earth metals," whose chemical character did not vary with atomic weight—these did not fit into the system at all. In several other cases a fit was obtained only by exchanging adjacent elements, so that, for example, tellurium (at. wt. = 127·6) preceded iodine (at. wt. = 126·9). Fifty years of further inquiry failed to reconcile such anomalies with the Daltonian atom.

Indeed, like all good theories, the Periodic Law raised almost as many questions as it answered. In this century most of these problems have been solved; but before solutions could be offered Lavoisier's definition of an element had to be abandoned, and Dalton's atom added to the other venerable relics in the museum of scientific history.

Elements in Decay

Readers of this volume will be aware that the period around 1900 witnessed a sudden forward surge in scientific progress. New experiences crowded in, new instruments were developed, new ideas appeared on the nature and the relationships of matter, energy, space, and time.

In 1900, the Curies isolated and examined a new, heavy element of the calcium type. They called it " radium," and radium was plainly breaking down. It was soon realized that no element heavier than bismuth was stable. Here was Time's scythe at work among the " indestructible " elements.

The radioactive elements did not, in fact, decay at any standard rate. Emitting particles of matter or tiny parcels of energy, they lost weight and changed their chemical characters through a series of transient elements in a process terminating with lead. These intermediate elements could result from different decay processes, and in some cases it was clear that two forms of the same element must have *different* atomic weights.

Soddy coined the name " isotopes " for these oddities. In 1914, it was established that lead from suitable radioactive ores varied in atomic weight from 206 to 208, although the atomic weight of normal lead is 207·2. Thus within the first few years of this century it became evident that *new* definitions of the terms " element " and " atom " must be found. The attainment of these new definitions has been a chief achievement of chemistry and physics in the last fifty years. Much is due to the study of atomic physics, which lies on the borderland of chemistry and physics, and cannot here be treated in detail. It is still possible to visualize fairly simply the changes of chemical significance.

The Modern Atom

Our atom is not impenetrable—indeed it is largely empty, with a tiny, heavy nucleus occupying perhaps one part in ten thousand of its radius. The nucleus of ordinary hydrogen contains one proton, which has

one unit of weight and carries one positive charge. Every other type of nucleus contains protons, and also neutrons which have one unit of weight and are electrically neutral. As the number of protons increases, so does, in general, the proportion of neutrons; but the number of protons defines the nuclear charge—the "atomic number."

The neutrons appear to confer stability on the nucleus. Their proportion can, however, vary within limits, so producing isotopes. Some elements, such as beryllium ($^{9}_{4}$Be),[1] consist of only one isotope; more generally the elements are mixtures of isotopes. In tin ($_{50}$Sn), the number of isotopes is ten, and the number of neutrons packed with the fifty protons varies from 62 to 74; these isotopes vary in abundance, providing as a statistical average the atomic weight, 118·7, of the element. The constant factor is the number of nuclear protons, the atomic number, which most adequately characterizes the chemical element, and equals the number of planetary electrons which complete the neutral atom without increasing its weight.

Increase of atomic number is paralleled fairly closely by an increase in atomic weight, as in the successive elements vanadium ($_{23}$V, at. wt. = 50·95), chromium ($_{24}$Cr, at. wt. = 52·01), and manganese ($_{25}$Mn, at. wt. = 54·93). This explains the success of Mendeléeff's brilliant improvisation; but this degree of agreement does not always extend to *adjacent* elements. It is in order of *atomic number* that tellurium ($_{52}$Te, at. wt. = 127·6) logically precedes iodine ($_{53}$I, at. wt. = 126·9);

[1] Throughout this article, if E is the symbol of the element under consideration, in $^{b}_{a}$E, *b* is the "weight" (no. of protons + no. of neutrons in the nucleus), and *a* is the atomic number (no. of nuclear protons = no. of planetary electrons).

here, as in other anomalous cases, characterization by atomic number has shown its deeper significance and resolved long-standing problems.

Research directed by this new concept of a nucleated atom has had some astonishing successes. In 1919, Rutherford converted nitrogen ($^{14}_{7}N$) into oxygen ($^{17}_{6}O$) by suitable bombardment of the nucleus. This was a feat of major historical importance, realizing the dreams of alchemy, initiating a new kind of chemistry, and firmly establishing the prestige of the new atomic model.

Nowadays the equipment of the chemist includes " pure " isotopes shaken out from the others of their type by ingenious devices, like Aston's mass spectrograph; radioactive forms of common elements like sodium or phosphorus; and transuranic elements, like plutonium ($_{94}Pu$) and curium ($_{96}Cm$), which inaugurate a new field of chemical inquiry on man-made elements of the radioactive series.

The Planetary Electrons

While the composition of the nucleus has been so dramatically and effectively established, the chemist finds another set of interests in the electron screen which surrounds it and delimits its effective volume. These electrons are held with a tenacity which, for the outermost electrons, depends upon the nuclear charge and the distance of the electron from the nucleus. Thus electrons are more easily dislodged from large atoms, and most easily added to the relatively small.

This loss or gain of electrons by a neutral atom or molecule explains the phenomenon of " ionization," and relates electricity in a simple manner to the structure of the atom, since " ions " are conductors.

Atoms are so small that a Lilliputian unit is often

employed for their description—the so-called Ångstrom unit (Å), of which there are one hundred million in a centimetre. The radius of atoms varies from about 0·5 to 3Å. units, and X-ray investigations enable us to determine the distance between the atoms of a molecule to the second decimal place on this standard!

Within such minute volumes, one to a hundred electrons may circulate around the nucleus. On spectroscopic evidence, by application of Planck's quantum theory, by electrical and magnetic measurements, Bohr and others have worked out the details of electronic arrangement.

The electrons are arranged in quantum groups, or " shells," according to the average distance from the nucleus which is maintained by those of each group. The number of electrons which each group may contain is a function of this distance. The group, or shell, nearest to the nucleus has a maximum of two, the next of eight, the third of eighteen, the nth of $2n^2$. These electrons are normally paired in a manner which preserves magnetic neutrality.

A full description of the Rutherford–Bohr atomic model requires positrons, mesons, and other physical entities; but, for chemical purposes, it is envisaged as a complex of nuclear protons and neutrons, with neutralizing electrons. The electron number in the neutral atom is equal to the charge on the nucleus, to its number of protons, to the " atomic number " of the element so brilliantly established by Moseley at Manchester in 1913–14.

Electrons of Inert Gases

The electron configuration of the inert-gas elements, which are incapable of any chemical reaction, is of pivotal importance in chemical theory today. Their

electrons, if we neglect some secondary quantum factors, can be distributed in shells as follows :—

Helium ($_2$He)	2 :
Neon ($_{10}$Ne)	2 : 8
Argon ($_{18}$A)	2 : 8 : 8
Krypton ($_{36}$Kr)	2 : 8 : 18 : 8
Xenon ($_{54}$Xe)	2 : 8 : 18 : 18 : 8
Radon ($_{88}$Rn)	2 : 8 : 18 : 32 : 18 : 8
$2n^2$	*2 : 8 : 18 : 32 : 50 : 72*

Here the larger groups are not immediately completed; they are filled, retrospectively, only after the outermost shell has been begun. It is specially notable that, other than helium, each maintains an outer " octet " of electrons.

The frequent recurrence of eight and eighteen (8 + 10) is immediately suggestive of the octets and occasional decets in the Periodic Table; it is, in fact, their electronic interpretation.

It is possible, by physical means, to dislodge the electrons of an inert gas, but in chemical reaction inert gases remain unchanged. The other elements can lose or gain electrons with much greater ease, and it is reasonable to assume that they derive their chemical reactivity from electrons which are not, as it were, locked up in the inert-gas pattern, and are sufficiently distant from the nucleus to be relatively loose.

This latter qualification, and our growing precision in placing the planetary electrons, afford explanation of another long-standing puzzle—those " rare-earth " metals, which, despite increasing atomic weight from cerium ($_{58}$Ce) to lutecium ($_{71}$Lu), maintain their strong chemical resemblance to each other and to lanthanum ($_{57}$La) in the family of group III. Now, lan-

thanum has three (57–54) loose electrons; beyond lanthanum the fourth quantum-group or shell of xenon with 18 electrons attains by steps its full quota of 32 electrons. This group is so deep in the atomic structure that these electrons are tightly held, and so, until lutecium, there are fourteen successive elements with, in effect, only three loose electrons each. These " lanthanide " elements, which so sorely perplexed the nineteenth century, are thus systematically described in terms of the Rutherford–Bohr atom, and another gulf in our understanding of matter has been bridged.

Apart from this abnormal sequence, it is easy to calculate the available electrons if it be remembered that an inner electron group, in growing from 8 to 18, locks ten electrons of any surplus number over ten. Thus calcium ($_{20}$Ca) has two loose electrons (20–18); so has zinc ($_{30}$Zn) (30–18 = 12 : 12–10 = 2). By this juvenile arithmetic the elements arrange themselves, as in the earlier table, in family groups. The group O is the inert gases; groups I to VII have the group-number of valency electrons; group VIII elements have 8, 9, or 10.

Electrons and Chemical Structure

The re-interpretation of chemical combination and formulæ in terms of these electron structures has been a major achievement of this century. Some indication must be given of this new technique to illustrate the power in description of the atom as defined today.

Of the known elements one in twelve is an inert gas, nine are metals—whose atoms have from one to ten available electrons—and only two are non-metals.

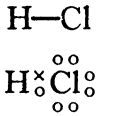
The atoms of non-metals are so small in relation to their nuclear charge that they tend rather to gain than to lose electrons; and they are found to have atomic

numbers just below the inert-gas numbers, with the result that only their outermost shell is incomplete.

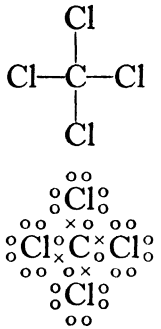
Apart from some loose inter-metallic complexes, metals do not combine among themselves; but non-metallic atoms are so reactive that they may combine with atoms of the same element, as in nitrogen gas (N_2), ozone (O_3), and sulphur (S_8), and they combine with other non-metallic elements to provide a host of compounds. The compounds of carbon with these electro-negative elements are so numerous as to require a special scientific section (organic chemistry) for their investigation.

Further understanding of the nature and composition of such chemicals was greatly assisted by the suggestion made by J. N. Lewis (1915) that a bond between two non-metallic atoms was composed, not of imaginary hooks or couplers, but of two electrons which they *shared* in common. When each atom provided one valency electron to the pool each raised its total number of electrons by one, and this process was continued, by forming other bonds with the same or with other atoms, until the incomplete outermost octet was increased to eight electrons, and the non-metallic atom had achieved the electron-number of an inert gas. Thus, to take a very simple case, in nitrogen gas (N_2) each atom beginning with five valency electrons has contributed to a shared group of six ($5 + 5 = 2 + 6 + 2$); there is a triple bond between the atoms, and, including the shared electrons, each has completed an outer octet and attained a neon structure. In this case, one pair of unused valency electrons remains with each atom. Another type of covalency bond may be formed in which one of the combining atoms provides both electrons, from such a " lone-pair " in its octet. A non-metal having four or more available electrons can

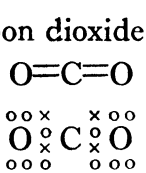
Hydrochloric acid, HCl



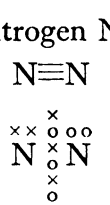
Carbon tetrachloride, CCl_4



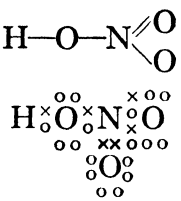
Carbon dioxide, CO_2



Nitrogen N_2



Nitric acid, HNO_3



Calcium chloride, $CaCl_2$

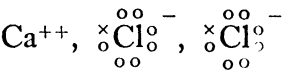


FIG. 1.—ELECTRONIC INTERPRETATION OF SOME SIMPLE FORMULÆ.

Hydrogen completes a "helium" doublet: carbon with four valency electrons, nitrogen with five, oxygen with six, chlorine with seven, achieve an outer octet and complete an inert-gas electron-number by sharing electrons, two per bond, with other non-metals. In nitric acid the nitrogen provides *both* electrons binding one of the oxygen atoms: this structure and that of hydrochloric acid are ionized by water, which separates the proton and produces an "acid" solution.

In calcium chloride, a typical salt, two chlorine ions are formed by each obtaining one electron from the metallic atom: the chlorine octets so formed complete an "argon" electron-number. The calcium ion carries a double charge of positive electricity. The electrostatic forces hold the molecule together in this case.

combine by covalencies in various ways ; but in the vast majority of cases it succeeds, as it were, in constructing an inert-gas electron-number for itself using some, or all, of its available electrons in bond formations which complete the deficient octet (see Fig. 1).

Interesting exceptions occur but are of relatively great rarity. Hundreds of inorganic compounds, scores of thousands of organic chemicals, have their composition readily rationalized by the simple numerical rule that, by pooling pairs of electrons, the non-metals in combination achieve the electron-number of an inert gas.

Unless they contain hydrogen in certain relationships, compounds formed entirely of non-metals do not readily provide ions, and as chemicals are generally characterized by volatility. They include most gases, liquids, and low-melting solids.

In many other types of compounds, metals and non-metals are found in combination. Many acids, most alkalies, and nearly all salts contain metals. Such substances are characterized by the ease with which they provide " ions," charged particles which explain the capacity of their solutions in water to conduct an electric current. Ions result when metals transfer one or more electrons to complete the octet of a non-metal and assure its inert-gas configuration. Metals, as has been said, may have from one to ten electrons ; but as ions with more than three positive charges—*i.e.*, having lost more than three electrons—are usually unstable it is evident that metals containing more than three valency electrons will seldom achieve an inert-gas number in this way. In a simple case like common salt, Na^+Cl^-, sodium ($_{11}Na$) contributes one electron to the outer seven possessed by the chlorine atom ($_{17}Cl$), which attains the electron-number of argon

($_{18}$A), while, simultaneously, the metallic atom obtains the electron structure of neon ($_{10}$Ne). A pinch of salt contains equal multitudes of oppositely electrified particles, all of which have the electron-numbers of an inert gas. Such symmetry is indeed a feature of the most reactive metals; but generally the electronic state of the metal is less regular. It is in most cases easily deduced from the inert-gas configuration which can generally be assumed for the non-metallic partner. Thus iron ($_{26}$Fe) forms two chlorides, in one of which it combines with two chlorine atoms, while, in the other case, analysis shows it has combined with three. In one of these ($Fe^{++}2Cl^{-}$), iron has contributed two of its loose electrons to complete chlorine octets; in the other ($Fe^{+++}3Cl^{-}$), it has, so to speak, satisfied three chlorine atoms by forming a more highly charged ion.

The electrons composing covalent linkages are shared, but not necessarily *evenly* shared, between the two atoms concerned. They may be much more under the control of one partner than of the other, with a resultant uneven distribution of electric charge over the neutral molecule, and " polar " molecules of this type have received much attention.

Again, " resonance " theory has emphasized that in bond formation by electrons every shade of variation is possible between the equipartitioned electron-pair in the hydrogen-gas molecule, H : H, and the stage of complete transference in common salt, $Na^{+}Cl^{-}$.

One most interesting development occurs in compounds where hydrogen is attached to strongly electronegative elements like fluorine or oxygen. Here the hydrogen atom can form a liaison with another atom of fluorine or oxygen in the same, or in a separate, molecule. This is not a normal valency, and the so-called " hydrogen bridge " is a comparatively weak

attachment. It is, none the less, of crucial significance in the interpretation of some formulæ.

Valency theory is thus, like any other theory, subject to continuous modification and refinement. It remains true that the covalent and ionic types of interatomic linkage, based on the electron theory, have been among the most fruitful ideas of this century, have systematized previous knowledge with remarkable success, and have provoked a great body of productive research and inquiry.

The structure and reactions of compounds have become illustrations of electron patterns and of the re-shuffling of these patterns which ensues when new molecules are formed. Throughout all normal chemical procedures the nuclei of the atoms concerned are untouched and unaffected.

New Views on Water

A brief consideration of water will illustrate many of these modern attitudes. The present picture of this fundamental molecule is much more informative than the simple H_2O, or H–O–H, of the last century.

From X-ray examination of ice and from other evidence it has become clear that the molecule is not linear but angled as in Fig. 2, and the control of the valency electrons by the oxygen atom produces, in this formation, a strongly polar structure. This interprets the uniquely great capacity of water as a solvent of salts—*i.e.*, ions—and its ability to induce ionization of some covalent substances like hydrochloric-acid gas ($H{:}Cl \rightarrow H^+ + Cl^-$).

Indeed, water molecules can ionize each other: one water molecule in every ten million is split to hydrogen and "hydroxyl" ions ($H_2O \rightarrow H^+ + OH^-$). This explains the slight electrical conductivity of the purest

water, and also the behaviour of " indicators," whose colour-changes depend upon variations induced, by added materials, upon the proportion of hydrogen and hydroxyl ions originally present.

Again, the hydrogen atoms of water are adapted to the formation of hydrogen bridges. This reduces the volatility of the liquid, which boils at a temperature

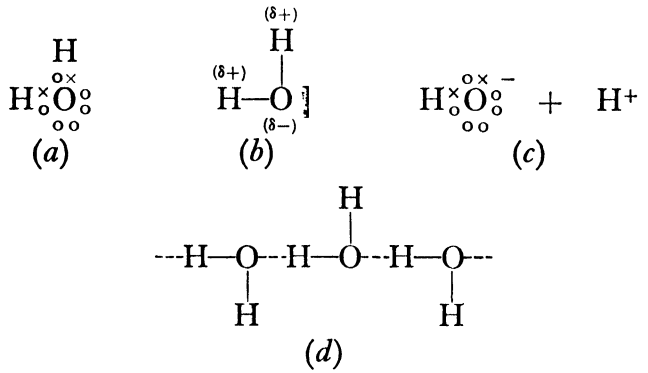


FIG. 2.—THE STRUCTURE OF WATER.

The covalent molecule, H_2O, is angular in shape (*a*). The electron pairs are unevenly shared, in favour of the oxygen atom, so that local distribution of electric forces produces a strongly polar molecule (*b*). A few molecules are ionized as in (*c*). In ice and liquid water, " hydrogen bridges " effect a loose attachment between neighbouring molecules which has important results (*d*).

otherwise difficult to relate to the size and weight of the simple molecule, H_2O. (Fig. 2 *d*.)

Finally, water molecules vary in weight. Less than one hydrogen atom in five thousand is of the " deuterium " type ($^{2}_{1}H$ or D), where a neutron has been added to the proton nucleus of the lighter isotope ($^{1}_{1}H$). The deuterium isotope gives rise to " heavy " water particles, D_2O, and to the intermediate DOH. Thus there are at least three varieties of water-molecule.

Indeed, if we remember that there are three isotopes of oxygen ($^{16}_{8}O$, $^{17}_{8}O$, $^{18}_{8}O$) there are nine types of

neutral particle in water, and two charged ions as well. This is a strange development in a liquid so long the accepted standard of physical purity and chemical homogeneity.

" Heavy " water has an educative history. Inconceivable before the isotope theory of 1905, it was unsuspected until 1931. It was isolated by electrolysis of water in 1932; by 1935 it was a familiar, if expensive, laboratory chemical; by 1940 it was in production on an industrial scale, and its application to the release of atomic energy made its provenance a factor in military strategy.

The application of new theories on the nature of element and atom has thus transformed our knowledge of a chemical so simple and familiar as water, with increased confidence in our interpretation of its many unique properties which had puzzled an earlier generation, and which ramify through a wide range of geological, industrial, and biological activity.

New Techniques of Research

This century has witnessed not only an increase in the power and precision of chemical theories; there has been a great extension in the number and accuracy of available instruments in research. Electrochemistry has inevitably increased its range and significance; magneto-chemistry is in effect a new branch of science, detecting, for example, those properties resultant from the magnetic field produced by unpaired electrons in a molecule.

One of the most unexpected devices available to the modern chemist is the " labelled " atom—he can, and does at times, mark atoms as a naturalist rings selected birds. This is possible in two ways, by the use of selected isotopes rather than the natural elemental

mixture, or by the use of radioactive forms of the common element.

When the substance incorporating these atoms takes part in reaction, new molecules are formed and separated out. The " labelled " atoms are traced by their effect on the specific gravity of the new substance, or by the degree of radioactivity they promote. In this way, some complex and ambiguous processes have been mapped out in detail.

Another powerful aid to research has lately been developed from an old practice. The " purifying " power of charcoal, in decolourizing solutions or extracting poisonous gases, has been shown to depend upon the fact that some molecules stick to (are " adsorbed by ") the surface of charcoal more readily than others. Such processes have received many new applications—other materials like aluminium oxide (alumina) have been found to have exceptional powers of adsorption. Chemists are now able to separate, neatly and quantitatively, substances whose chemical separation had been at best partial and laborious. With a piece of blotting-paper impregnated with alumina it is possible to separate and to identify half a dozen closely related chemicals in a few drops of solution, to replace the previous labour of weeks or months by the work of a few hours.

Great progress has also been made in analysis. " Micro-analysis " requires the researcher to prepare only one-tenth to one-hundredth of the earlier minimum, with a consequent reduction in the time and expenditure which the work involves.

X-ray apparatus has also been of great assistance to modern chemistry, defining the detailed arrangement of atoms in space and measuring the distances between them. Compounds of silicon, for example, have been

extremely resistant to the normal modes of inquiry, since they are so often refractory solids, insoluble and very complex. Yet their importance is unquestionable. Silicates compose the greater part of the earth's crust.

Bragg's investigation of these compounds by X-ray crystallography has provided a long-awaited thread to this labyrinth. The dominating structure was shown to be one silicon atom surrounded by four oxygens; the silicon octet is complete, and each oxygen, now with seven valency electrons, has still to find another partner. Variations on this simple theme are numerous and complex; it has, nevertheless, inspired a rationalization of silicon chemistry which has thrown a new light on the structure of continents, on the geochemistry of all minerals, on the nature of glass, on the composition and function of clay and of soil. Elsewhere, in inorganic and organic chemistry, the science has obtained new clues and new insight from the penetrating " eye " of the X-ray beam.

In this and other instrumental aids, of which the electron-microscope is the latest, modern chemistry has added a battery of new devices to the test-tubes, retorts, and distilling-columns of an older day. The chemist can obtain detailed information on the fine structure of matter with an accuracy and speed previously unimaginable. The field of his competence steadily expands towards ever more complex and resistant problems. No previous period in the history of science has devised more numerous and powerful instruments of research than the last fifty years.

Some Advances in Organic Chemistry

The organic chemist has special problems of his own, as the chemistry of carbon is complicated by the capacity of that element to unite its atoms in chains,

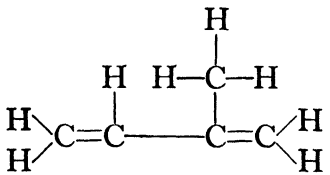
straight or forked, and in rings of many kinds. This entails an exceptional degree of " isomerism," due to the various ways in which a similar number of atoms can fit covalently together to provide distinct and different molecules. Thus there are five forms of hexane (C_6H_{14}), and more than ten thousand hydrocarbons are possible with the common formula $C_{16}H_{34}$.

Of the many structures possible from the thirty-five atoms in $C_{15}H_{11}O_4NI_4$ only *one* is the hormone thyroxine—of a thousand possibilities from $C_{40}H_{56}$ only one precise arrangement is carotene, the biological precursor of vitamin-A. The organic chemist by his devoted skill and ingenuity can piece the evidence from analysis and reaction together until he has determined the exact structure of each component ring or chain, has sited every atom of the structure in its exact position. Then he sets himself to the task which alone brings final conviction—the synthesis of this abstruse structure from simpler structures whose architecture is already established.

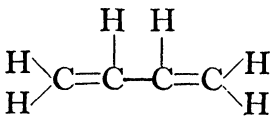
Spatial isomerism is not the only factor of its kind—the so-called " stereo " and " optical " isomerisms add further perplexity in many cases. Glucose (grape-sugar) is only the most important of many substances with the general formula $C_6H_{12}O_6$. Although this sugar was first described in the seventeenth century, it is only in our own day that final agreement has been reached on the precise arrangement of its atoms, in a fashion which can account for its chemical and physical properties. A firm classification of the many types of sugar, like the synthesis of many vitamins and hormones, is among the major achievements of the science in this century.

Yet those mentioned are indeed among the simpler and smaller molecules found in natural products.

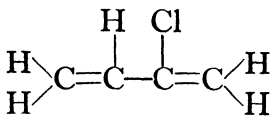
Natural proteins, for example, are often " macro-molecules " with thousands of atoms arranged in their structure. Here, and especially in the last fifty years, the secret has been found to lie in some simple chemical structure which may be repeatedly and



Isoprene, C_5H_8



Butadiene, C_4H_6.



Chlorbutadiene, C_4H_5Cl.

FIG. 3.—ISOPRENE AND RELATED STRUCTURES.

Isoprene is the unit structure which, multiplied many times, composes the macro-molecules of rubber. The other substances are of similar structure and can be condensed to useful synthetic forms of the material.

variously condensed to provide " macro-molecules " of many kinds. One arrangement of some glucose units may provide starch, another cotton, a third the cellulose of wood.

Another unit is the hydrocarbon " isoprene " (Fig. 3). This is basic to the architecture of the vitamin-A molecule; it has also been identified as the basic unit of rubber. Although it is not easy to provide an economic supply of isoprene, the related substance butadiene can be prepared from petroleum and another

relative, chlorbutadiene, can be synthesized by a process employing coal. When the Allies lost their supply of natural rubber to the Japanese, knowledge of these alternative units enabled them to produce, without trees, thousands of tons of rubber substitutes.

Similarly, in our own time a knowledge of the units in the structure of silk has led to the mass-production of " nylon " from coal, water, and air, so outmoding the customary worms and mulberry leaves.

These researches have introduced new industries of " plastics " and other substitutes. They have encouraged research on further, previously forbidding, themes. Alert minds are pursuing the pattern of hæmoglobin in blood; others are contemplating the possible chemical structure of those genes whose presence in the chromosome holds the secret of heredity. The chemistry of " macro-molecules " has, at least, been triumphantly begun; its progress will have important effects in medical and biological science.

One problem concerning a numerically simple molecule has engaged the full equipment in theories and instruments of the modern chemist. This is the problem of the " benzene ring," containing the twelve atoms, C_6H_6. It has long been agreed that the six carbon atoms, arranged in a hexagon, are each attached to one of hydrogen. It has never been possible to arrange the valencies between the carbon atoms in a manner fully interpretative of this chemical's behaviour.

The problem is an important one. The whole class of " aromatic " chemicals, many thousands strong, incorporate this particular type of ring, and these include the dye-stuffs, the drugs, the carcinogenetic compounds from coal-tar as only one significant group.

The present solution supported by X-ray analysis, in consonance with quantum-mechanical consideration

of the four valency-electrons of carbon, is that the carbon atoms are in a regular flat hexagon, and that the carbon–carbon distance (1·4 Å.) is intermediate between the length of a single bond (1·5 Å.), and that of a normal " double bond " requiring four shared electrons (1·3 Å.). The molecule is a " resonance hybrid," in which the carbon atom (tetravalent, group IV) can to some extent simulate the boron atom (trivalent, group III) extending three valencies at angles of 120°. The six electrons apparently unemployed contribute to the unusual chemical character of this formation. Although chemists can find much satisfaction and stimulus from such a presentation it serves as a reminder that " simple " structures of a few atoms can resist full explanation as well as the more complicated molecules. Here the more satisfactory interpretation had to await the development of advanced theories and analytical devices.

Summary

It will be realized that the discovery of radioactive matter, of elements whose weight and character changed with time, closed a notable chapter in the history of chemistry.

Previous advances had been based on the fruitful idea of indestructible elementary atoms. Their individual and group characteristics were mysteriously dependent on the weight of the primal stuff which composed their structureless, impenetrable particles. This theory had culminated in a Periodic System which comprised the infinite variety of material forms as products resulting from the combinations of some ninety elements. The combining power, the valency, of these elements allowed their further classification into nine families.

The response of physicists and chemists in this century to the stimulus afforded by the discovery of radioactive and isotopic elements has culminated in a very different atomic model, which is nucleated, largely empty, complex, and dynamic. The simplest evaluation of its scientific worth is that the modern chemist interprets the variety and variability of matter in terms of only *three* essential particles—the proton, the neutron, and the electron.

Although the atomic weights of the naturally occurring elements remain factors of great practical value, constantly under review by every refinement of analysis and assay, the atomic numbers are now the primary factors in the characterization of atoms.

The atomic number, or nuclear charge, defines the number of planetary electrons; and the astonishing conclusion has been reached that chemical compounds and their reactions are to be explained by simple changes in pattern among a few of the peripheral electrons, or by a limited number of electron-transfers in the group of atoms which participate. The frequency with which an octet pattern results often reduces the rationalization of molecular structure to the easiest type of arithmetic. The possibility of covalent or ionic linkages between atoms accounts for the difference in conductivity and other properties which has so long been noted as effecting a useful division of chemical compounds into two major groups.

In one sense this type of material change is curiously limited: it prevails only over the temperature and pressure range which typifies the resources of a planet. Stellar conditions are cosmically much more frequent; and they provide interior temperatures measured in millions of degrees, and pressures of corresponding intensity. The result is a chemistry of *nuclear*

collisions and re-arrangement. The sun is a vast atomic pile, where the electron screens of " terrestrial " chemistry are torn and stripped away. When this is imitated by certain types of physical apparatus, the dream of the alchemist comes true: mercury can be, and has been, turned into gold—but not by the forces released in simple chemical reaction. The alchemist's error was one of means rather than ends: his processes achieved only the clashing of peripheral electron groups. His furnaces were too cold.

The social services of chemistry in this century have been dramatic and effective. The atmospheric fixation of nitrogen; the provision of cheap and abundant aluminium; the supply of D.D.T. and the " sulpha " drugs, of synthetic fibres and constructional materials by an extended application of the potentialities of coal and petroleum: these will serve as examples. The list is growing and will continue to grow, for such possibilities increase in direct proportion to the refinement of our knowledge concerning the elements and their powers of combination.

In the preface to his *Chemical Elements and their Compounds* N. V. Sidgwick has written recently, of our present ideas on the structure of atoms and molecules, that " the development of these ideas in the last thirty years has made it possible to transform ' Inorganic ' Chemistry from a mass of disconnected facts into an ordered system of relations."

Development still goes on. Each advance discloses new horizons. The tempo of progress continues to increase. The atom of 1950 epitomizes a half-century of research. The chemist of the year 2000 will express his knowledge, and ours, in another and a better model. He will smile at our simplicity.

GEOLOGY

PROF. DUNCAN LEITCH, D.Sc., F.G.S., F.R.S.E.

IN writing of the contributions to thought which have been made by geology during the present century it is difficult to avoid making frequent reference to those of the century that went before. This earlier period opened in what has been called " the heroic age of geology," when great and exciting discoveries were being made and when in fact the main fundamentals of the science were in process of being established. The discoveries of William Smith, Cuvier, and Brongniart, the writings of Lyell and Darwin, and the two great concepts of Uniformitarianism and Organic Evolution were about to bring geology into the forefront of nineteenth-century science. In the years that followed to the end of the century, the science that had waited so long for the necessary stimulus acquired an energy which led in a relatively short period to the foundations of principles in most of its branches which are the basis of present-day geology.

Within recent years reference has frequently been made to the decrease in public interest in theories regarding the nature and the history of the earth. While this is undoubtedly true, it must be realized that much the same can be said of the other sciences. The nineteenth century was in fact the heyday of the amateur scientists, and geology, being in its infancy, was particularly attractive to the layman. Today not only has the public attitude changed to science in general, but these sciences themselves have changed; specialization and detailed analysis have forced the amateur to leave the final solution of problems to the

professional and to accept for the moment many of his findings. Nevertheless, it is somewhat surprising that, in a civilization which is so influenced by the results of geological science, many of these early nineteenth-century discoveries have yet to find their way into the modern educational system. Today we may claim that our knowledge of the earth's crust is expanding more rapidly than at any time since the " heroic age." Professional geologists are engaged in exploration and mapping in almost every country in the world, and the importance of the application of geology to the problems of mineral resources is reflected in the pattern of everyday life, in which fuel and power have become such dominant factors. But of even more importance are the advances which have been made in what for lack of a better term I shall call " pure geology." The detailed and systematic collection of fossils from ascending sequences in the stratified rocks has yielded more information regarding the history of life through geological time and has provided biologists with information which has been essential to the elucidation of their evolutionary problems. The extension of the work of the nineteenth-century stratigraphers has led not only to a refinement of subdivision of their original formations, with the fuller understanding of the palæogeographical condition which led to their deposition, but also has, with the extension of zonal correlation, culminated in the production of detailed geological maps which form the work of most of our professional geologists.

The past fifty years has been a period of consolidation and of the filling in of detail, either from minute work in well-explored areas or from pioneer work in areas of which hitherto little has been known. But

yet the full story of the earth has not been told: much of its surface still awaits surveying, and many conflicting theories regarding its configuration and early history have yet to be judged.

The Earth's Crust

Although the geologist has been mainly concerned with the study of the rocks as we see them at the earth's surface and with the processes that have gone to their making and shaping, he has always been faced with other problems, the final solution of which will be achieved only by collaboration with workers in the other sciences. The origin of the earth as a planet and the early stages of its history, the original distribution of the continents and oceans, and the gigantic forces upon which the geologist is forced to speculate in any explanation of their present distributions are obviously a part of that middle ground which lies between physics, astronomy, and geology. The physico-chemical processes which have been involved in the origin especially of igneous rocks, and of their subsequent disintegration, and the distribution of the elements in the earth's crust are of as much interest to the chemist as to the geologist. With the advance in knowledge of the physical sciences in recent years it was not surprising that many new fields of inquiry in geophysics and geochemistry should have been opened up. Earlier theories have had to be re-examined in the light of new facts, geological, physical, and chemical, and in some cases they have ceased to be accepted. Many of the new theories, however, have factors which are still speculative, and it is probably this field which has yielded the most controversial aspects of the science in the present century.

The recent scientific study of earthquakes, for

example, has resulted in an increased knowledge of the inner structure of the earth and its bearing on the earliest stages of its evolution. While certain earthquake waves travel from the focus of origin along the surface of the globe, it is now known that others pass directly along nearly straight lines through the deeper parts of the earth, their rates of travel being related to the nature of the material through which they pass. It has therefore become obvious that the study of seismographic records should give some information regarding the constitution of the globe below the visible crust. As a result of these investigations it now appears that the actual crust of the earth extends downwards for a comparatively few miles and in fact forms only a thin skin. Beneath this skin the materials forming the inner earth are arranged in concentric shells, each with a fairly sharp boundary, the deeper layers being of progressively higher density. With regard to the condition of the core itself, there is still considerable doubt; that the material beneath the crust is often hot is shown by a variety of types of evidence, but this does not necessarily mean that the centre of the earth is in a molten condition. Owing to the pressure which exists at depths, it would seem probable that it is in fact in a solid state, but against this assumption there is the seismological evidence that the core, some 6000 miles in diameter, does in fact act as a fluid and diffuses any seismological distortion waves which enter it.

Bearing in mind the two lines of information regarding firstly, the increasing density of the inner earth and, secondly, the concentric shell-like arrangement of its constituent layers, it is possible to draw some conclusions regarding its early history. If we assume that the materials now composing the earth were once

evenly mixed together in a more or less homogeneous mass which solidified in cooling, it must be inferred that there was an early gravity separation in accordance with density. Following this theory, therefore, it is claimed that the original earth's crust rose to the surface of the globe more or less as a scum of lighter material. It must be remembered, however, that as early as 1900 Laplace's nebular hypothesis of the origin of the earth was recognized by the astronomers as unacceptable, and Chamberlain's suggestion that it originated as a great mass of small solid bodies, or *planetesimals* which were separated from the sun by the attraction of a passing star, postulates an earth which grew by the slow addition of solid material and therefore did not pass through a liquid stage. However, a further modification of Chamberlain's hypothesis by Jeans and Jeffreys favours a molten condition, although of a non-nebular origin, and it would seem that while the question is still unanswered their theory is more acceptable to the geologist.

Further seismological investigations indicate that the crustal rocks which form the continents are essentially different from those which form the deep oceanic floors, that the crust, some 40 miles thick, in fact comprises two different kinds of rock, one (called *sial*) which is thick underneath the continents and reacts to seismic vibrations in a manner which suggests that it is similar in composition to granite, and the other which is probably of basaltic composition and is called *sima*. It is now known that the continental sial is either absent or very thin under the ocean floors, and it is believed that the continents float as great rafts of sial on a substratum of heavy basaltic sima. Something of this idea was embodied in the earlier isostatic theories of Airy and Dutton, but these twentieth-

century observations have not only verified it but have added greatly to the evidence.

Continents and Oceans

The concept of great masses of sial floating on a basaltic substratum was carried a stage further by Wegener in 1915 and later by Du Toit in an attempt to explain the early history and the present distribution of the continents and oceans. The distribution of these, the greatest units of the earth's surface relief, has long attracted speculation by geologists. All have been impressed by the fact that although there has been considerable evidence in geological times of interchange of land and sea by relative uplift and subsidence, this interchange has been confined to the shallow seas and to what amounts to little more than the margins of the continents. For example, considerable areas of Russia and of Northern Canada have not apparently been submerged since very early times, while again there is little evidence in our sedimentary rocks of deposits which have originated in the depths of an ocean; they are mostly of relatively shallow-water origin. In the nineteenth century this led to the belief that during geological time the positions of the ocean basins had remained unchanged. The Theory of the Permanence of Ocean Basins claimed that the present large-scale relief was established early in the history of the crust. There were, however, certain difficulties inherent in this theory, the most important of which was the similarity of terrestrial faunas and floras in the rocks on either side of the oceans. This similarity is so marked that connection between the land masses during at least some stages in earth history is essential. The nineteenth-century theorists surmounted this difficulty by the proposition that land-

bridges may have developed periodically across the oceans, but the idea has never been satisfactory; while the migration of some faunas and floras may have required only an under-sea ridge or even a series of islands, the similarity of the equatorial forests of Carboniferous times in Europe and America demands something much more widespread.

Wegener suggested that the continents had once been united and that their present distribution had been initiated at the beginning of Mesozoic times, when this great sial mass broke into raft-like fragments and slowly drifted apart; the Atlantic Ocean was thus formed as a great gap between the drifted masses now represented by the Americas on the one hand and Europe and Africa on the other. Several variations of this idea were developed, the most notable of which was that of Du Toit, who suggested that the separation had taken place by stretching and thinning of the sial rather than by drifting, but the main criticism has been on the grounds that the hypothesis would require a force of great magnitude, and no suggestion that has yet been made as to the nature of such a force has proved acceptable. On the other hand, adherents claim that so far there is no other explanation which does not demand an equally gigantic and equally speculative mechanism. Today opinion is still divided, although the balance is probably in favour of accepting the hypothesis. While it still leaves unanswered such questions as the concentration of land in the northern hemisphere, the antipodal arrangement of continent and ocean, and the characteristic triangular outline of some of the main continental masses, all of which were embodied in the earlier Tetrahedral Theory of Lothian Green, it does explain many geological "coincidences" which are unanswerable by any other theory. Before

leaving it, let us consider some of these coincidences on either side of the Atlantic Ocean which have made the idea so attractive.

One of the first points to be noticed is the general similarity of their coastal outlines. This is so close as to suggest that if they were brought together, they would fit in an almost jig-saw manner. The fit, of course, must not be thought of as perfect, for it must be remembered that the present coasts as we see them are not the true edges of the continents but are separated from them by a continental shelf which is covered by shallow water. Nevertheless, even the approximate fit would seem to be too close to be accidental. Moreover, it becomes even more marked when we consider some of the geological structures and deposits on either side. Several of the great mountain-making movements in geological history are known to have taken place at approximately the same time in Europe, Africa, and the Americas. Although some of these great systems are now represented only by their denuded roots, they can be recognized, and the direction of their folds and the trend of the original mountain chains can be mapped. If we now restore the process of the alleged drift we find that these trend lines pass continuously from one continent to the other. Of particular interest in this respect are the two ancient mountain chains that cut across Britain—one from Scandinavia, through Scotland to Wales, the other in an east–west direction from Europe cutting across southern England and Wales. According to their trend these two chains should cross each other somewhere west of Ireland : actually what would appear to be their meeting-place occurs in North America.

Even more impressive is the evidence which has been obtained from the study of past climates. This

is particularly true of the period of glaciation which occurred shortly after the time of the formation of our coal seams. Much of this ice age, which is known to have affected South America, South Africa, Madagascar, southern India, and Australia, appears to have been most important in areas which are now near the equator. The ice would seem to have moved in some cases *from* the equator and to have passed on to the continents as though it originated over the oceans. These apparent anomalies are removed if it is supposed that the continents just mentioned were then in contact with one another. It is suggested that this great continental mass may have been in some position far to the south of the equator during the time of glaciation.

The Age of the Earth

In the understanding of geology the most vital fact which must be accepted is the length of time which has been involved in the working of its processes. The realization of this fact was the turning-point in the growth of the science, and by the beginning of the century geologists had become convinced that an age of many millions of years was necessary in order to make room for the known episodes in the earth's history. It is true that geologists had at that time become involved in some difficulty over this question of the earth's age, because many physicists, headed by Lord Kelvin, were inclined to emphasize the rather limited period during which the earth had been in existence; judging from the rate at which the earth was believed to be losing its heat it was held that it could not have taken much more than 40 million years to cool to its present temperature.

The discovery of radioactive minerals in the earth's crust in the early years of the century completely

invalidated Kelvin's method of calculation. In the first place the discovery of a widespread distribution of these radioactive materials by Rayleigh provided automatically an unexpected source of heat, and in fact it became doubtful as to whether the earth is cooling at all. Perhaps even more important, radioactivity gave to the geologist a means of arriving at a numerical age-value which would seem at the moment to be acceptable on scientific grounds and which gives results which are much more in keeping with our ideas regarding geological processes.

The radioactive method of age determination is dependent on the fact that uranium and thorium, in a process of atomic disintegration, break down through a series of unstable radioactive elements to culminate in the production of helium and of certain isotopes of lead. Rutherford and Rayleigh demonstrated that these changes take place at a speed which is fixed and measurable and is unaffected by changes in pressure, in heat, and presumably—and here is the only assumption —also in time itself. If a uranium mineral is found which has disintegrated to a certain percentage of uranium–lead, an estimate can be made of the time which has been taken for this condition to have been reached. Since the discovery of this method many analyses have been made, and results have been arrived at independently which have not only shown agreement but which have also been roughly verified by the original *relative* time-scale of the stratigrapher. Estimates up to 3,000 million years have been arrived at for some of the oldest known rocks and of 500 million years for the oldest known fossils. Recently Holmes has carried this a stage further and has given figures for the beginning and the duration of the geological periods. This establishment of a time unit in the

history of the earth is comparable with the measurement of astronomical space and is probably geology's most important contribution of the present century.

Igneous Rocks and the Origin of Granites

In the generally accepted classification, the rocks of the crust comprise three broad groups—Sedimentary, Igneous, and Metamorphic. The first consists of such rocks as sandstone, shales, grits, and conglomerates; they are fragmentary in character and are stratified in arrangement and are mainly the products of weathering of a land surface. The second are distinguished by the fact that their minerals are crystalline in character; they are for the most part non-stratified and are believed to have resulted from the cooling of a silicate melt or molten rock magma which has ascended from a sub-crustal reservoir, either to be extruded at the surface as lava or intruded at various levels in the earth's crust. They comprise an enormous variety of rock types with a wide range in chemical and mineral composition. The metamorphic rocks have resulted from various processes of alteration of pre-existing rocks which may originally have been either sedimentary or igneous.

Late in the nineteenth century a new era in the study of rocks was initiated when Sorby showed that by a suitable technique they could be examined by transmitted light under the microscope. By this method the optical properties of the constituent minerals, and their relationship to each other, could be determined. It was immediately taken up by Zirkel and Rosenbusch in Germany, and a flood of knowledge, especially with regard to igneous rocks, began to rise and has continued until the present day. During the early years of the present century this branch of the science was mainly

descriptive and was based on the microscopic study of rocks, reinforced by quantitative chemical analysis. These years were marked by detailed descriptions and by the development of many elaborate classifications. With the accumulation of knowledge, and under the influence of the newly growing science of physical chemistry, philosophical studies dealing especially with genetic relationships and magmatic processes acquired an added interest, and there followed a period of theoretical and experimental work on the chemistry of silicate melts which has become such an important part of modern petrology. Magmatic conditions have been simulated in the laboratories, and the stages in growth of the constituent minerals have been studied. The problem which has provoked most discussion has been that of the differentiation of lavas and of rock types. There would seem to be little doubt that magmas have been capable of producing derivative magmas which are quite unlike the original melt, and various explanations such as crystallization-differentiation, the separation of two or more liquid fractions, or the assimilation of surrounding rocks, have been suggested. In this field of research the Geophysical Laboratory at Washington has played an important role.

There is little scope here to discuss these problems of petrogenesis, but it will perhaps suffice for our present discussion if we emphasize again the three main points enumerated in the first paragraph—that the sedimentary rocks are mainly the result of fragmentation and disintegration of pre-existing rocks and that the igneous rocks originated in a molten condition, while the metamorphic rocks have been formed by the *alteration* of pre-existing rocks. These have been widely accepted for many years as fundamental

principles. Recently, however, considerable controversy has arisen over the claims that some of the so-called igneous rocks, in particular many deep-seated acid types, which we shall refer to generally as the granites, did not originate in a molten condition and are in fact much more closely related to the metamorphic than to such igneous rocks as volcanic lavas. This theory, which involves a process " by which solid rocks are *converted* to rocks of granitic character without passing through a magmatic stage," is not exactly new, having had its origin during the last century in France, where it had many supporters, but it is only within the last few years that it has received the attention which it merits in Britain and America. It invokes the action of ascending emanations or juices which periodically, during mountain-making movements, have invaded the already existing rocks, soaking through their pores and structural planes and gradually converting the material to a rock of granitic character which, if this interpretation of origin is correct, must be regarded as of metamorphic type. There are many different views as to the nature of these emanations; even the term " emanation " is by no means universally accepted. Moreover, it is admitted by some that in the process of alteration there may have been phases in which the newly-formed rock mass has become fluid and may therefore have been capable of invading, to some extent, the surrounding sediments. Nevertheless, its ultimate origin, it is claimed, was non-magmatic. This does not mean, of course, that true igneous rocks of molten origin do not exist. While it is evident from any active volcano that magma does produce basalt lavas, it must be remembered that most lavas, ancient and recent, are of basaltic character; acid lavas are comparatively rare and we have little

actual experience, therefore, of a magma which under deep-seated conditions would produce granite. Apart altogether from the granitization controversy, this is now accepted as fact, and it would seem that there are indeed two distinct sub-crustal agencies, one ascending from the basaltic or sima layer and the other from the granitic or sial layer. The new theory would suggest that the former is magmatic and produces igneous rocks (mostly of basic or sub-basic composition) while the other is a granitizing fluid which produces granites. The importance of this concept lies not only in the explanation of the origin of one or a few rock types but also in the introduction of a new sub-crustal agency and its influence on the fundamental concept of petrogenesis, ore-formation, and metamorphism. At the moment geologists are divided in their opinion, and though the anti-magmatists would seem to be gaining ground it is to the future that we still must look for the elucidation of the problem.

The Study of Fossils

In 1927 Bather wrote: "The intimate analysis of large series of fossils collected with anxious precision from successive feet, or even inches, of stratified rock is surely a research that should illuminate the course and method of evolution, that should clarify our views as to the nature of species—the elements of our classification, and that should make manifest the affinities of genera and larger groups."

This passage is quoted from one of the most advanced palæontologists of recent times because it so aptly summarizes the lines along which the study of fossils has progressed during the past fifty years—years that have been characterized not by the development of

new theories or even of unexpected discoveries but rather by an increased conviction that the ultimate aim of palæontological thought, and the only solution to its problems, lies in the detailed analysis of evolutionary change as expressed in stratigraphical sequences; the broad generalizations with regard to the possible relationships of the larger taxonomic categories, which were so typical of much of the work in post-Darwinian years, have been replaced by a search for phylogenetic affinity.

The pioneer work of Rowe in 1899, in which he reminded us of the simple biological truth that evolutionary processes act on populations rather than on individuals and that evolutionary progress is the summation of changes in characters in the individual rather than the wholesale development of new species, was to form a basis for much subsequent work in many groups of fossils. Rowe collected some thousands of specimens of a small sea-urchin, *Micraster*, from an unbroken sequence of strata in the chalk. He showed that while each successive community was broadly similar to its immediate predecessor, a detailed examination of certain characters which constituted the individual showed that change was taking place in the whole series of populations; thus those at the top of the sequence were easily distinguishable from those at the base. The change, however, was almost imperceptible, and at no point could a line be drawn to indicate that a new species had been created. Once Rowe had established the sequence of changes in his type area, he examined equivalent rocks elsewhere and showed that they again exhibited comparable changes. He consequently established a series of zones within the Chalk which could be used to correlate any particular part of the formation.

A result of the analyses of fossil series has been the acquiring of an intimate knowledge of the detailed evolutionary history of many of our fossil groups and of some of the processes which have been involved in these changes. Modern palæontology has been marked by the erection of lineages in an attempt to classify fossils not by the older systems which depended largely on morphological resemblances, but on descent. Whether we can ever hope to know enough of the evolutionary history of all fossils—and even so, whether a satisfactory classification could be built out of this knowledge—is still an open question. Unfortunately palæontology has to satisfy two needs—biological and stratigraphical, the former in the search for evolutionary truths, the latter in the recognition of fossils for use in stratal correlation. Consequently, many difficulties in nomenclature have arisen in an attempt to satisfy both needs. The position is at present one of compromise, and various means have lately been suggested for expressing the smaller taxonomic units as a more fluid concept in such a way as to make it intelligible and useful to the stratigrapher and yet indicate something of the evolutionary history of the group.

The need for understanding the detailed evolutionary history of the smaller taxonomic units, of the processes involved, and of a concept of species which will be acceptable to stratigraphers, has become apparent in modern work on zonal correlation. Admittedly this technique was developed late in the nineteenth century, frequently with little reference to descent or to homeomorphy and variation with their attendant problems in nomenclature, but it is in the present century that we see its fullest development.

A further result of the study of fossils in series has

been that the geologist has become impressed with what he believes to be a directiveness or a rectilinearity in the course of evolution. This idea has been most strongly expressed by Lang, who, in a series of stimulating papers, has shown that many groups of invertebrates have tended to change in only a limited number of directions and that these directions of change have been repeated several times in stocks which are not always related. The fact that many of these trends appear to have no recognizable functional value and that they would seem to have frequently led to ultimate extinction has suggested that the changes have taken place under the influence of some " internal factor." There is no doubt that such limited trends have taken place; it has been shown, for example, that the graptolites evolved along only a few lines and that these changes were repeated in many lineages so that most of the features which differentiate species are to be interpreted in relation to the stages reached in a number of trends. Changes undergone by the Cretaceous Bryozoa have been found again to be limited and again widely repeated, consisting mainly of " an increased calcification of the skeleton which ultimately appears to have overwhelmed the organism owing to exaggerated development." Numerous other examples from other phyla could be quoted; the prevalance of homeomorphy in certain groups is itself proof of repeated change in a similar direction. But while the existence of trends is admitted, the influence of an internal factor in the sense of determinative variation or of a predestined course in evolution, is denied. Most geologists of the present day would tend to interpret the words " internal factor " as something related either to the physiological or the genetic constitution of the organism. With regard to the

latter it is interesting to note that modern geneticists in their work on induced mutations have observed not only that similar changes may occur repeatedly in one species but also that closely comparable mutations in different species may occur. It would seem, therefore, that such species have inherent possibilities for mutation and that these internal factors may limit the scope of variability.

Geology and Mineral Resources

While emphasis has been placed largely on the theoretical and more controversial aspects of recent geology, it must be remembered that most geologists of the present day are engaged in the search for mineral resources in this and in other countries. It is beyond the scope of this article to discuss the results of their researches or even the methods which have been employed. Nevertheless, they have given us such a vast amount of valuable geological information, and have utilized techniques which in the future will undoubtedly make their contribution to fundamental research, that they merit some introduction to the present discussion.

The search for fuel, coal, and petroleum has been responsible not only for an enormous expansion in the demand for professional geologists and in the development of geological surveys but has yielded knowledge which must have remained hidden but for the added incentive of industrial needs. Thousands of borings, some of them 15,000 ft. deep, have provided " a third dimension " in geology which was unknown to earlier geologists. Many of these have verified much that was speculative, others have stimulated research which has had considerable repercussions in our ordinary academic problems.

Geophysical Exploration

Much of the preliminary geological exploration in oil-producing countries consists of a search for great up-foldings in the sedimentary rocks, for it is in such structures that oil tends to accumulate. These structures are, of course, sub-surface and can be deduced only by geological mapping, and their detailed extent later verified by an exhaustive programme of boring. Consequently, any techniques which will reduce these preliminary stages are constantly being utilized. The most important of these have been devised by collaboration with the physicists and depend on the accurate measurement of anomalies in certain physical properties of the earth, such as gravity, magnetism, and the differing capacities for the conduction of electricity or the transmission of seismic vibrations.

These physical properties are not constants, and their variations are controlled largely by the nature and arrangement of the underlying rocks. Geophysical prospecting methods are based therefore on the belief that if a large number of physical anomalies are mapped at the earth's surface, deductions can be made regarding the sub-surface structures which have influenced them. The general method employed is the same—that is to say, a series of traverses are made across the surface of the ground to be explored and the anomalies are recorded. There are four main lines of investigation—gravitational, magnetic, electrical, and seismic. Gravity variations are measured by the Torsion Balance, which is capable of recording variations in gravity to a millionth of its normal value. These variations are influenced by density and by the structural distribution of the rocks, so that, for example, the great salt domes of the Gulf Coast of the United

States, which are associated with our most important oil deposits, have yielded the greatest success by this method. The magnetic methods, on the other hand, have been applied mainly in the search for magnetic ores, but many rocks, owing to their mineral constitution, have an increased magnetism the recording of which can be used to trace the probable boundaries of the rock mass.

The seismic method is perhaps the most interesting and consists of detonating an explosive within a shallow bore-hole and recording the behaviour of the artificially produced seismic vibrations on a series of seismographs placed at suitable positions with respect to the structure under examination. These waves, in traversing the crust, will be either refracted or reflected where they encounter any considerable change in rock structure, and the results will be apparent on the seismograms.

The seismic method is of greatest value in determining the depths at which there is a marked discontinuity in rock types, and recently it has been applied to problems of a more academic nature, especially in Britain. It has long been known that the Mesozoic rocks of the South of England lie with marked discontinuity on an ancient Palæozoic floor. Little is known regarding the nature and structure of this floor, but the recent search for oil in Britain, in which geophysical prospecting has played such a prominent part, has yielded some information which, it is hoped, will be greatly extended in the near future. Considering that the concealed coalfield of Kent lies within this Palæozoic floor, and that the rich measures of Wales occur in an exposed extension of it, there is little need to point out that the investigation would not be an entirely academic one.

It should perhaps be emphasized here that by far the greatest part of the earth's crust is covered by sea-water, and is therefore not subject to the normal methods of geological surveying. It is therefore not surprising that our knowledge of these sea-covered areas has lagged far behind that of the land. Recently, however, various methods, mostly geophysical, have been developed which have yielded information regarding the deposits, the structure, and the relief of the ocean floors. They promise to mark a new epoch in geological research. A modern coring device, for example, by which a long tube is driven into the ocean muds by means of an explosive, has for the first time revealed, in their chronological order, the extremely fine sediments which have been deposited within comparatively recent geological times.

No less interesting are the problems related to submarine topography, especially of the Continental Shelf, which have arisen as a result of sonic soundings. These have revealed the surprising fact that the Continental Shelf is by no means a featureless floor but has frequently been excavated to canyons, sometimes of gigantic proportions; many of these are under-sea continuations of great rivers of the present day, but others have no relation to present drainage and start far out on the Continental Shelf. Obviously the mode of origin of these valleys presents a problem which is of fundamental character; if they resulted from the normal sub-aerial agents of erosion then a relative change in sea-level of at least 10,000 ft. since the Pleistocene glaciation would require to be postulated. A much more likely explanation has been suggested by Daly, who invoked a new process of under-sea denudation by means of the heavily charged suspension-currents which must have issued to the Continental

Shelf during glacial times, but whether such an agent could have been capable of producing, for example, the Hudson submarine-canyon, which is fifty miles long, six miles wide, and over three-quarters of a mile deep, is still an open question.

In such a brief survey many important achievements have had to be omitted. The fuller understanding of Alpine tectonics and the application of the concept of nappes to the elucidation of the geology of the ancient mountain systems, especially in the Highlands of Scotland; the investigations of the mechanism of igneous intrusion which followed the classic analysis of the volcanic complex of the Island of Mull; the development of X-ray techniques in the examination of mineral structures which has opened up a new field in the study of the clay minerals: all have made their very real contribution to the geology of today.

ZOOLOGY

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IN this short article I shall endeavour to give an outline sketch of the history of zoology—mainly British zoology—during the past fifty years. The history of a science is, no doubt, duller than most other sorts of history, partly because it has an antiquarian more often than a genuinely historical flavour, but mainly because the body of received scientific knowledge does in some way contain its history within itself. Moreover, there is no particular coherence in the recent history of zoology and little sense of necessary connection between the successive steps of its progress. Zoologists have not yet made up their minds about the strategical objectives they are trying to secure. They are like footballers in a London fog who, while deeply inspired by the spirit of the game, hold conflicting views on where the goalposts are supposed to be. A single example will be enough to make this clear. To one group of zoologists, the final purpose of their studies is to reveal the process of animal life in nature; if an animal is brought into a laboratory, the conditions under which it lives and is to be experimented on must be a faithful reproduction of its habit of life out of doors. If not, observations made upon it are irrelevant or misleading, because "artificial." To this, zoologists of a second group reply that the "nature" so spoken of has over the greater part of the earth's surface long since ceased to exist, since it is a nature purged of its most influential ingredient, man. The naturalist in this sense, they may argue, is still befuddled by the literary propaganda of the romantic revival: the real, true

nature he shuts his eyes to is not the babbling brook of a now obsolete poetic imagery but a brook contaminated by factory waste or the mud stirred up by the bathing of boys upstream. Moreover, the behaviour of an animal in the laboratory—of a rat, say, in a maze—is certainly not less interesting than its behaviour out of doors, and is almost certain to be a great deal more informative. So it is that there has grown up a deep and deeply undesirable cleavage between experimental zoology and descriptive field ecology—" nature study." Its practitioners hardly speak the same language; which is not surprising if they cannot even come to terms over what " nature " is supposed to be.

However diverse they may be, all the great achievements of modern zoology are the outcome of an *analytical* treatment of biological problems. It is a curious and contrary fact that speculative or " theoretical " biology (wrongly so called) has been almost wholly anti-analytical in temper. Lloyd Morgan's *Emergent Evolution* (1923), General Smuts's *Holism and Evolution* (1925), the *Gestalt* theory of Wertheimer, Kohler, and Kofka, and much of the writing of J. S. Haldane and E. S. Russell have all been concerned in one way or another with the rehabilitation of the Whole. Zoologists who accept their precepts—in practice, few do—are habitually suspicious of laboratory experimentation; they have come to believe that by bringing an animal into a laboratory some secret mischief has been done to it as a result of which it ceases to be a Whole and becomes a mere assembly of its constituent parts. It is, indeed, hard to admit that the theoretical biology of the present century has done the science much credit. Yet Hans Driesch, biology's first Gifford lecturer, gave theoretical zoology a good send-off in

1907 with a work genuinely analytical in spirit,[1] even if marred by the scholarly sleight-of-hand with which he introduced the parameter *E* (for Entelechy) into his fundamental equation for development. We were obliged to wait until 1929 for the first full-scale analysis of the stock problems of theoretical biology, in J. H. Woodger's *Biological Principles*. Woodger's work has since developed along more austere lines (*The Axiomatic Method in Biology*, 1937) in the belief that everyday language is neither subtle nor discriminating enough for the formal analysis of biological problems, and the hope that the theorems of biology may one day be rendered into an axiomatic system. In the main there is very little that can be called " theoretical " biology in the sense in which we speak of " theoretical " physics, but A. J. Lotka's *Elements of Physical Biology* (1925) is a most notable exception.

The distinction between analytical biology and its opposite (which is hardly yet coherent enough to have earned itself a special name) is partly co-extensive with the antithesis between vitalism and mechanism, and yet again with the antithesis between Lamarckism and selection theory. Indeed, the threefold belief in vitalism, holism, and the inheritance of acquired characters, combined with active detestation of mathematical analysis, is a syndrome that has by now achieved an almost clinical dignity. In one form or another, vitalists believe that the analysis of biological processes yields a residue beyond the competence of physics and chemistry to explain. A " mechanist," properly so called, is one who does *not* assert this. Methodologically, his position is purely negative ; the " hypothesis of mechanism," as some have called it, does not exist ; and whereas a mechanist of this sort would willingly

[1] *The Science and Philosophy of the Organism.* London.

become a vitalist if he were given better reasons than Hans Driesch could think of for doing so, the reverse reaction could not, *ex hypothesi*, occur. The more truculent mechanist, however, positively asserts that the vitalist hypothesis is wrong, and that biological processes are in the last analysis explicable by the theorems of physics and chemistry. In this way he inflates a pragmatic rule of laboratory conduct into an article of faith.

The year 1900 turns out to be quite a good time to begin an account of modern zoology. Zoologists had become a body of men large enough to form the rudiments of a distinct and well-recognized profession, though still small enough for every zoologist in England to know his colleagues personally. This remained true, perhaps, until the First World War. Since then the profession has increased three or fourfold in size. The annual graduate output of the larger teaching departments has increased from 3–4 in 1920 to 15–20 today. Of these, as many as one-third may become teachers in the secondary schools, since biology has come to be recognized as a major teaching subject for the certificate examinations.

Much of the systematic descriptive zoology of the nineteenth century had been the work of amateurs—Edmund Gosse's father among them—and knowledge of natural history was far more widespread then than now. The microscope was a well-recognized part of the furniture of an upper-middle-class home, and popular works of natural history, perhaps George Henry Lewes's, might be found beside it. By 1900 the amateur had been squeezed out of almost every province of zoology except natural history and simple descriptive anatomy. Phylogenetic speculation required a range of knowledge and an access to materials quite beyond the

amateur's reach. Today the really gifted amateur may still be admitted to the Linnæan Society. Membership of the Zoological Society caters for a less technical interest. The " Zoo " itself was founded in 1829, and though housed without much possibility of local expansion in about thirty-five acres of Regent's Park, and smaller by far than the *Tiergarten* or the Bronx Zoo, it is nevertheless acknowledged to be the finest, zoologically, in the world. Since membership is not regarded as an academic qualification, laymen mostly fail to realize that the Zoological Society is a " learned " society; indeed, the only one in England devoted exclusively to the interests of the profession. Its success has turned upon a skilful and probably unique combination of scholarly activity, profitable showmanship (the Society is not subsidized), and the indulgence of lay interest in animals of all sorts. Most of the credit for this must go to P. Chalmers Mitchell, its secretary from 1903 to 1935. The Aquarium and Mappin Terraces, and, most important of all, the great zoological park at Whipsnade, were built during his tenure of office. Both of this century's notable contributions to the land fauna are to be inspected at the Zoo: the okapi of Central Africa, discovered in 1900 by following up a somewhat tenuous clue left by H. M. Stanley; and that oddly vegetarian carnivore, the Giant Panda.

If one flicks through the pages of the learned journals issued in 1900, one is first struck by the real liberality of zoological thought. The narrow specialization of the journals and their contributors had not yet begun. A man who subscribed to the *Philosophical Transactions of the Royal Society* could in one volume read about the extinct mammals of Madagascar, or follow a comparative study of the effects of ligaturing the cerebral arteries (calling in evidence, among other things, the clinical

consequences of vampyrism); join with Tomes in seeking among fish evidence of the origins of mammalian tooth-enamel; or enlarge his meagre knowledge of the localization of the visual area in the human cerebral cortex. The *Quarterly Journal of Microscopical Science* still admitted papers of an almost wholly physiological nature; it showed only the first rudiments of the austerely anatomical character it was to acquire under E. S. Goodrich's editorship later on. The period 1890–1910 was indeed the Augustan age of comparative anatomy, an age which culminated in the publication of the majestic, if incomplete, *Treatise of Zoology*, under Ray Lankester's editorship. Phylogenetic speculation was authorized by the concept of *homology*, as it had been applied with scrupulous care by anatomists and embryologists like Gegenbaur, Balfour, and Goodrich. In some hands, unfortunately, the construction of phylogenies degenerated into a diverting indoor game. Utterly irresponsible homologies could now serve to assimilate any two groups, even arthropods and vertebrates; and zoologists became suspicious of any far-reaching phylogenetic speculation—even one, like W. Garstang's hypothesis about the relationship between chordate and sea-urchin or starfish larvæ, which, right or wrong, was at least scholarly and responsible.

By 1900 the classification of the Animal Kingdom into its *major* subdivisions was pretty well complete, and Lankester's classification, the one which in outline we now accept and use, did not itself differ very profoundly from Haeckel's, the first of the post-Darwinian schemes. Little need be said of the finer classification into genera and species. Repeated heretical deviations from the Strickland Code of 1845–46 had by 1900 led to hopeless confusion. The first world-wide attempt to clean up the Augean stables and teach its inmates

tidier habits came with the publication, in 1905, of the *International Rules of Zoological Nomenclature*. Today, finer systematic classification has been relegated to the expert, though most entomologists, whatever their work, are still expected to be reasonably competent systematists. A profound analysis of the problems of systematic classification has yet to be written, and many systematists have now resigned themselves to a pure pragmatism, which, though it has relieved them of any burden of analytical thought, could perhaps be said to have lowered the standing of their profession.

Palæontology, appropriately enough, has had a slower and smoother and more coherent development than the other branches of zoology. Like most sciences that rely to a large extent on intuition and personal judgment, it demands a strict continuity of teaching tradition. The leading palæontologists of the present century—among them D. M. S. Watson in England, and H. F. Osborne with the pupils he trained in the American Museum of Natural History—received the torch directly from the great post-Darwinians. Within the last ten years several palæontologists have been drawing heavy drafts on modern genetical and embryological theory, mainly from the genetical theory of natural selection and from the study of differential growth. This new attitude is best illustrated by G. G. Simpson's *Tempo and Mode in Evolution* (1944). But there can be no doubt that palæontology's most *spectacular* achievement was the discovery, in 1939, off South African coastal waters, of a fish, *Latimeria*, belonging to a group which palæontologists had bid farewell to in the Cretaceous—in lay terminology, a living fossil.

The Comparative Physiology Movement

The First World War had no important material influence on zoological research, and though it is true that some zoologists contributed to the theory of camouflage, and that Chalmers Mitchell invented aerial propaganda warfare, the nation made no use of the zoologist's professional qualifications. But the war marked the end of the reign of pure comparative anatomy, and shortly after it there arose the fine English school of comparative physiology under the acknowledged leadership of J. Gray at Cambridge. To the period 1920–35 belongs, for example, H. M. Fox's investigation of the blood pigments of invertebrates, C. F. A. Pantin's work on the behaviour of lower organisms, and L. T. Hogben's use of colour-change mechanisms as an instrument for studying comparative endocrinology. Rather more recently insect physiology has developed, under the leadership of V. B. Wigglesworth, into what is perhaps the largest single specialist field in zoology today. Insect physiologists deserve some credit for trying to set themselves more exacting tolerances in their finished work than is usual in experimental zoology.

A fair proportion of animal physiology is concerned with the study of behaviour in its narrower and more usual sense—a science no longer spoken of as " animal psychology." The methodology of the science of animal behaviour is, in the now accepted technical sense, *behaviouristic—i.e.*, it is founded on the psychological positivism of Ernst Mach. It is concerned with the annotation and causal analysis of the overt and recordable behaviour of animals, not with their passions, hopes, and fears. (His recognition of this fact is what causes us to acknowledge Jacques Loeb as the founder

of the science, no matter what the imperfections of his own narrow and over-simplified scheme of interpretation.) Laymen are quite mistaken if they believe that behaviourism is " dead," though they have been encouraged to think so by the people who not so long ago invited them to celebrate the decline and fall of Darwinism.

The comparative physiology school became no less dominant between the two wars than comparative anatomy had been before the earlier of them. It has at least two quite serious defects. Comparative physiology has given the mammals less than their due, since it has come to be thought that the study of mammals is rather specially the concern of the departments traditionally associated with the medical schools. Second, research and teaching practice have alike tended to depreciate the importance of the two central disciplines of zoology —embryology and genetics. But in spite of these shortcomings the movement has contrived to turn out zoologists of a new and admirable type—men who can say of any physiological problem that it is best studied on this particular animal or that particular cell, without being very deeply concerned with the animal or cell for its own sake. The sperms of ostracods may be just the things for studying the chemical morphology of fibre proteins; squids, so J. Z. Young has shown, reveal the physiological ground-plan of the working of the brain more clearly than rats; the kidneys of desert frogs and sea-living teleosts may give one critical information about the mechanism of excretion; and so on. But it must be made clear that the brain of the squid is studied, not with the ultimate purpose of learning more about squids' brains, nor, for that matter, about the brains of human beings. The ultimate objective is to reveal *the working of the brain.* It

is hard to get laymen to appreciate what is implied by this simple distinction.

Applied Zoology

Materialist historians of science seem to despair of showing that any large part of zoological research is influenced by the social and economic importunities of the times. There are, nevertheless, at least four well-recognized disciplines of applied zoology. Parasitology is as old as the systematic exploitation of the colonial empire, and both teaching tradition and recruitment to its service associate it with zoology rather than with medical science. The life history of the liver fluke, for example, was first worked out by a professional zoologist, and students of the parasitic protozoa have for the most part been zoologists both by inclination and professional training. The importance of economic entomology in England relates principally to agriculture and the storage of its produce. Its growth is almost wholly of the present century, and its further progress will be interesting to watch, since a determined effort has been made by its practitioners to abandon pure empiricism for procedures adequately supported by theoretical research. A high proportion of " pure " research in insect physiology is indeed done in the confident hope that it will in due course be contaminated by some measure of practical usefulness. The science of applied animal genetics, further advanced in America than in England, is, in essence, simply an application of Mendelian theory to livestock breeding. It is a backward science for several reasons, quite apart from the leisurely breeding habits of its subject-matter. Differences between such complex attributes as shape, size, carcase-quality, and milk-yield are governed by the concerted quantitative action of a multitude of heredi-

tary factors: the analysis of such inheritance, though it is Mendelian in principle, requires the use of the special and complex biometrical procedures of which K. Mather has made a special study. Moreover, the animal geneticist is obliged to set the authority of Mendel against a tradition of empirical breeding theory which dates from before the beginning of the recorded history of mankind. Experimental animal breeding is therefore for the most part undertaken on subsidized farms. Its biggest technical innovation, artificial insemination, was introduced in the early 1930s, but still awaits full-scale use.

The branch of applied zoology most nearly in touch with ordinary zoological practice is that concerned with the fisheries. At the turn of the century our leading marine station at Plymouth was manned by a Director, E. J. Allen, and two assistants; Allen had been Director for the six years beforehand, and was to remain so for thirty-six years more. The fact that his salary in 1919 was £300 per annum gives some measure of the importance attached to his work at that time. In due course, however, the station's finances were taken over by the Development Commission, and an era of expansion and new building began. Today, Plymouth is a worthy headquarters of the Marine Biological Association. Marine zoology is not, of course, confined to coastal waters; among its most important achievements has been A. C. Hardy's quantitative and analytical treatment of the ecology of the surface waters of the ocean, largely made possible by the development of continuous and automatic sampling and recording apparatus. (More purely hydrographical work is done under the general supervision of the Conseil Internationale pour l'Exploration de la Mer.) Plymouth yields place to the Stazione Zoologica at Naples (1872)

as the world's foremost marine station. Naples has become a recognized training-ground and busman's holiday-camp for three or four generations of British zoologists; rightly so, if it is true that Anton Dohrn in founding it was inspired by a rudimentary station, subsequently to become the Millport station, already at work in the Firth of Clyde. One of the most important functions of the British marine stations is in running Easter and Summer courses for undergraduate zoologists. The first such course was organized by W. Garstang in 1894.

Genetics and Evolution Theory

Although this article is not to deal with genetics as such, some mention must be made of the influence of genetic theory on current speculation about the mechanism of evolution. It has achieved a silent revolution of which many zoologists are still quite unaware, perhaps because the genetical theory of selection is, of necessity, rather formidably mathematical. But if we peer into the mathematical undergrowth, the principles stand out clearly enough. What does or does not evolve is an animal *population*—not, as family-trees encourage us to believe, an animal lineage or pedigree. The geneticist proposes to treat such a population as a population of pairs of *genes*. Not every gene will be represented in every animal; to each gene, therefore, may be assigned a figure representing the frequency of its occurrence in the population. The mode of Mendelian inheritance is such that if no outside influence is brought to bear upon it, the gene frequencies and so the degree of genetic diversity within the population rapidly achieve and maintain a "statistical" constancy. And since G. H. Hardy's famous theorem of 1908 enables one to translate statements

about gene frequencies in a population into statements about the proportions of individual genotypes to be expected in it, the apparently wilful abstraction by which the geneticist chooses to think of populations of genes rather than of animals can be dissolved at will.

In populations of finite and actual size, gene frequencies do not, of course, remain constant. Mutations, for example, being recurrent phenomena, will introduce a slight systematic bias towards increasing the frequency of a mutant gene in the population. But the factor of overriding importance is natural selection —that impressed " force " which causes one certain fraction of the population to make a disproportionately large contribution to the ancestry of future generations. The animals bearing that combination of genes which does, in fact, increase in frequency are said to be " fitter " than those which do not, and the genes themselves, in that combination, may be said to confer that fitness upon them. It is clear, then, that in speaking of the " survival of the fittest " we are being wise, if we are being wise, after the event.

Evolutionary change is the outcome of a *systematic* change of gene frequency. The virtual irreversibility of evolutionary change comes with the fixation of some particular constellation or spectrum of gene frequencies by a variety of methods that in one way or another achieve the sexual isolation of the changed population. Speciation so achieved is thus the unit act in evolutionary change, as the systematist regards it.

The theory of the analysis of gene frequencies is the work of J. B. S. Haldane, R. A. Fisher, and, in America, S. Wright. The same principles have been applied to competition between (rather than within) species by A. J. Lotka and V. Volterra.

One of the most notable achievements of modern

genetics has been to provide one possible answer to a major conundrum of Darwinian theory. Mutation is a very rare phenomenon, and mutant genes are for the most part deleterious and fully recessive in expression. How is this to be reconciled with the fact that, to put it crudely, mutation provides the " raw material " for evolutionary change ; more exactly, that it is the source of all new variation that is not merely recombinational in origin—*i.e.*, due to Mendelian segregation ? Modern speculation has suggested an acceptable and ingenious answer. It is argued that the common recessive laboratory mutants are just those which have been tried by natural selection and found wanting ; that they have *evolved* towards complete recessiveness of expression. Fisher suggests that on their first appearance mutant genes are not fully recessive. On the contrary, they have some outward expression in heterozygous state, and may thereafter evolve towards dominance or recessiveness as selection may dictate. Any rare gene will occur enormously more frequently in heterozygous than in homozygous combination, and if the mutant is phenotypically recognizable as a heterozygote, then the argument from the rarity of mutant genes loses most of its force. At all events, both Fisher and E. B. Ford have conclusively shown that genes having some expression in the heterozygous state may be caused to evolve towards complete dominance of expression ; and in nature it is presumed that mutant genes so canonized become ordinary ingredients of the " wild " genotype.

It must not be inferred from this bald account that Lamarckism is dead. At regular intervals throughout the present century experiments of faulty or even fraudulent design and execution have been offered as evidence of the inheritance of adaptive somatic modifications and not accepted. This has had the unfortunate

effect of denying a fair hearing to quite a number of conundrums of heredity which, if fairly presented to an orthodox selectionist, are apt to cause a certain shifty-eyedness and shuffling of the feet. Stock examples of such conundrums are: the inheritance of the thickening of the epithelium on the sole of the foot, of the "squatting" facets of Orientals, or of the reversal of hair tracts in certain marsupials. The property they all share in common is that such modifications are, or could be, brought about in an individual's own lifetime merely by habit of use. To adopt a now accepted but none the less odious neologism, they deal with inherited differences of a type of which it is particularly easy to make "phenocopies." A completely satisfying Darwinian solution of these conundrums is still to seek.

Statistics

It is characteristic of modern quantitative biological research that its authors now publish their results with a numerical estimate of the degree of confidence that may be attached to them. This has been made possible by the specially rapid development during this century of mathematical statistics. The layman, of course, still thinks of statistics as the presentation of data in the form of rows or columns of figures, and he can still be heard to affirm, without any apparent awareness of inconsistency, that statistics can prove anything and that statistics prove nothing.

Most statistical tests involve a comparison either between statistics computed from two or more sets of actual measurements, or between statistics computed from a particular set of data and those predicted by the terms of a preconceived hypothesis. The measure of plausibility that we adopt is very often in the form of an answer to the question: To what extent can disparities

so great as those I have recorded be attributed to the luck of random sampling alone? Men vary in height; we cannot measure them all; so we measure a sample which, though chosen at random, cannot be expected to be fully representative of the population as a whole. The mean of this sample is only an *estimate* of the mean of the population. We are therefore obliged to attach to our estimate a *sampling error*, itself an estimate of the limits within which a certain fraction of our estimates of the mean would be expected to lie, supposing that we had taken repeated samples from the same population and had computed the mean height from each one. If we now wished to compare the mean height of adult Englishmen with that of adult Patagonian Indians we should be able to decide to what extent the difference we found was merely the consequence of the vagaries of random sampling.

R. A. Fisher has done the lion's share of the work of devising the great variety of statistical methods required by biologists—chiefly those relating to the small samples which biologists are so often obliged to deal with; and he has made them available to biologists in his *Statistical Methods for Research Workers*, which has passed through a dozen or so editions since its first publication in 1925. This is work of the most profound importance: no single property more clearly distinguishes the biological work of 1950 from that of 1930 than the biologists' insurance against the hazards of his sampling errors. Genetics and agronomy, the sciences for which biological statistics was first devised, remain those in which statistical techniques have achieved their greatest refinement.

Growth and Development

The year 1900 marks no natural division in the history of embryology. The new century inherited from von Baer the germ-layer concept and from Haeckel the Law of Recapitulation—two great connecting principles that were to be highly qualified in the course of time. In the first ten years of the century the descriptive embryology of vertebrates, particularly of marsupials and higher mammals, was enlarged by the work of A. Brachet, J. P. Hill, J. W. Jenkinson, E. G. Conklin, and many others, while the causal analysis of development, "experimental embryology," pursued a quite independent career. (The, at the time, sensational researches of Jacques Loeb into the phenomenon of artificial fertilization belong to this period.) To the great German school of experimental embryology of the years 1900 to 1924 we are indebted for many of its basic theorems: of the genetic equality of the early nuclear divisions of the embryo, for example, and the first rudiment of the "organizer" concept, born of researches into the relationship between the eye-cup and the lens.

It had been known for some time that a certain region of the amphibian embryo (that which invaginates through the blastopore to form the primitive gut roof) is capable of giving rise to a complete little secondary embryo if removed from its normal position and grafted to a sufficiently young embryo elsewhere. It had been supposed, naturally enough, that the secondary embryo was formed of the tissues of the graft. By using donor and host distinguishable by the degree of pigmentation of their tissues, H. Spemann and H. Mangold showed in 1924 that the new little embryo was formed in part of the tissues of the *host*. The graft had forced the host

tissues neighbouring it into a pathway of development quite other than that which they would normally have followed: it acted as an " organizer " of differentiation. The organizer came to be regarded as a key-concept of causal morphology. In the early 1930s it came to be known (thanks in part to the work of the new-born Cambridge school of embryologists) that some of the consequences of organizer action could result from the use of dead grafts or even purified chemical compounds —notably chemicals of the sterol family, already known from the work of E. A. Kennaway and J. W. Cook to be among the active cancer-producing ingredients of coal tar. Entelechy being thus rudely dethroned (or shown to be ether-soluble), it seemed as if embryology were to be the instrument of a grand biological synthesis. At present, however, with the disbanding of the German and Cambridge schools of embryologists, there is some dissatisfaction with the high claims made for the universality and explanatory value of the organizer concept; an extreme view, mainly championed in America and probably too extreme, is that organizers provide only non-specific " trigger " stimuli (akin to those responsible for artificial fertilization) and that the occurrence of organizers is of secondary, adaptive significance.

In 1929 W. Vogt published the first *fate map* of a vertebrate embryo.[1] By staining small areas of the amphibian blastula with harmless dyes and tracing the subsequent fate of the dyed cells, he was able to prepare a map of the blastula showing which tissue systems its parts would ultimately go to form. The " presumptive neural area," for example, is that part of the

[1] By the obliging help of certain natural differences of colour and texture, something like a fate map has already been worked out for the embryos of sea-squirts.

blastula whose cells (or their descendants by fission) will in due course form the central nervous system of the gastrula embryo. The presumptive neural area has no substantive neural differentiation as such; it is presumptive neural area in much the same way as a schoolboy may be a presumptive tinker or tailor. The kinematic analysis of development by Vogt's methods made it clear for the first time that cell movements and the convolutions and stretchings of cell sheets are the principal instruments of embryo formation. The organization of the embryo into the system of epithelial sheets called " germ layers " thus became mechanically intelligible, and the germ layers are now thought to have a tactical rather than a strategical significance in embryo-formation.

The thoroughgoing extension of Vogt's work was undertaken by the Belgian school of embryologists, led by A. Dalcq and J. Pasteels, and now unquestionably the greatest in the world. Its labours have refounded the whole of comparative embryology on the comparison of fate maps and of the patterns of the morphogenetic movements.

The study of differential growth—those inequalities of growth rate between the several parts of the body that cause it to change in shape during its development —was not rendered of general significance until the work of D'Arcy Thompson (*On Growth and Form*) in 1915 and the researches which J. S. Huxley first collated in 1932 (*Problems of Relative Growth*). Thompson showed that the overall shape of one animal could often be treated as a single geometric function of the shape of another, however much the two might differ in particular little ways, and later developments inspired by it have made it possible to measure the rate of change of shape in development. Huxley formulated the useful and

widely applicable general rule that the specific growth rates of the several organs of the body often stand in constant ratios to each other, and he devised the correct and now accepted mathematical framework for demonstrating the phenomena of differential growth.

Repeated efforts have been made to devise theoretical formulæ to describe the overall growth of organisms in time, irrespective of differential growth in their parts. Some of these attempts have the arbitrary grandeur so often associated with research work conducted from the arm-chair, but there are grave theoretical reasons for supposing all of them to be at fault.

Zoology and Immunology

The existence of specific differences between the red blood cells (or sera) of mammals, revealed by the reactions of agglutination and lysis, had already been made clear in the latter half of the nineteenth century by experiments on blood transfusion. In a footnote to a paper published in 1900 Karl Landsteiner, inquiring into the possibility that serum from sick patients might agglutinate the red cells of the healthy, reported that the sera of a certain proportion of normal and healthy men would violently agglutinate red cells from other normal men. From this first observation of what we now call the A–B–O agglutination system arose the whole structure of modern blood-immunity research. It has proved to be of high importance to zoologists—in the study of human inheritance, of the manner of action of genes, and of the evolutionary problems of genetic polymorphism. Many new agglutination systems are now known, and Landsteiner's name is associated with the discovery of most of them. So rich is their combinatorial variety and so fine-grained the polymorphism they reveal that it cannot be long before

every individual human being may be given a unique serological label or identification card based upon blood-agglutination properties alone.

It is the great virtue and strength of immunological methods—the most delicate and discriminating of any that are known—that they may be used to anatomize animal relationships as broadly or as finely as we choose. As Nuttall first showed (*Blood Immunity and Blood Relationship*, 1904), phyla, classes, orders, and genera may be distinguished from each other and ranked in their affinities by serological methods in addition to those at the disposal of the ordinary zoologist. Even in its very precise refinements by A. Boyden, who has organized a " Serological Museum " at Rutgers University, much of this work has served only to reinforce conclusions independently arrived at by comparative anatomists; but where the anatomist has failed, as in determining the affinities of whales, the serologist has been able to provide a pretty well decisive answer. The scale of affinities down to differences between individuals may be completed by blood-agglutination and tissue-transplantation methods; skin transplantation, for example, may be used to distinguish one individual from any other save its identical twin. Serology thus joins with philosophy and common sense in a concordant trio of opinion on the uniqueness of individual mice and men.

Zoology and Cancer Research

A tradition as old as cancer research itself has it that the cancer problem is in some vague but important way a biological one, and the present century has witnessed a fitful and prolonged flirtation between zoologists and cancer-research workers. Theories of the origin of tumours are indeed quite sharply divisible into the

purely pathological (that tumours are formed in consequence of chronic irritation, or by the action of viruses of extrinsic origin) and the mainly zoological (that tumours develop from embryonic cells persisting by accident into adult life, or as a result of faulty embryogenesis or the mutation of somatic body cells, and so on). Special mention should be made here of C. D. Darlington's (1944) astonishingly radical theory of the relationship between heredity, development, and infection. Right or wrong, it is the widest and most spectacular synthesis that has been presented to biologists since the days of Darwin.

" Microbiology " has been midwife to much speculation on cancer etiology in the last ten years : it is our youngest discipline. Bacteriology, protozoology, virus research, and mycology are given their unity and coherence by the organisms that happen to form their subject-matter. Microbiology, like genetics, is knit together by the nature of the problems it studies. The role of the cytoplasm in heredity, the relationship between cytoplasm and nucleus, the nature of genetic systems, and the mutations and adaptive activities of individual cells are among the problems for which microbiologists are now coming to make themselves responsible, since it happens that all of these problems are most easily accessible to analysis in single-celled or non-cellular organisms.

Cellular Physiology and Biophysics

Modern cellular physiology and biophysics may be said to have begun with the work of W. Hardy and E. Fischer at the turn of the century. Earlier speculations on the structure of protoplasm are now of only antiquarian interest. In the early years of the century, biophysics made good use of such physical theory as

was then available to it—on osmosis, for example, and hydrostatics in general—and much of this work is summarized in D'Arcy Thompson's *On Growth and Form* (1915). But biophysics of the molecular order could only develop step by step with modern colloid and capillary chemistry, which may perhaps be said to have started with the recognition, by I. Langmuir and others, of the surprisingly literal geometric meaning that could be given to the structural formulæ of the larger organic compounds. Today it is commonplace to hear speak of flattish molecules or long, thin ones, though the pioneers had to put up with a certain amount of genteel persecution for so describing them. Two further contributions, from more orthodox physical chemistry, have now passed into the biologist's ordinary technical armoury: the potentiometric determination of true acidities associated with the name of Sorensen and the pH notation, and, more recently, the measurement of oxidation–reduction potentials by methods similar in principle.

With such equipment as this a promising beginning has been made with the analysis of amœboid movement, differential permeability, the viscosity and surface tension of "protoplasm," and so on. J. Gray's *Experimental Cytology* (1928) may be said to have given the science of biophysics its first definition. Biophysics is still very much at the mercy of its technical resources. The electron microscope, in 1939 still used only by a handful of workers, is now widely available and is well beyond the stage of being thought of as an agreeable but costly toy. Zernicke's phase-contrast microscope, revealing phase differences as well as differences of absorptive power in transmitted light, has also become generally available within the last three years, but C. Burch's ingenious mirror-lens reflecting microscope,

purging the conventional instrument of chromatic aberration and enjoying a particularly long working distance between objective lens and object, has not yet become available on a commercial scale.

Perhaps the most spectacular and theoretically important contribution to biophysics has come from the bold application of crystallographic techniques to " biological " molecules, work associated with the names of W. T. Astbury and J. D. Bernal. It is now, for example, possible to think of muscular contraction, and contractility in general, in molecular terms.

Biophysicists can be called " cytologists " in the sense that they study cells and the ingredients of cells ; but by an unfortunate caprice of zoological history people who call themselves " cytologists " are divisible into three sharply distinct species. First, because longest established, are the cytologists who pursue orthodox microscopical technique, using microtome and staining-bottle and a number of more or less trustworthy microchemical reagents. Their purpose is to reveal the fine morphology of the cell. Favourite objects of study are the Golgi Apparatus, which has now successfully lived down the odium of being thought not to exist at all, and the mitochondria, which atone for their conspicuousness by being almost wholly obscure in function. Second are the nuclear cytologists, who unravel and interpret the activities of chromosomes with the impressive authority of Mendelian genetics behind them. It is their real virtue that they have not waited for orthodox physical theory to authorize their speculations—on the existence of long-range intermolecular forces, for example—but have boldly asserted where the more timid have but ventured to suggest. Unfortunately, they have woven into their subject matter an empirical (but, to be fair, contextually necessary)

terminology and structure of hypothesis that has to a large extent closed their sub-science to the layman zoologist. Third are the pure biophysicists, at present all too often crippled by a lack of biological training or a proper grasp of the problems which they are technically so well equipped to investigate. It is not yet possible to report on any *rapprochement* between the members of these three groups.

Tissue culture may for convenience be classified with cytology. In 1907 R. G. Harrison for the first time showed that adult vertebrate cells other than leucocytes would survive if cultured aseptically outside the body, and his observations were developed under the influence of A. Carrel in America, T. Strangeways in England, and A. Fischer in Denmark. It has at least one profound theoretical achievement to its credit: the famous " Rockefeller " strain of chick-heart fibroblasts, cultivated from 1912 until 1939, showed (as other strains confirmed) that the lineages of at least some somatic body tissues are indeterminate—to put it crudely, that such cells are potentially " immortal." It naturally took many years to demonstrate this beyond reasonable doubt; in 1915, when the first talk of immortality began to be heard, Harrison himself pointed out that the cultivated cells of the Rockefeller strain had not yet lived beyond the span of life that would have been enjoyed by the chicken from which they were originally taken. In due course, as we now know, they did.

Knowledge that the cells of tumours are " immortal" in just the same sense had come earlier. Here the demonstration turned upon the repeated propagation of tumour grafts from one animal to another, and the tumour strains started by Ehrlich and Jensen in the first ten years of the century are still among the most popular for experimental work today.

The layman might well expect the problem of senescence to stand high on the agenda of the major biological research schools. Unfortunately it does not. Although repeated attempts have been made during the present century to interpret the phenomena of ageing,[1] no one has yet improved upon August Weismann's guess that senescence is in some manner the outcome of the action of natural selection. Weismann's own more specific interpretation is unacceptable. Ageing, then, remains a problem for the biology of the future.

Prognosis is no part of a historian's job, and the zoologist who undertakes to play Mother Shipton on behalf of his science cannot expect to do much more than irritate his colleagues. Of course, we may be quite certain of one thing—that specialization and subdivision within zoology, however far they have gone already, will go still farther. But, though no man can *master* the whole of biological science, he can still come to terms with most of it, and zoology will remain a unified science for just so long as there are people who can do so. In *this* respect we are steadily improving; there are more zoologists today than there were thirty years ago who make it their conscious business to achieve some sort of synoptic grasp of all the departments of their science. We are therefore in a stronger position than ever before to advance biological science. We need not fear that physics and chemistry can ever dispossess biology of its subject-matter, nor, indeed, suppose that they have ever in any degree done so. The problems of biology centre round biological concepts —heredity, development, and infection; reflex action,

[1] Cf. C. Minot, *The Problem of Age, Growth, and Death* (1908); E. Metchnikoff, *The Prolongation of Life* (1910); C. M. Child, *Senescence and Rejuvenescence* (1915); R. Pearl, *The Biology of Death* (1922); scientific papers by G. P. Bidder (1932); and many others.

instinct, and learning ; adaptive activity and self-maintenance ; organic growth and transformation. There is nothing " empirical " about these concepts, as some think who fidget for their translation into the language of physics and chemistry ; nor anything " fundamental " about them, as others insist who fear that physical analysis is going to do them an irreparable mischief. They are simply the *contextual* concepts of that rank of the natural order that happens to form the subject-matter of biology. The hope or fear that biology is soon to become a sub-department of the orthodox physical sciences is thus simply the outcome of confused thinking.

GENETICS

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GENETICS is a new science. Its origin and growth, from small beginnings to vast proportions, and its recognition as a great power in human affairs, falls wholly within the last fifty years. It comprises the study both of heredity and variation, and though this involves problems which have long attracted attention, our approach to them has been so changed by the events of the year 1900 as to receive altogether a fresh orientation. The whole concept of evolution—the central one of biology—was without genetics quite imperfect, and the Darwin–Wallace theory of natural selection had to await in suspended animation the development of that subject. Its influence on agriculture is no less profound, and its bearings upon sociology and medicine are just beginning to be appreciated. Institutes for genetic research have sprung up in numbers throughout the civilized countries of the world, while Chairs of Genetics are already founded in the majority of those universities which actively support the biological sciences.

So great a contribution to human knowledge had its origin in the vision of one man, Father Gregor Mendel (1822–84), a member and finally Pralat (Abbot) of the Augustinian Monastery of St. Thomas at Brno, in Moravia. It now seems possible that he reached the central concepts of genetics by theoretical deduction and that he then undertook a series of experiments on the edible pea calculated to display their working. At any rate, whatever mental steps led Mendel to these experiments, he designed them with singular foresight and performed them with skill. His theoretical analy-

sis and practical results were communicated to the Brunn (Brno) Natural History Society in 1865. They were published in its *Proceedings* the following year and, so far as their immediate influence upon scientific thought were concerned, they were a total failure. It has often been said that this was due to their publication in a little-known journal. That is not correct. Though certainly obscure, it was by no means unread: its circulation was wide (copies were available in England), and several of the leading botanists of the world were soon acquainted with Mendel's results, including Nägeli, with whom he corresponded. But men's minds were not ready for them; they were ignored and then forgotten. Only in 1900, sixteen years after Mendel's death, was his paper read almost simultaneously by Correns, Tschermack, and de Vries—three men capable of appreciating its significance. The facts were then promptly given to the world afresh, and that date marks the birth of modern genetics.

It is not my intention to treat this subject historically, and we can speak of Mendelian heredity, in the sense that we speak of Newtonian physics, so as to include hereditary concepts founded upon the views of Mendel without suggesting that he personally was responsible for all of them. He was, indeed, responsible for very few. What he stood for, and has been made to stand for by those who followed him, can conveniently first be summarized in general terms. The subject can then be amplified and explained, and its bearing upon human affairs briefly considered.

The essential features of genetics are these: the qualities of all living things, plants and animals, including their variation, are partly inherited and partly due to the environment in which they live. These two influences are of approximately equal importance, at

least in promoting diversity, and we may consider the hereditary one first.

It must be allowed that some sort of units responsible in some kind of way for the characteristics of the individual are transmitted from parent to offspring if heredity be a fact. Now, its existence can be established by a mathematical test (that of correlation) which is independent of any theories respecting its nature. This test is capable, moreover, of assessing the hereditary contribution made by males and females, which proves to be approximately equal.

Here we are faced with two alternatives: either the hereditary units derived from the two parents contaminate one another or " blend " in the offspring, in which they are brought together into the same individual, or they remain permanently distinct. Mendel showed that the latter alternative is the correct one, and this is now known to be a universal generalization. Its truth can be ascertained by the study of variation. If the hereditary units, or *genes* as they are called, contaminate one another, the amount of variability would be reduced on inbreeding in a constant environment, for blending leads to uniformity. But quite the contrary effect is found. When the offspring of a given mating are interbred, the grandchildren are actually more variable than the children. Consequently, the hereditary units must retain their identity; more than that, they must be subject to a reshuffling process capable of producing new combinations, so leading to diversity.

Evidently the fact that the genes do not contaminate one another does not of itself ensure their permanence; they might still be liable to frequent intrinsic change—that is to say, they might be unstable substances in the chemical sense. But that possibility is not realized. A change in the structure of any unit of heredity is known

as a *mutation*, and it proves to be a very rare event. On the average, a given gene mutates in perhaps one individual in several million. Now, the rarity of this occurrence is one of the essential facts of heredity, and for this reason. The Mendelian system allows a vast store of potential heritable variability to accumulate, so that genes which arose far apart in time or in space can be held in reserve and tested repeatedly in fresh environments, thus giving them a chance to meet one in which they prove a success. It might be argued that this result would be as well attained if the genes were constantly mutating, and that is so ; but once an organism has its genes in correct adjustment it would be fatal if they had not permanence. Mendelian heredity has this strange, transcendent quality : that it allows of great variation, which can yet be stabilized when required. Stability is achieved when needed both because the genes do not blend with one another, and because of their low mutation rate, two of the fundamental features of the situation. The other side of the picture, the way in which it provides extreme diversity, remains to be considered.

Since the two parents contribute in approximate equality to the heredity of their offspring, every feature of the organism must, at the simplest, be due to two hereditary units (genes). These genes therefore are present in pairs (allelomorphs), whose members are derived respectively from the father and the mother. In making this statement, we are looking back to the previous generation ; but what of the future ? If the offspring are to make a similar claim, it is evident that at some stage in the development of the reproductive cells the members of the allelomorphs must separate so that one only is transmitted by each parent ; the allelomorphic pairs are then restored by the additive nature

of fertilization. This separation from one another of the pairs of genes constituting the allelomorphs is termed *segregation.*

It is now possible to deduce the basic action of heredity and to illustrate it by means of an example; for this purpose two common flower colours, red and ivory, in the well-known bedding-plant, *Antirrhinum*, may be used. The gene controlling this colour-difference may be represented as *C*, and it can exist in either of two phases: C^r, responsible for the red, and C^i, for the ivory, shade. A red-flowered plant has, as explained, a *pair* of its appropriate genes, C^rC^r, whose members, forming a pair of allelomorphs, are derived respectively from its two parents; so, too, has an ivory-flowered one, which possesses C^iC^i. Owing to segregation, the genes of each pair separate from one another during the formation of the pollen grains and ovules, so that every reproductive cell is equipped with only one of them.

Consequently, if a red-flowered and an ivory-flowered plant be crossed, the seedlings receive *C* in its different phases from their two parents, so that in them the allelomorphic pair is restored in the form C^rC^i. As might be expected, such plants have flowers of an intermediate (pink) shade.

The information provided by the next (inbred) generation is critical. Owing to segregation, the reproductive cells of such pink-flowered antirrhinums receive one member only of each allelomorphic pair possessed by the other cells of the plants which bear them. Their reproductive cells therefore receive, in approximate equality, either C^r or C^i from the pair C^rC^i. Consequently, when two of the pink-flowered type are crossed, the chances are equal that C^r brought in with the pollen is combined with C^r or C^i ovules, thus restoring the allelomorphs as C^rC^r or

C^rC^i. So, too, with the equally numerous pollen grains carrying C^i, which also combine with C^r or C^i ovules to produce C^rC^i and $C^i\,C^i$ progeny in equality. The combinations so produced can be summarized as:

$$\begin{array}{ccc} C^rC^r, & C^rC^i & \\ & C^rC^i, & C^iC^i \\ \hline 1 & 2 & 1 \end{array}$$

That is to say, three types of offspring, red (C^rC^r), pink (C^rC^i), and ivory (C^iC^i) flowered, result from crossing two pink-flowered plants, and in a proportion of 1 : 2 : 1.

This ratio is the one inevitably produced when two objects having corresponding alternative forms are combined at random. If we toss together two dice 100 times, they will fall approximately both evens 25 times, one even and one odd 50 times, and both odd 25 times: a ratio of 1 : 2 : 1, being the same as that arising in genetics, and for a similar reason.

These results are worth considering in the light of the generalizations which preceded them. Plants of different flower colours (red and ivory) were crossed and gave rise to a uniform progeny, with pink flowers. That is to say, the observable quality, flower colour, had indeed blended; but evidently its hereditary determinants had not, for in the next generation (the inbred grandchildren) not only do pinks reappear but reds and ivories also, and in definite proportions. Thus the second (inbred) generation of the cross is more variable than the first. Moreover, the " extracted " reds or the " extracted " ivories so obtained can be bred together to re-found red or ivory strains, which breed permanently pure in spite of their origin from a cross-bred mixture. Evidently the genes had not contaminated

one another when brought together into the same individual. The properties of Mendelian inheritance are here clearly displayed, and indicate its capacity for producing variation.

When the members of an allelomorphic pair are similar (C^rC^r or C^wC^w in this instance) they are said to be *homozygous*; when dissimilar (as in the pink C^rC^w plants), *heterozygous*. From what has been said, it will be realized that in general a mating between two heterozygotes must lead to segregation in a ratio of 1 : 2 : 1. Any one who has grasped this simple system so far can, without further information, predict the result of mating a heterozygote with a homozygote—a pink- with a red- (or an ivory-) flowered antirrhinum. As before, the heterozygotes C^rC^w form two types of reproductive cell, with C^r and C^w in equality, while the homozygotes (either C^rC^r or C^wC^w) produce only one, all carrying C^r or C^w. Consequently, two types of progeny must be produced in equality: C^rC^w and C^rC^r, pink and red, if the cross be between a pink and a red-flowered plant. Evidently this result also is an example of a wide generalization—that a mating between a heterozygote and a homozygote gives rise to segregation in a ratio of 1 : 1.

In this instance, the heterozygotes are intermediate between the two homozygous forms, but more usually they are identical in appearance (though not, of course, in hereditary behaviour) with one of them. If an ordinary brown rabbit be mated with an albino, the offspring are indistinguishable from one of their parents, being of the normal brown form; but when interbred they produce both browns and whites in a ratio of 3 : 1. The " extracted " whites so recovered are all homozygotes, but the " extracted " browns are not, for we can show by breeding that one-third of

them are homozygotes, and two-thirds are heterozygotes. Thus the genetic types (homozygotes and heterozygotes) are, as expected, distributed in a 1 : 2 : 1 ratio; but since the homozygous and heterozygous browns are identical in appearance, the middle and one of the two end terms are combined when the animals are judged by their coat colours. We describe the characteristic which is equally developed in the homozygotes and heterozygotes as *dominant*, and the one manifest only in the homozygous phase as *recessive*. Evidently the 3 : 1 ratio is merely the fundamental one of 1 : 2 : 1 obscured by dominance.

During the present century, the mechanism responsible for segregation has been studied in great detail. The living substance of the body is divided into working units—the *cells*—which are generally of microscopic size. The fusion of two of them, specialized for reproduction and derived respectively from a male and a female, originates a new individual. Evidently the cells must contain the hereditary material, and by 1900, though not in Mendel's day, enough was known of their structure to determine where it is situated within them.

Every normal cell consists of two parts: a relatively small *nucleus*, which controls its activity, and the rest of the cell substance, known as *cytoplasm*. Within the nucleus are the *chromosomes*, rod-like or thread-like structures whose number is generally constant in all members of a species, though it may vary from one species to another. The chromosomes carry the great majority of the genes, a fact fully established by experimental means, but really evident from their behaviour, which parallels that of the genes to an extraordinary degree. For they are present in pairs (*homologous chromosomes*), the members of which are

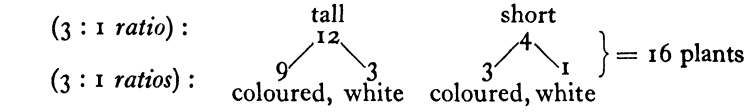
derived respectively from the two parents. The chromosome number is reduced by half during the formation of the reproductive cells, for these each receive one member only of every homologous pair. The pairs are restored again at fertilization, which consists in the union of two such (halved) cells. Though the chromosomes cannot be rendered visible for microscopic examination except when the cells are dividing or undergoing the changes which lead to division, there is clear evidence that they are invariably present in some form.

It will be noticed that the distribution of the chromosomes as seen under the microscope, and of the genes as revealed by breeding tests, is in complete harmony. It is inescapable that the pairs of genes (allelomorphs) are born on homologous pairs of chromosomes, and that the segregation or separation of the members of each pair of genes is achieved by the separation from one another of the members of each homologous pair of chromosomes during the formation of the reproductive cells.

The remarkable parallel between the chromosomes and genes does not, however, extend to their number. There can be no doubt that most higher organisms possess at least several thousand pairs of genes, while they are equipped with a few dozen pairs of chromosomes at most. The result to be anticipated when we study the distribution of two or more pairs of allelomorphs in the same individual will be affected by this fact.

So long as the allelomorphs are carried in different pairs of chromosomes, the distribution of one pair of genes is independent of any other. Thus, in the sweet pea, tall growth is dominant to short, while coloured flowers are dominant to white. Both these contrasted

qualities segregate in a ratio 3 : 1 when plants heterozygous for each of them (and therefore tall with coloured flowers) are crossed. That is to say, both the talls and shorts (themselves distributed in the proportion of 3 : 1) are, on the average, subdivided into three coloured to one white-flowered plant:



The rarest class, being the double recessives (which are short and white-flowered), occupies $\frac{1}{4} \times \frac{1}{4}$ of the whole, so that a minimum number of 16 specimens is required to display this ratio accurately. Evidently we have here the simple mathematical combination of two 3 : 1 ratios, which, when distributed at random, produces a ratio of 9 : 3 : 3 : 1. Three pairs of allelomorphs, of course, segregate in a combination of three 3 : 1 ratios. That is to say, in a ratio of $(3 : 1)^3 =$ 27 : 9 : 9 : 9 : 3 : 3 : 3 : 1.

Similarly, when a double heterozygote is crossed with a double recessive (necessarily a homozygote), each pair of allelomorphs segregates, as already explained, in a ratio of 1 : 1, so producing *four classes in equality*. Consequently, when a doubly heterozygous tall pea with coloured flowers is crossed with a short and white-flowered specimen, talls and shorts are produced in equality, while both these classes contain equal numbers of coloured and white-flowered plants.

These results depend upon the fact that the pairs of allelomorphs are carried in different pairs of chromosomes. On the other hand, those carried in the same pair fail to assort independently because they travel in the same vehicle and therefore tend to reach the same destination; an effect known as *linkage*, which results

in a departure from the 9 : 3 : 3 : 1 and 1 : 1 : 1 : 1 ratios expected on the assumption of independent segregation.

Lists can therefore be made of those genes which are linked with one another, and it is found that the number of groups into which they fall equals the number of chromosome-pairs when the cells of the plant or animal in question are examined under the microscope. It is an impressive fact that these two results are in harmony, though obtained by such different means.

It must not be supposed, however, that all the genes carried on a chromosome are irrevocably associated with it. On the contrary, during the formation of the reproductive cells, lengths of material are interchanged between the members of each pair of homologous chromosomes. At the moment when this *crossing-over* occurs, the chromosomes have already split longitudinally in preparation for the next cell division.

An analysis of this process has provided an extraordinary amount of information on the mechanism of heredity. It has demonstrated that the genes are carried in a row along the chromosomes, and has made it possible to determine not only their order but their relative distances apart. For the number of interchanges taking place is small, and the chance that one shall occur between genes widely separated is greater than between those in close proximity. Such results enable us to construct maps showing the arrangement of the genes along the chromosomes. These are based upon breeding experiments, but independent evidence of their correctness has many times been obtained. For when a piece of a chromosome accidentally breaks away, or is broken by exposure to X-rays, the resulting damage, visible under the microscope, leads to the loss of a group of genes whose members prove to be

the very same that have been placed together on the chromosome maps.

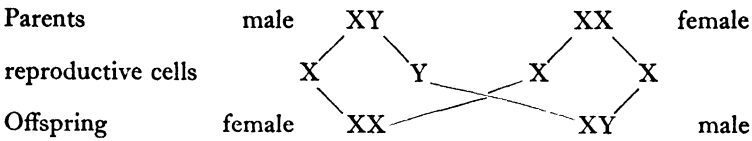
Having briefly considered the way in which the hereditary units are carried and distributed, it will be possible to discuss their working. We shall then be in a position to consider the place of genetics in biology and its impact on human affairs. These subjects can conveniently be introduced by describing a special modification of the chromosome mechanism—that responsible for the control of sex.

This is a problem in heredity, for one inherits the sex either of one's father or one's mother. Also males and females *segregate* in those species in which they exist separately, whether animal or plant, appearing in definite proportions, which usually, though not always, approach equality. The parallel with the situations already described, in particular with that producing a 1 : 1 ratio, is obvious and striking.

In order to achieve this result, a special chromosome mechanism has been built up. For one chromosome-pair out of the whole set, which in man amounts to twenty-four pairs in all, has been dedicated to the control of sex. In one of the two sexes, the female in mammals and most, but not all, other creatures, is a pair of chromosomes (the *X-chromosomes*), the greater part of which contains, among other genes, those decisive in the production of sex. The male of such forms possesses a single X-chromosome, whose partner (the *Y-chromosome*) is of an exceptional kind, being largely inert and usually destitute of the sex-determining genes. Upon the distribution of the X- and Y-chromosomes (the *sex-chromosomes*) the production of a male, with XY, or a female, with XX, depends.

The members of the homologous pair of sex-chromosomes separate from one another in the usual way

during the formation of the reproductive cells, which therefore receive an X-chromosome in the female and, in equal numbers, X or Y-chromosomes in the male. The chances, then, are approximately equal that to the necessarily X-carrying reproductive cells of the female is added another X or else a Y at fertilization, so reproducing a female or a male:



The diagram illustrating this mechanism, though so simple, is yet worth consideration, for it shows that the control of sex is quantitative and depends upon the number of X-chromosomes, not upon their kind—that is to say, they are not of male- or female-determining types. It has already been pointed out that the Y-chromosome is in relation to sex-control usually a dummy. Indeed, in some animals it is known that the accidental presence of extra Y-chromosomes has no effect upon sex, while such a multiplication of X-chromosomes has a profound influence upon it. Thus an individual develops as a male merely because it possesses a single X-chromosome in each cell. Yet an inspection of the figure will show that he necessarily receives this from his mother; while of the decisive two X-chromosomes of the female one has come from her father, in whom it produced maleness. That is to say, the X-chromosomes are constantly shuffled across from one sex to the other.

Sex is, in fact, determined by a balance between predominantly female-determining genes in the X-chromosomes and predominantly male-determining ones in the rest of the chromosomes. Two doses of the

female determinants over-ride the action of the fixed quantity of male determinants, while one dose does not. Thus, a number of genes co-operate to determine sex.

Moreover, it will be evident that the behaviour of those genes unconnected with the development of sex that chance to be carried in the X-chromosomes must be peculiar. Normally it is immaterial which member of a cross is male and which female ; not so if the gene concerned is carried in the X-chromosome, and therefore *linked* with sex.

Red-green colour-blindness, a simple recessive, is of this kind. A colour-blind man cannot transmit his defect to his sons, for, as shown in the last diagram, they receive only X-chromosome from their mother. He can, however, hand it on through his daughters. They will not develop it themselves, since, being recessive, two doses of the gene would be required for them to do so. Yet such women will be " carriers." Half their sons will receive the particular X-chromosome containing the gene for colour-blindness, and these men will suffer from their maternal grandfather's abnormality. This is due to the fact that the genes in the sex-controlling part of the X-chromosome of a man exercise their full effect, since they have no partner in Y to obscure their action even if recessive. Red-green colour-blindness, then, is an inherited condition, but much commoner in men than in women. Colour-blind women must be homozygotes ; they are produced when a woman who is a " carrier " marries a colour-blind man, and that is a relatively rare event.

Several other such *sex-linked* genes are known in man, including that responsible for hæmophilia, a disease which affects the clotting of the blood. The sufferers are not in special danger of bleeding to death from a cut, but they are liable to internal hæmorrhages,

and rarely reach the age of twenty-five. Hæmophilia has become famous because it is inherited in several of the Royal Families of Europe. Queen Victoria was herself a carrier. One of her sons, Prince Leopold, died of the disease, and through some of her daughters it has been disseminated widely, with consequences of great political importance. However, as with colour-blindness, only women can act as carriers. A man cannot transmit the condition unless he suffers from it, and, since King Edward VII was unaffected, the English Royal House is now free from hæmophilia.

The inheritance of sex suggests that though the genes behave as distinct units in heredity, they are not independent in their working, a consideration which merits brief study. It must first be noticed that each of them, though a distinct entity, has multiple effects, a point which may usefully be illustrated by examples.

In man, a single gene, which acts as a recessive, produces both deafness and *retinitis pigmentosa*, a grave defect of the eyes leading gradually to blindness. The first genetic studies on the fruit-fly *Drosophila melanogaster*, subsequently famous in this field, were directed to the difference between flies with the normal red eyes and a recessive variety with white. But the distinction proved to be more fundamental than appeared, and to affect also the colour of one internal organ (the testis sheath) and the shape of another (the spermatheca). In maize a gene which controls the type of starch produced by the plant affects also the rate at which the pollen tubes grow down to fertilize the ovules. These are single instances chosen from a great variety, but a wide generalization shows that the genes usually, and perhaps always, have multiple effects. For varieties, whether slight or striking, produced by their allelomorphs are less hardy than are the corresponding

normal forms—for instance, human albinos not only lack pigment in the skin, hair, and eyes, but have a shorter expectation of life than have ordinary people. Thus there is good reason for thinking that even those genes whose results appear to us trivial or superficial have, in reality, an important influence upon the working of the body.

Single hereditary units therefore are not restricted to a single type of effect; moreover, they interact with one another to produce the characteristics for which they are responsible. For example, in Asiatic cotton the development of one form of crumpled leaves requires the co-operation of two pairs of allelomorphs, each undetectable alone even when homozygous. While in the fruit-fly *Drosophila melanogaster* the genes for brown and for scarlet eye-colour combine to produce white eyes. Indeed, the effects of any particular gene depend to some extent upon its "genetic setting," which in some individuals will enhance and in others minimize them.

But the genes have to work not only in co-operation with one another but also in the conditions in which the animal or plant lives. As might be expected, we have many specific instances of the influence of the external environment upon their action—like the simple recessive responsible for producing the Siamese pattern in cats. This gives rise to black pigment at a low temperature and to a whitish-fawn shade at a higher one, the change taking place near the mammalian constant temperature. Consequently, Siamese cats are whitish when born, but their extremities, which are cooler than the rest of the body, subsequently darken. However, the fact that plants grown in different soils, or animals supplied with different food, are not necessarily the same is in itself a clear indication that the environment affects

the expression of the hereditary material. An animal may be a dwarf because its genetic constitution makes it so, or because it has had insufficient or unsuitable food. Breeding-tests in properly controlled environmental conditions may be needed to decide between the two possibilities.

Yet the low mutation rate of all organisms shows that the hereditary material itself is adjusted to withstand unaltered those changes in the environment to which it is normally exposed. A considerable increase in mutation, producing changes both in the structure of the genes themselves and in the chromosomes which carry them, can be obtained only by the use of short-wave radiation, most conveniently by means of X-rays. It must not be supposed, however, that this is merely damaging or destructive in its action, for not only do the same mutations arise from such treatment as occurs spontaneously, but they may be in either direction—from the normal to the abnormal or the reverse.

A restricted form of mutation can be obtained from the use of certain drugs, such as colchicine, which sometimes interfere with the proper course of cell division and so produce a doubling in the number of chromosomes. Such a reduplication of the chromosome set has a variety of effects, to one of which special attention must be directed: it allows an organism to use several phases of the same gene simultaneously. This introduces a new concept, yet it is a clear and logical one. We have so far considered the behaviour of two forms only of each allelomorph. But the genes must be large molecules, and it is not to be expected that they will be limited to a single type of change (mutation). Since, however, most animals and wild plants have but two sets of chromosomes, not more than two members of the same allelomorphic series can be combined in one

individual. That restriction is of course relaxed as the sets of chromosomes are increased, an event which, in rare instances, takes place naturally and, as we have seen, can be induced artificially.

The variation so far discussed is of the " discontinuous " kind, the occurrence of clear-cut alternative forms controlled by one or a few genes. But much of the variation of animals and plants is " continuous "—that is to say, the forms can be ranged in a graded series, as in human height. They do not fall into distinct classes, but the intermediates are commoner than the extremes. Yet it can be shown that heredity and environment interact to produce this type of variation also. Moreover, if the extremes are crossed, their offspring approach the mean form and prove to be less variable than the succeeding generation (the grandchildren of the original pair) produced by inbreeding. Clearly, then, the genetic component of continuous variation is *Mendelian*, controlled by units which segregate and retain their identity without blending.

Continuous variation differs from the discontinuous merely because the genes responsible for it are numerous and have small, similar, and cumulative effects. All gradations must be recognized between one type of variation and the other. Indeed, the same gene may produce striking differences in some parts of the body and small effects, taking their part in continuous variation, elsewhere. Yet there does seem to be a special class of genes, the *polygenes*; and groups of these are responsible for the continuous variation of different parts of the body. There is some ground for thinking they are in a measure distinct from the rest of the hereditary material, for they can be located anywhere along the chromosomes; even in those regions known to be otherwise inert, and having a somewhat different

chemical structure from the rest of the chromosome material.

It has already been implied that the crowning advantage of Mendelian heredity is in the field of evolution. The theory of Natural Selection developed by Darwin and Wallace has shown how plants and animals have in the past been modified and evolved from simpler forms, and how they may continue to be so in the future. It requires that organisms produce more offspring than survive, and that they should vary from one another. Those forms which are the best adapted to their mode of life will be *selected*—that is to say, they will be at an advantage compared with their less successful rivals and will tend to leave more descendants. Thus they will contribute more to posterity, and, in so far as the qualities which give them superiority are inherited, these will in time be spread through the population, causing it to change or evolve.

Hereditary variation, then, is an essential feature of this concept, but the special form of heredity required to carry evolution along the course which it has in fact followed was not appreciated either by Darwin or Wallace. A mechanism such as they envisaged, in which the hereditary units blend or contaminate one another, is dependent for its diversity upon " new variation " (that is to say, on mutation) since blending leads to uniformity. Consequently, all the hereditary variation upon which selection could act would be recent in origin and lacking in permanence. Evolution by the selection of minute advantages over long periods of time would be an impossibility with blending inheritance, yet that is the very course which it is known in the main to have followed. To achieve it, the Mendelian mechanism, which provides immense diversity capable of stabilization, is required. The rarity of

genes outside the nucleus is a measure of this need. They can exist elsewhere in the cell (in the cytoplasm), but there they are incapable of segregation and lose the evolutionary advantage which has just been indicated. It is not surprising therefore that the bulk of the hereditary material is consigned to the chromosomes.

No one who has reflected upon these aspects of genetics could accept a theory of evolution that is supposed to work by producing mutation and controlling its nature. Yet that is precisely what the famous theory of Lamarck involves, for it assumes that the use and disuse of parts of the body affects their structure and action—which is true—and causes such effects to be inherited—which is false. The same difficulty is inherent in the wider claim that environmental variations in general can so influence the constitution as to become hereditary, a fallacy long exposed, which has been revived again in modern Russia and is now a part of the official Soviet creed.

Evolution has proceeded much more quickly in domesticated forms, in which selection has been practised consciously or unconsciously by man, than by natural means ; so that there is here a great opportunity for applying the principles of genetics to human needs. Already much success has been attained in this field. Thus the difficulties raised by sexual incompatibility, either of a plant with itself or with another variety, have long attracted the attention of seedsmen and fruit growers. They are now being understood and overcome. Taking a broader view, it has become clear that we can tap the great store of heritable variability that exists in sexually reproducing organisms by inbreeding the progeny of a cross (by mating brother and sister or, when possible, by self-fertilization). Methods are

being devised for making available any desirable qualities which thus arise, by introducing them into other existing strains. Moreover, the useful effects of a gene can be improved, or its harmful ones minimized, by selecting those individuals which provide a " genetic setting " of the appropriate kind.

The application of genetic principles to man is still in its early stages. Yet it has already produced important results, notably in the cure of *erythroblastosis fœtalis*, a frequent and usually fatal disease of the newly born. It is due to a reaction between the blood of the mother and child, and this has been analysed genetically in a way that now makes successful treatment possible and easy.

There are in man many uncommon diseases due to single genes. These could never be eliminated by any eugenic programme because they are maintained only by rare mutation, which cannot be checked. That objection, however, does not in general apply to insanity, which is often due to the cumulative effect of numerous genes; a situation much more easily controlled by selection could this be correctly applied.

If half as much information were available on the genetics of man as already exists for several of the lower forms—the fruit-fly *Drosophila melanogaster*, for example—it would usually be possible to give the advice that is so much needed by members of a family in which a serious defect is inherited. For those incapable of transmitting it could be distinguished, and informed that they could marry and have children with safety. Moreover, valuable help could be given in the diagnosis of certain diseases—for example cancer. But we must await the results of further research before it is possible to reap the benefits of genetics in this way.

GENERAL MEDICINE

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BEFORE beginning to write about progress in medicine one thing has to be made plain. Advances in scientific knowledge in general can be discussed to a considerable degree without the need to review their effects upon humanity. Medicine is so much an applied subject and so frequently at grips with the problem of the selection of a correct course, dictated not only by science but also by compassion, that its progress can be reviewed only in terms of scientific advance if the realization of its humanitarian aims is also kept in mind. It is not infrequently within the power of a doctor to prolong a life that pain or other infirmity will render unbearable; the problem of how to do so is a scientific one, the question of whether to do so is an ethical one. It is not so much a question of ethical advance (I doubt if ethics have advanced in fifty or 500 years, even in medicine) as of harmonizing scientific progress with what is right and proper—a question that in the long run often has to be decided by the medical man himself.

Progress in medical thought is not an easy subject for discussion, because medicine is not very strictly demarcated from many of the sciences of today. Itself so in need of some of them, such as anatomy and physiology, as to have been virtually their parent, medicine now derives knowledge from other subjects with increasing frequency as knowledge itself enlarges. Medicine may be thought of as the application of general knowledge to the betterment of the health of mankind, to the cure or alleviation of illness in the individual and the community, and to the improvement of mankind's

environment in so far as this is related to health. Were this all, medicine would have a considerable resemblance to engineering, which draws much of its knowledge from mathematics and physics, or industry, which builds new processes from the research of chemists. Custom, usage, and the nature of the urge which makes men and women become doctors have endowed medicine with moral and philosophical problems which are not to be evaded when questions of advance in knowledge come up for discussion.

Fifty years ago medicine had relatively few claims either to be called a science or to be regarded as aware of its potentialities in this field of thought. There were, of course, some notable exceptions to this. Pasteur, not himself a medical man, had opened up the great field not only of pure, but also of applied, bacteriology, and by one of the greatest strokes of genius recorded in scientific history had shown that human beings could be inoculated against rabies, a disease caused by a virus so small that no hope of its visual demonstration existed at the time. Lister, realizing the significance of Pasteur's researches into fermentation and recognizing an analogous biological process in wound infection, had introduced antiseptic methods into surgery which—to be followed by aseptic methods—opened up enormous fields in which the surgeons could at last operate in safety. Pasteur and Lister both had to fight hard battles to obtain acceptance of their views. But in many ways medicine in 1900 was unaware of her potentialities, and though medical students began their training in the scientific disciplines of anatomy, physiology, and pathology, applied medicine—that is to say, the diagnosis and treatment of disease—was still taught as an empirical subject, experience taking precedent over scientific method, even to the point where the

latter might be scoffed at as inapplicable at the bedside. So great and so many have been the advances in medical knowledge and technique in the last fifty years that only a small selection can be described in this section, in which also it will be desirable to try to evaluate medical progress in broader terms.

Many infectious diseases produced by bacteria had had their causative micro-organisms assigned to them before 1900; bacteriology developed quickly after the work of Pasteur, and the tubercle bacillus, typhoid bacillus, and diphtheria bacillus (to name only three) were known prior to the twentieth century. In the last fifty years many more have been found and much learned about those already known. Two other classes of infective agent have been discovered since then. Many diseases are due to infection by viruses—organisms too small to be seen by ordinary microscopes. Pasteur postulated such an organism as the cause of rabies. Evidence that these existed has been confirmed through the demonstration that the diseases spread by them can be given experimentally by tissue fluids filtered through pores too small to allow the passage of bacteria. Viruses have been cultured artificially in developing eggs, and it has been shown that they will, unlike bacteria, grow only on a medium that is itself alive. Finally, the electron microscope has enabled a beginning to be made in the study of their structure. Among important diseases caused by viruses identified in the last few years are infantile paralysis, epidemic influenza, smallpox, and yellow fever. Another important class of micro-organism, called Rickettsia, which causes the typhus group of diseases, was discovered by Ricketts (after whom they are named) in 1909.

In spite of the enormous amount of research devoted

to the subject, the cause of cancer is unknown, though such ancillary causes of its appearance as chronic irritation of tissues, ductless gland abnormalities, and heredity are believed to be important. Much of this work relates to animals and little of it is, as yet, regarded as of proven relationship to human cancer.

In the field of investigation of diseases associated with nutrition the last fifty years have seen enormous developments in knowledge, particularly in relation to vitamin deficiency. Not that such diseases were previously unknown, for scurvy had been prevented or cured by lime juice for centuries, and by 1897 Eijkman had shown the relationship in Asia between the common disease, beri-beri, and the consumption of polished rice ; the disease did not occur if rice were eaten husk and all. He did not, however, postulate a deficiency disease, though Grijns put forward this suggestion in 1901. But by 1906 Gowland Hopkins, whose quiet progress at Cambridge virtually gave birth to the science of biochemistry, had obtained sufficient experimental evidence to state that life depended not only upon what appeared to be a complete diet of protein, fat, carbohydrate, and certain salts, but also upon minute quantities of as then unidentified substances, and to state that " no animal can live upon a mixture of pure protein, fat, and carbohydrate, and even when the necessary inorganic material is carefully supplied the animal still cannot flourish " (Hopkins, 1906). These substances were named Vitamines in 1912, the name being shortly afterwards changed to Vitamin. The demonstration that diseases could be due to deficiency in the food of small, often minute, quantities of necessary food factors thus occurred in the early years of the century and explained the anomalies discovered by Eijkman and by others

whose gropings after fact had not led to its realization by scientific workers in general.

> "As often happens in the development of science, a fundamental idea is foreshadowed in many quarters, but has long to wait before it emerges as a basis of accepted knowledge. As in other cases so with recognition of vitamins as physiological necessities. Their existence was suggested long ago, but a certain right moment in the history of the science of nutrition had to arrive before it could attain to universal recognition. Some workers had discovered suggestive facts, but failed to recognize their full significance; while the work and words of true pioneers lay forgotten because published when average minds were not ready to appraise them at their right value." [1]

Since the work of Hopkins a number of important diseases, such as rickets, have been proved to be due to vitamin deficiency, many vitamins have been extracted and identified, and most of them, even when of complex molecule, have been artificially synthesized.

Endocrine, or ductless gland, diseases are another important group which had just been recognized prior to the twentieth century and which has, thanks to the researches of a number of brilliant scientists, now been shown to include many diseases the causation of which was till this century a complete mystery.

Perhaps enough has been said about the increase of knowledge in the causation of disease; it is not a matter of ease to put these advances clearly to the non-medical reader, because to him the most striking changes are to be seen in treatment of, and recovery

[1] *Vitamins: Survey of Present Knowledge.* Medical Research Council's Special Report Series, No. 167, p. 11.

from, diseases which fifty years ago were only treatable by hope and good nursing, and were often—some of them always—fatal. Fifty years ago diabetes and pernicious anæmia when diagnosed were death warrants; diphtheria killed thousands of children every year; pneumonia was a dangerous disease; and there was no treatment other than nursing and the relief of symptoms for typhoid or typhus. All these, and many more, can now be cured (except in rare cases) or alleviated to the degree that a normal life can be led—thanks to the advance of therapeutics, that branch of medicine which concerns itself with treatment. In 1900 nature cured most of the curable diseases, while all that the doctor could do was to relieve symptoms and, in a relatively few cases, administer a drug that experience had shown would do good. The paroxysms of malaria could be relieved effectively by quinine; the fever and prostration of typhoid had to be watched helplessly, while drugs of no particular use were hopefully administered. Most drugs then in use were extracted from plants, and experience had shown (and pharmacology had subsequently substantiated) that a few, such as quinine, and digitalis (an extract from foxglove leaves), were of real benefit. But apart from the initial researches of a few scientists like Paul Ehrlich, there was little indication of what was to come. Largely through this man's work, which some readers will remember having seen portrayed in the film *The Magic Bullet*, attempts were made to synthesize chemical compounds that experiments indicated might be of service in curing infection. The object was to find substances of greater toxicity to microbes than to the patients suffering from their depredations. Thus, in 1909, Ehrlich was able to introduce the first of a series of new chemical substances—salvarsan—for the cure of syphilis, scoring the first triumph

for what is termed chemotherapy. The impetus thus given, research workers rapidly enlarged the number of new substances, and the scope of their application grew to include many dangerous diseases. In 1935 prontosil, the first of the sulphonamide drugs, was introduced and was found to be a powerful weapon in the cure of diseases caused by the streptococcus, a microbe that, among other conditions, causes puerperal sepsis and scarlet fever. Further synthetic work afterwards led to the production of sulphonamides that were effective against other bacteria, sulphapyridine (a credit to the research workers of the firm of May & Baker) being the first of a series that was potent against the ordinary or pneumococcal type of pneumonia. During the Second World War a further substance, sulphaguanidine, proved most effective in the treatment of dysentery.

A chance observation has not infrequently set in motion a train of thought and action. When a scientist lights on a new phenomenon he may disregard it as of nuisance value or foresee its potentialities; that latter type is usually regarded as a genius. When Sir Alexander Fleming found that a mould had grown on a culture medium where he wanted certain microbes called staphylococci to grow, and that the staphylococci in the vicinity of the mould stopped growing, he studied the matter further, finding that the mould penicillium notatum produced a substance, penicillin, which inactivated the staphylococcus. Following up this work in 1939, Florey and his team produced penicillin in a form that could safely be given by injection to human beings and in sufficient strength to cure infection by staphylococci. This microbe being the principal cause of wound infection, amenable to no drug previously found, the discovery saved the lives of thousands of wounded men during the war, and is now a principal

weapon against diseases caused by staphylococci. It is equally potent against a number of other bacteria—streptococci, pneumococci, and the spirochætes of syphilis among them. Research has, in the last few years, revealed other moulds which produce analogous substances called *antibiotics*; streptomycin is potent against the bacillus that causes tuberculosis, though not always, as yet, successful when used in treatment, and chloramphenicol is now available to cure, among other diseases, typhoid fever and typhus fever. The research into antibiotics is probably still in its infancy.

Another great therapeutic development of the last fifty years may be said to date from 1800, when Jenner discovered that smallpox could be prevented by inoculating humans with the matter from the lesions of cowpox. The artificial production of vaccines of dead or attenuated pathogenic micro-organisms has had many successes in establishing immunity by injection into human beings. Two need special note: the first, typhoid vaccine, already on trial in 1900, but not in time to prevent typhoid from claiming more victims than did war wounds in the Boer War, was able so to protect our troops in the First World War as to make typhoid fever a rare disease under conditions in which it would otherwise have been rife; the second, yellow-fever vaccine, has rendered travelling through, and life in, yellow-fever areas safe. Others, like cholera vaccine, should be mentioned.

There are two ways of producing immunity: one by stimulating the defences of the human body to produce what are called antibodies by injecting either dead or harmless variants of the microbe of the disease concerned, or modified toxins (rendered harmless by previous treatment), which stimulate immunity to develop;

the other is by injecting the serum of an animal, itself rendered immune by being given graded injections of the microbe concerned or its toxic products. The first method, active immunization, is long lasting in its effect; the second, passive immunization, gives protection only for a few days or weeks. Use of these methods has rendered diphtheria a rare disease and one that can be successfully treated when it occurs; it has provided the soldier with protection against tetanus, and has given us the means of prevention or treatment of many other diseases.

Certain moral and philosophical issues arise out of the use of vaccination against smallpox and the use of sera prepared from animals. The former is deprecated by a minority of the population as a more dangerous process than having smallpox epidemics. Statisticians do not agree with them. The preparation of sera from animals is regarded as wicked by those who disapprove of vivisection. It is difficult ethically to weigh the lives of a few thousand people every year against the occasional injecting and pricking of a vastly smaller number of horses. From the pragmatic angle there is little doubt that human beings will continue to claim first consideration, and, if they could choose, there would probably be very few horses who would leave that seventh heaven of equine bliss, the serum farm, for the toil and sweat of the usual horses' world even to escape an occasional prick.

A short account must now be written about the progress made in knowledge of diseases of tropical climates, for here it has been very great and indeed it had need to be if populations, let alone individuals, were to survive. That the problems of over-population of certain vast tracts of country are likely to follow must not at the moment concern us. Of all tropical diseases malaria

is the most important, and attention will therefore be focused upon it as an example of much wider advances in knowledge. Malaria is a disease in which progress over the last fifty years can be readily assessed, for it was in the year 1900 that Sir Patrick Manson demonstrated the infectivity of certain mosquitoes by bringing them to England and, in a place where this disease did not normally occur, inducing them to feed upon human volunteers, who subsequently developed malaria. Prior to this the malarial parasite had been demonstrated in human blood by Laveran (1880), Ross (1897) had demonstrated the parasite in dissected mosquitoes, and Grassi (1898) with his co-workers had traced out the developmental cycle in man and mosquito. But Manson made the crucial experiment.

Malaria is probably the most widespread and important disease in the world, and has in the past caused illness and death to millions every year in tropical and sub-tropical countries. It still does, but the outlook has changed. Fifty-odd years ago the knowledge had been gained of its mode of occurrence; since then this has been increasingly employed to the cure of the individual and to the prevention of the disease. Discoveries have been harnessed from sources at first sight remote from medicine, and further research—stimulated by the amount already known, in other words by what now became obviously *unknown* but *suspected*—has filled in many of the gaps in our knowledge of this disease. It is a sobering thought that humanity owes so much of this knowledge to its supposed greatest curse—war. Sir Neil Hamilton Fairley wrote in 1949:

> "Under the stimulus of war in the tropics, more scientific effort and medical man-power were

directed to intensive study and research on malaria than to any other disease in history. In such circumstances it is hardly surprising that great advances in our knowledge of this disease, and of its control, have resulted in the past decade."

But to go further back, knowledge of malaria in 1900 had established proof of its cause by a unicellular organism, the plasmodium, had shown that this was transmitted to man by the bite of certain anopheline mosquitoes, and had pointed the way to its prophylaxis by removing people from infested areas or, failing this, by rendering them more healthy. Mosquito larvæ breed in stagnant water, so stagnant water could be drained; mosquitoes feed particularly at dusk, during the night, and before sunrise, so that protection by netting of dwelling-places and of beds could be adopted. Quinine was known to cure attacks of malaria and to reduce its incidence considerably when taken prophylactically.

While it would be impossible to mention all the important advances in this subject over the last fifty years, and even more so to indicate the amount of painstaking work that gradually led to these advances, the following will illustrate some of them:

1. For a number of reasons a phase in development of the parasite in the human body was suspected in which the plasmodium existed outside the blood stream; this would account for the absence of the parasite in the blood for several days between inoculation and the malarial attack. It would also account for relapses of malaria many weeks after the blood had become free from parasites in people who could not have been freshly infected by mosquitoes. The exis-

tence of this phase was recognized in birds in 1934, but it was not until 1948 that the analogous stage was recognized in the liver tissues of a human volunteer infected with malaria.

2. In 1900 quinine was the only drug of potency in malarial treatment. In 1930 Kikuth and Schulemann discovered atebrin, which was soon found to be a potent antimalarial. Without this discovery we might well have lost the Second World War in the Far East, for the Japanese quickly overran the lands from whence came cinchona bark, from which is prepared quinine. But for the fact that chemists in the United States were able to synthesize atebrin for the cure and, of far greater importance, the suppression (virtually the prevention) of malaria, our armies in the Far East would have been rendered impotent. In contrast, batches of Japanese soldiers in Burma, cut off from their medical supplies, were in places found to be over 80 per cent infected. The search for more powerful and less toxic antimalarial drugs continued, so that in 1945 paludrine had been synthesized. Subsequent investigations gave it pride of place in the control and treatment of malaria. The search continues.

3. The substance D.D.T. (4′4′-dichlorodiphenyltrichlorethane) was synthesized in 1874, and for many years it was used as an insecticide in the fur trade. It was applied as a means of mosquito destruction quite late in the Second World War and to such purpose that it has superseded other methods. Whether sprayed locally in and around houses, or generally over wide mosquito-breeding marshlands, it is the most useful destructor known of the mosquito and its larvæ. So that it may be said in

1950 that malaria is considerably nearer to control as a human scourge and could, theoretically, by education and adequate outlay be abolished altogether.

Surgical progress, by taking in an extra twenty-odd years so as to include the introduction of antiseptic and aseptic methods, can more easily be marvelled at than described. Lister's foresight made it possible for surgeons to operate without danger of the wound becoming infected. This led to the fact that by the beginning of this century pioneers were carrying out operations that in the last fifty years have required only refinement in technique. But that refinement has been amazing. Surgery, however, is not today merely the process of operating upon a patient. Increased accuracy of diagnosis, greater knowledge of pre-operative preparation, the enormous increase in skill and ability of the anæsthetists, the study of post-operative treatment—all these sides of today's successes represent the painstaking pioneer work of a fifty-year period. Perhaps the three advances that represent the most important contributions to success have been the perfection of blood-transfusion methods and the establishment of blood-transfusion depots, realization of the causes and means of combating shock, and the use of modern drugs, particularly penicillin, to control infection, especially in relation to wounds in warfare.

Another important evolutionary process in surgery has been specialization. The complexity of technique required has caused some surgeons to devote the whole of their time to one branch of their subject; without this development the progress in neuro-surgery, thoracic surgery, orthopædic surgery, and ophthalmology (to mention some) could hardly have occurred. It is fitting

to mention here the developments in anæsthetics that have been necessary to enable surgical progress to occur.

Maternity and child welfare are perhaps more suitable subjects for consideration under the heading of social medicine, but those matters already discussed in this section have played important parts in reducing maternal death-rate and neonatal mortality to today's low figures.

There remains for consideration the harnessing to medical purpose of some of the important progressions of experimental science that have not yet been mentioned. Röntgen discovered X-rays in 1895, and these rays were first used for clinical diagnosis in 1896. By 1900, X-rays were beginning to be used for a number of purposes, but the primitive nature of the apparatus limited their application to the location of foreign bodies such as bullets, to broken bones, and to such gross features as stones in kidneys. The collateral developments, during fifty years, of apparatus, technique in relation to diagnosis, and application of the X-rays to research have led to the establishment of what is perhaps the greatest diagnostic achievement in history. Today it would probably be safe to record that round about 50 per cent of all patients passing through a hospital are submitted to X-ray examination for assistance in diagnosis of their complaint. In a number of cases, such as in tuberculosis of the lung and in the treatment of fractures, the control and assessment of progress depends largely upon the use of Röntgen-ray examination at frequent intervals. By the ingestion or injection of radio-opaque materials, organs as varied as the stomach, gall-bladder, kidneys, and arteries can be accurately photographed on X-ray sensitive plates. Nor must the use of X-rays for purposes of treatment be overlooked;

attempts in this direction were recorded in 1896, but it required the research and the improvement in apparatus over the last fifty years to bring radiotherapy to its present importance, for now it can cure a number of malignant and infectious diseases and ameliorate the symptoms of many more. The application of radium and its emanation has a similar history, and in recent years the harnessing of radioactive isotopes to medical research and treatment is a continuation of this progress.

Another diagnostic advance as important as X-ray examination has been the laboratory investigation, by microscopical, bacteriological, and chemical methods, of the blood, tissue fluids, and excretions of sick people. This highly complex subject, clinical pathology, does not lend itself to dramatic description as does Röntgen-ray work, but for the doctor of today, with his responsibility for the diagnosis and treatment of the sick, it would be difficult to say to which of these two developments in knowledge he is the more indebted.

Of many advances in medical thought—psychology and neurology for example—other writers will speak. One only is omitted which I feel should be included, but to which it is not in my competence to do justice, for not only knowledge but also all those characteristics which we call " good " and find it difficult to discuss further contribute to it in equal measure. Progress in nursing is the foundation upon which most medical advance must, in the long run, be constructed.

An attempt has been made to describe the progress in medicine during fifty years,[1] and much has, for reason

[1] When this section was nearly completed the *British Medical Journal* devoted its issue of January 7, 1950, to a series of articles on fifty years of progress, and I obtained a number of facts from this issue which I desire to acknowledge. Readers wishing to obtain fuller information are advised to refer to this excellent and comprehensive number of the *Journal.*

of space, been neglected. The following points need brief discussion :

1. The stimulus to progress arises from two sources—scientific and humanitarian. The former is the outcome of an urge to explore the unknown and is a logical product of thought. Fifty years ago scientific progress could be made by those so mentally equipped at relatively small expense ; so much was unknown. Today research requires carefully trained workers and often needs the expenditure of much money. Through its Medical Research Council, its universities, its research foundations, and some of its drug firms Britain expends large sums annually on medical research and with a degree of correlation and control that ensures a careful use of the facilities available. The humanitarian motive is more difficult to assess ; it is probably a generalized mental approach to problems that has its practical outflow through the doctors and nurses, who apply new measures of treatment and care at the bedside and in the health centres of communities.

2. Medical thought and action may make " progress " which conflicts with established belief and custom, and even law. Many medical men advocate euthanasia, a process grossly at variance with law, and a problem that, though based on objective scientific knowledge, cannot be tackled by science as it involves ethical and social considerations. Artificial insemination is not illegal, though children so conceived (unless information is concealed, which social custom is likely to render the case) might be pronounced illegitimate. For scientific knowledge to be applied in this manner

is abhorrent to a great many medical men. Some people loathe the idea of vivisection, even under the most careful control of Home Office inspectors; a considerably smaller number even refuse life-saving treatment, the product of such research, though this is impressively uncommon.

3. A survey in general terms of the benefits conferred by medical progress upon the population of the world will make it clear that it is not the knowledge of the scientist but the naïve philosophy of the earth's occupants in general that causes a lack of appreciation of what medical thought, translated into scientfic progress, can and cannot do. The following are among the causes and effects of this progress:

(*a*) Fewer people die in infancy and youth; more live to old age.

(*b*) More people, therefore, suffer and die from diseases common to old age; some of them from degeneration due to old age.

(*c*) More people, therefore, require care and treatment for disease of old age.

(*d*) More care is taken of sick members of the community.

(*e*) The community has become more health-conscious and also more disease-conscious.

(*f*) The torch of medical progress is burning in backward and in disease-ridden communities; this has led to the cure and prevention of disease and to the making of fresh tracts of country inhabitable. It is also enabling populations to increase at a rate incompatible with their food production.

(*g*) Two furious wars have acted as an enormous stimulus to applied medical research.

(*h*) The complexity of medical science is requiring more organization, more investigation, and more legislation, and may be producing more psychoneurosis.

(*i*) It has yet to be shown that those who, as the result of all this progress live longer lives, live happier lives, or that a conscious struggle to keep healthy keeps a community happy.

But this depressing analysis of benefits to humanity as a whole is offset when we consider the problem of individual benefit in terms of emotion; we have to think of the relief to thousands of mothers and fathers whose children are piloted through illnesses that in 1900 would have killed them, of parents cured and homes thereby left intact, of greater relief of pain and suffering when cure itself is out of reach. This is the overall benefit that seems to matter most, and any ensuing credit must be shared among all, from the great surgeon to the probationary nurse, the distinguished research worker to his most junior technical assistant.

Progress has brought many difficulties with it. The education of a medical student to qualification and his or her further post-graduate training in a speciality have become more involved, and the great increase in the need for specialization and specialized departments now requires an integration in the framework of medicine that is costly and cumbersome. There is danger that sight will be lost of those very aspects of the medical worker that make him most useful to men and women who are sick in body or anxious in mind. Integrity, discretion, kindness, and a broad philosophy in which scientific knowledge is able to guide action but be itself reasonably subject to the emotional urge to help those in need—these things must not be shouldered out of the

way either by scientific advance or ministerial organization. The people of the world, too, need education in the meaning of medical progress. They must learn its powers and its limitations, they should realize that medicine is the applied science of " human biology " (a phrase of Wilfred Trotter's), and that, while it can prevent and cure many diseases, it is of value only in as much as it restores them to health ; it cannot of itself make a restored life a more contented one. These, however, are the problems of the philosopher and the divine. There is an ever-present danger that, if it pays too much court to the cult of health for its own sake only, the community will develop a neurosis and sterilize itself in a rigid condition of disease-consciousness. Security is a fundamental instinct in our lives ; in medicine as in all things it may be in danger of becoming not a means to an end but an end in itself. Remember Hecate's remark to the witches : " And you all know security is mortals' chiefest enemy."

SOCIAL MEDICINE

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SOCIAL medicine, or the study of environment in relation to health and disease, is an attempt to widen the field of medical science by a shift of emphasis from the individual to the community.

There are, in theory, two spheres in which to study health and disease. The first is what Claude Bernard called the "*milieu interieur*," or the internal environment of the body, in which case the central theme is the individual; and the second is the actual or external environment, in which case the central theme is the community. In practice, the former has engaged the attention of physiologists and pathologists much more than the latter, and it is as rare to find a medical research worker studying the methods and techniques for examining communities as it is common to find the opposite.

Since disease is unlikely to occur unless man is out of harmony with his environment, and since a sick or healthy person is only one manifestation of a sick or healthy community, this biased approach to medical problems has left some curious gaps, which are becoming more and more conspicuous as our knowledge of the internal environment increases.

Yet in spite of the fact that clinical medicine is focused on sick persons and the immediate cause of disease—*i.e.*, bacteria, lack of vitamins, etc.—while social medicine is focused upon populations at risk and the remoter causes of the disease—*i.e.*, those produced by the impact of civilization on natural habits and customs—the subjects have much in common.

Faced with a live patient, it is rarely possible to do

more than establish likely relationships between cause and effect. So that although physicians are apt to regard a statistical approach to medicine with the utmost suspicion, it is only by " weighing up odds " that they ever diagnose disease or forecast what is going to happen to their patients. Since it is usually impossible to examine the whole population at risk, social medicine has had to invent techniques with " samples," but similar devices are constantly used by clinicians—*e.g.*, blood counts. By dint of observing sick persons and recording the progress of diseases, physicians have in the past initiated fruitful lines of research for laboratory workers. At present, owing to the fact that the subject is largely unexplored, social medicine probably offers even better opportunities for disclosing good research problems than does clinical medicine.

The study of environment in relation to disease is not, of course, a new science. Indeed, one can see, looking back, that this approach has done more to prevent disease than any other; for by such means the major epidemic fevers were brought under control before their nature was really understood. Of this, the most striking illustration is the story of Dr. John Snow. About a century ago (1853–54) this country was experiencing the third of a series of cholera epidemics. At that time it was thought that the disease *might* be due to a vague miasma, but was more likely to be an act of God to punish mankind for its sins; and the only steps taken against the disease had been to institute days of national prayer. A certain Dr. John Snow, however, acting on information obtained from the newly formed Registrar-General's Office, decided that it was something to do with contaminated water. To check this theory, he proceeded to investigate the drinking supplies of people living in different parts of London. He discovered that

South London was largely supplied by two private companies. At the time of the second outbreak (1848), both companies drew their water from the lower reaches of the Thames, where the water was grossly contaminated with sewage, but in the interval between the second and third outbreaks one of the companies had shifted its pipes to a point much further up the river where the water was comparatively clean. Dr. Snow looked up the addresses of the first 1,514 deaths that had occurred during the third epidemic and found that they were almost exclusively confined to the houses that were supplied by the company which had continued to draw its water from the original plant.

Hardly was this discovery made when there was a sudden explosion of the disease in central London and within ten days more than 500 people, who lived within 250 yards of the junction between Broad Street and Cambridge Street, died. Dr. Snow hurried to make a house-to-house canvass of the early cases, and quickly began to suspect a much-frequented street pump in Broad Street. He finally convinced himself that, to use his own words, " there was no other circumstance or agent common to the circumscribed locality in which this sudden increase of cholera occurred," and, knocking modestly on the door of the room where they sat in consultation, persuaded the vestrymen of the parish to take the handle off the pump. Later on, when he had time to work out his results in detail, he was able to show:

1. That sixty-one of the eighty-three persons who died during the first three days of the epidemic *regularly* drank water from the suspected well.

2. That the inmates of a brewery and workhouse in the immediate vicinity, both of which had *private* drinking-wells, had escaped.

3. That, during the same period, a few cases outside the area had died of the disease. Of these, one was a woman who had moved to Hampstead but still had water fetched from the pump because she admired its sparkling qualities; one was the niece of this woman, who had visited her and had drunk some of this water, and had subsequently died in Islington.

4. The third case proved to be a visitor from Brighton who had spent only twenty minutes in his dead brother's house near Broad Street, but had drunk the water mixed with brandy and died the next day.

The vestrymen did remove the pump handle, but it is on record that they did so only because they could not think of anything better to do.

The features of this investigation that deserve to be particularly underlined are:

1. First, that on neither occasion did Dr. Snow attempt to find out more about the disease than the fact that certain people were said to have died from it.

2. Secondly, that on the first occasion he confined his attention to a group of people who had, at first sight, nothing in common except the fact that they had been more beset by the disease than others. He then applied a stricter definition to this group—namely, water supply—and discovered that it really consisted of two groups. He then established what we should today call a statistically significant excess of cholera in one of these groups.

3. Thirdly, having established a likely relationship between cause and effect, when the second epidemic occurred he again examined the popula-

tion at risk from the point of view of water supply and disease incidence, and again found that these two were related.

4. Fourthly, that the information about cholera deaths and water supply were collected from independent sources.

Shortly after this period two unfortunate things happened. First, active inquiry along these lines became drowned in administrative work; secondly, the dramatic successes of pathology and bacteriology led the thinking minds of a whole period to suppose that disease could be conquered by concentrating on the narrowing questions—which lead to dissecting the organs and systems of the body and defining immediate causes, to the neglect of the expanding questions—which aim at defining the population in which disease occurs and seeking those remoter causes which arise from the impact of civilization on natural habits and social customs. This was a mistake of logic.

If, for example, " consumption " is defined in terms of symptoms—a persistent cough accompanied by blood spitting and loss of weight—the tubercle bacillus can be considered an answer to the question: How did this condition occur? But if consumption is defined as infection of the respiratory tract by the tubercle bacillus, it is legitimate to ask, when, where, and in whom does this condition occur?—which are the fundamental questions of social medicine.

It is not only legitimate to do so, it is also profitable, since the effect of breaking down the community into smaller and smaller groups according to the incidence of a certain disease is to define more and more accurately the " population at risk "—*i.e.*, those who, though not yet ill, are the most liable to become so. Having

discovered the population most at risk, we know where preventive measures should be applied to the community so as to reduce to a minimum the need for remedial measures applied to the individual.

Such studies of the pattern of disease in the community had never been recognized as a science " in their own right," so to speak, until the foundation of the Chair of Social Medicine at Oxford in 1942. Consequently, during the past fifty years—and indeed during the past hundred years—what has been done in this sphere has been done spasmodically and erratically by people who were primarily concerned with something else and who gave their work many different names. But it can be recognized retrospectively as social medicine by its aims, outlook, method, and logical structure.

The foremost exponents of social medicine in this country have undoubtedly been the medical statisticians of the Registrar-General's Office. The movement for the standardization of the naming of diseases, which is now international in its scope, was initiated by them for the purpose of making death-certificates a sound basis for calculating the mortality rates of different diseases.[1] They have, moreover, progressively improved their classification of industries and occupations, so that it is now possible to say what is the incidence of the main fatal diseases in the country at large, in each administrative area, for either sex, for different age-groups, and for different occupations. This is obviously one way of subdividing the community and defining the population at risk. A further advance in the Registrar-General's analysis of social

[1] William Farr was the first and most famous of medical statisticians to the Registrar-General's Office. His remark that records were to his department what weights and measures are to a physics laboratory should be engraved on the walls of every department of social medicine.

pathology was the division of the community into social-economic classes. This was introduced into the survey of 1921 (published 1928), and took the form of the Five-Class grouping of occupations in the volume for 1931 (also published with a seven-year delay). It was the demonstration by the Registrar-General's Office, in the 1921 survey, that infant mortality was a " poverty disease " which led to the campaign of 1930 and the almost universal provision of free ante-natal and infant-welfare clinics by the local health authorities in the following years. There were revealed at the same time one or two unsuspected " affluence diseases "—in the sense not that they are necessarily caused by being well-to-do, but that they are unduly common in the upper social classes. English fiction had long recognized one such complaint—the gout—but until the Registrar-General pointed it out no one had realized that angina pectoris was also disproportionately frequent in the professional and managerial classes.

Such an analysis of the population is, however, only a beginning, and the Registrar-General's figures are now most useful as standards of reference. Thus, anyone who has access to the records of a particular group of persons and wants to discover whether they are exceptional, will first compare them with the Registrar-General's figures to see if they are in any way out of line. A number of interesting investigations have begun in this way. For instance, the incidence of pneumonia in Messrs. Boots' factory at Nottingham was compared with the Registrar-General's figures for the region and for the main occupational groups, and found to be very high. The factory population was then analysed according to the various " shops," and it immediately became apparent that the workers who were handling manganese were providing the excess. This

led to some interesting medical research (which ended by showing that the complaint was not really pneumonia), but the element of " social medicine " was essentially this narrowing down of the population concerned, based on the initial observation that this community compared unfavourably with the Registrar-General's averages. Similarly, the Medical Officer of Health for South Shields, having established that the tuberculosis rates for the town were higher than the rates for England and Wales, for county boroughs in general, for the counties of Durham and Northumberland in particular, and for the boroughs in those counties, proceeded to analyse the figures first by wards and then by smaller divisions, in an attempt to decide which of the factors covered by the term " poverty " was most concerned—whether, in fact, overcrowding, malnutrition, damp housing, or lack of heat should be considered the operative factor. The fact that in this case the investigation did not succeed in isolating the dominant causal factor shows how intricate the analysis can be, and indicates the need for better techniques for subdividing communities.

The same thing was done in the now famous case of Stockton-on-Tees, where the Medical Officer of Health (G. C. M. M'Gonigle) observed that a population which had recently moved from a slum area to a new housing estate showed a marked rise in death-rate. By the progressive subdivision of this population and of a " control " group consisting of people who were still awaiting re-housing, he was able to show that among the unemployed the higher rents in the new estate had led to less money being spent on food. Finally, he established the fact that a diet which was deficient in good protein was the main factor responsible for the high death-rate.

It is important to note that when social-medicine problems are being discussed, the word " population " is used not in the narrow everyday sense (*i.e.*, persons defined by region), but in the broad statistical sense (*i.e.*, persons defined by sex, age, occupation, diet, period, etc.). Any number of people who can be covered by a strict definition constitute a population in this sense, and it is not necessary for the persons concerned to reside in the same district. If such a group or population is found to have a high incidence of some disease, and this high figure is found again in only some of the subdivisions, smaller and more homogeneous populations can be named as being at risk. Even if the subdivisions all show the same incidence, the problem has still been advanced, since in scientific investigation negative steps (which demonstrate that certain factors are irrelevant to the problem and can be ignored in further study) are often as important as positive steps.

This close alliance between social medicine and statistical method is fundamental. First, because impressions, though valuable as a starting-point for any investigation, are notoriously unreliable. Secondly, because it is rarely possible to examine a whole population. This necessitates much of the work being done on " samples " which, in order to be truly representative of the whole, have to be chosen according to strict statistical rules. Finally, statistics are essential in order to extract the maximum amount of reliable information from a mass of data.

Weaver's work on the teeth of children in South Shields is a good example of the method which has been described by Prof. Crew of Edinburgh as the " study of co-variants within a community." It arose from an impression on the part of a dentist in Westmoreland that the children evacuated from South

Shields had exceptionally good teeth. This impression had first to be verified by comparing samples of school-children from different areas. It was found to be true. Thereafter the work proceeded *negatively*. It was necessary to show by statistical analysis that the different batches of children did *not* differ significantly in social status, in economic level, in housing conditions, in nutrition, etc. The strictly medical element in the work—namely, the dependence of good dentition on an optimal intake of fluorine—was thus based on the statistical demonstration that there were populations who differed significantly from each other *only* in respect of their teeth and of the amount of fluorine in their drinking-water.

By contrast, the impressions of the people of Northampton concerning tuberculosis in the boot and shoe trade did not stand up to numerical investigation.[1] It was widely believed in the district that there was an excessive amount of tuberculosis in the trade because it was a " family trade," and that the disease was maintained by hereditary predisposition and family infection. In this belief there proved to be enough truth to hide the error. Investigation showed that there *was* a high tuberculosis rate and also that shoe-making was largely a family trade; further analysis of the data, however, showed that there was little connection between these facts, as appears from the (abbreviated) table on p. 249 based on the examination of some 3,000 workers in the town.

The figures in italics clearly show that the workers' trade determined the tuberculosis rate, rather than their

[1] An investigation into the alleged high incidence of tuberculosis in the boot and shoe industry is at present being carried out by the Institute of Social Medicine in Oxford. The statistics given below are taken from some of the unpublished data.

family; and this was confirmed by the rest of the investigation. Furthermore, the second-column figure of 14·5 per cent suggested that the children of tuberculous parents, of whatever trade, tended to go into the boot and shoe trade—*i.e.*, there was selective recruitment *into* the trade. This figure as it stands is not statistically significant, but it served as a pointer, and further investigation eventually established the fact that the trade does in fact recruit an exceptionally large number of tuberculous subjects. Statistical proof was also obtained that healthy recruits were developing tuberculosis as a result of meeting infectious persons at

In boot and shoe trade	*Workers notified as tuberculous, per cent*	*Workers with family history of tuberculosis, per cent*
Workers and parents	1·30	17·0
Workers but not parents	*1·19*	14·5
Parents but not workers	*0·57*	10·8
Neither parents nor workers	0·59	10·0

work, which finally exploded the original theory that home contacts and hereditary predisposition were the only factors responsible for the high death-rate.

The inquiries now being pursued by various departments cover a wide range, whether one considers their starting-point or the type of action to which they might eventually lead. Some are, so far as one can see at present, absolutely disinterested inquiries, undertaken to provide exact information where none exists, just because none exists. To determine the age of onset of menstruation, and of the various indications of puberty in boys, seem to be examples of inquiries which will lead to no developments that can be foreseen. However, only a little reflection on the history of science is needed to remind us that disinterested inquiries have sometimes the most startling consequences of all.

Many separate lines of inquiry are found to lead to the same focal point in the politico-economic sphere—malnutrition. A certain deformity of the spine, long familiar to orthopædists, has lately been shown to be more common among the boys of a poor rural area than among those of the same age in one of the public schools. This finding suggests that malnutrition, possibly associated with mineral deficiencies, is responsible for this disease, the cause of which was previously unknown. Some recent studies in the prevalence of stillbirths in different sections of the community also seem to be leading in the same direction. Before the war the stillbirth rate was much higher in Wales than in England, but during the war the rate declined much more steeply in Wales than elsewhere. Subdividing the Welsh and English populations in various ways revealed little deviation from the average rate in both countries, nor was there any evidence of a greater improvement in the maternity services during the war period in Wales than in England. The only factor which the investigator was able to relate to the converging trends was the system of rationing and of pegged prices of essential foods during the war, pointing again to malnutrition as one factor responsible for stillbirths.

An investigation into the death-rate of infants over four days and under one month old has been made to equate the different causes of death with different aspects of poverty. Some depend upon malnutrition, some upon density of the population, some upon size of family; but they all indicate that the greatest wastage of life occurs among the overcrowded poor just because they are overcrowded and poor, not because of any inherent weakness. There is, however, one exception to the rule that babies of the well-to-do do better than those

with poor parents. While infant mortality has declined as a whole, deaths due to injuries at birth have increased, and have increased most in the upper social classes. From a study of the figures it is difficult to resist the conclusion that there is a fashion in some strata of society which leads to unnecessary interference with the birth process. Interferences that are unnecessary are usually harmful, and this seems to be an example of privileged social custom being inferior to natural habits.

An interesting investigation, which points further back than the time of birth of a child, concerns the system of blood-grouping by which human beings are divided into three main groups according to the compatibility or incompatibility of their blood for transfusion purposes. The proportions in which these groups occur in the adult population is known, and it can be calculated how many children ought to be born as a result of the various cross-matings that are possible. According to this investigation, one of the groups falls markedly short of the calculated expectation. Because of technical difficulties concerning the quantity and the reliability of the data, the investigation is not a finished one, but only a " pilot " study designed to show that there is something to investigate. It would appear, however, that there may sometimes exist an incompatibility between the blood of the mother and that of the child such that the child is killed *in utero*, probably during the early months of pregnancy. If this suggestion were confirmed, the results would be social rather than legislative or administrative. Would anything, one wonders, induce the English people to let any factor but sentiment enter into their choice of mates ?

The investigation of tuberculosis in the boot and shoe trade indicates the need for definite legislative

action more than the other inquiries so far mentioned. A long and complex investigation on the statistical side can be summed up by saying that, within certain common limits of density, the number of new cases of pulmonary tuberculosis which develop as a result of contact with an infectious fellow-worker varies more with the number of persons working in a single workshop than with the variations of density—*i.e.*, room space per person. On the side of medical interpretation this is consistent with the hypothesis of direct airborne infection. Contrary to local and popular belief, it means that there is more exposure to infection in the modern factories with large workshops than in the older ones with small workshops. At the present moment patients with inactive tuberculosis are one of the categories of employee of whom a factory is obliged by the Disabled Persons Act to engage a certain quota. It is obvious, from the viewpoint of social medicine, that the Act should be amended, but it may be difficult to persuade the public that such action would not amount to ostracism of the tuberculous.

The examples so far given—of subjects studied, of method, of the trend of conclusions—serve to define the Oxford conception of social medicine as closely as it can be or should be defined at its present stage of development. There are other interpretations. At Cambridge there has lately been founded a Chair of Human Ecology. This title suggests a very wide definition which might include with what is generally recognized as social medicine all the rest of the social sciences and a good part of agriculture, economics, and the theory of politics. It remains to be seen how this school will develop. At Edinburgh and Birmingham the basic conception of the subject seems much the same as at Oxford. The work so far published seems

a little more social at Edinburgh, a little more mathematical at Birmingham, a little more medical at Oxford. This may be temporary, in all cases. The very idea of social medicine is a blending of two subjects in order to ensure that the peripheral areas of both shall be adequately treated. The partners have to learn to work together.

At Durham, where the Chair is called Industrial Health, the interpretation is radically different; but it ought to be mentioned, since the term " social medicine " is sometimes used at Durham as an alternative title. To begin with, the work is founded on a clinic where individual patients are both examined and treated. The patients seen are those who have, or would like to have, claims under the National Insurance (Industrial Injuries) Act. There is a thorough " follow-up " of cases by social workers and close co-ordination with psychiatric and rehabilitation services. Finally, there is a statistical analysis of the cases seen, from the medical and sociological points of view. The conception is practical, efficient, and very valuable. But how different it is from the one discussed in the main part of this paper can best be expressed by saying that the interest at Durham is centred on the sick person applying for treatment, while at Oxford the interest is centred on those " at risk."

The whole subject of industrial medicine has marked resemblances to social medicine, both in aim and in method, but differs from it in emphasis. A factory population, being definable and partly closed, is a good field for statistical analysis, and in practice a factory medical officer and a social-medicine research team may work in exactly the same way. As a rule, however, the factory doctor concentrates on diseases almost limited to that community—pure occupational hazards such as cadmium poisoning, tetrachloride poisoning, or " pneu-

matic-hammer disease," or his work is governed by certain preoccupations : production rates, labour supply, safety measures, and the interpretation of the National Insurance (Industrial Injuries) Act. Thus the end-points lie within the field of work. On the other hand, a team of workers in social medicine, analysing a factory population, studies diseases that are common in society at large and seeks results that are of general application —so much so that a strictly local occupational risk might invalidate the study. Consequently, the relation between them might be defined by saying that though both branches of study aim at guarding the health of the community, the immediate practical concerns of industrial medicine have, and are likely to have for a good many years, only secondary importance in the plans of social medicine.

In the public mind social medicine is also loosely used to refer to the increasing practice of following-up cases in the home surroundings with a view to help in treatment, rehabilitation, or psychological difficulty. And it is used (even more loosely) to refer to the increasing assumption of responsibility by the community for the health of individuals, as evidenced by the provision of many types of free medical advice and treatment at clinics maintained and staffed by public authorities. Both are aspects of clinical medicine centred on the sick person ; their only connection with social medicine is that they are to a large extent the administrative result of the demonstration that this or that disorder is a co-variant of some aspect of social structure. These " socialized " medical services arise from the fact that the community has found it easier to provide free services to the poor after they fall sick than to eliminate the factors responsible for their falling sick.

In the case of any young science, still engaged in

establishing its right to a separate existence, over-definition is a mistake. It is probably better at this stage to convey by description a general picture of what has been done through thinking along certain lines; to define the picture further by distinguishing it from activities with which it might be confused; and, finally, to state as generally as possible what are the considerations that govern its activities, so as to give some idea of its future direction.

The governing ideas of social medicine arise from the circumstances of its rebirth, which occurred at a time when vital statistics showed that important demographic changes were pending. The crux of the nation's problem today is the changing proportions of very young and very old persons. These two age groups constitute the " dependent " section of the community, and only one of them—the very young—will ever pass into the " supporting " section. Whenever the proportion of old persons in a population shows signs of increasing faster than the proportion of children, that population is heading for difficulties; for the total " load of dependency " is bound sooner or later to increase. The accompanying table shows that in England and Wales, while the " senescents " are increasing, the " immatures " are actually decreasing.

" Dependants " as Percentage of Total Population

	1901	*1911*	*1921*	*1931*
Persons over 65	4·7	5·2	5·8	7·1
Persons under 15	32·4	30·6	28·5	23·9
Total	37·1	35·8	34·3	31·0
	1933	*1935*	*1937*	*1939*
Persons over 65	7·7	8·1	8·5	9·1
Persons under 15	23·6	22·8	21·8	21·1
Total	31·3	30·9	30·3	30·2

Even when, as is still the case in this country, the " supporting " section of the community continues to increase in proportion to the dependants as a whole, the weakness is still there. There will be a temporary lightening of the burden of dependency, but this cannot last long, since the supply of recruits to the active population is dwindling. It is evident, therefore, that the changes shown in the table will eventually and inevitably result in serious difficulties for a population which is already finding it hard to produce sufficient goods to maintain even its present standard of living.

The knowledge that this country will have to bear an increasingly heavy burden calls for every possible measure to conserve its vital forces—which lie in the health of its growing and working members and still more, if we take a long-term view, in its unborn children. For this reason, studies of reproductive efficiency, child health, and occupational health tend to receive more attention in departments of social medicine than other problems.

The desire to have children is not so abundant that society can afford to see it frustrated by curable barrenness ; so the incidence and causes of infertility and subfertility must be investigated. To conceive children who are not viable, but are lost as miscarriages, is a waste of maternal effort, as are also stillbirths and neonatal deaths. Infant mortality is another way in which maternal effort is wasted when, as is usually the case, replacement of the lost infant is undertaken ; for in the end the mother has only two children or three children to show for four or five pregnancies. Early deaths are also a wastage of a possible fifty years of life in the " supporting " group of the population, and should be regarded as a loss of these fifty years, plus a varying amount of energy already invested by others in the

rearing of those who have died. Similarly, every death before the age of sixty-five means a loss of power to the section of the community which not only carries most of the burdens of society but will also have to bear an even heavier load in the future.

From the description of the work envisaged and already in progress in departments of social medicine it will be seen that every person who falls sick is regarded as a warning sign, indicating a section of the community weaker than the remainder. Other persons of the same age and sex who are in the same environment are presumed in danger. The task, often difficult and obscure, is to define the environment with precision. Since highly civilized communities always tend to have a weakening population, a science that aims at conserving vital energy has an importance that it is difficult to exaggerate.

It has been specifically mentioned that the best figures produced by the Registrar-General's Office are mortality rates. It is because of this that, so far, the greatest amount of work in social medicine has been done on fatal diseases. But in most modern communities the wastage of life through early death is exceeded by the loss of vigour and happiness caused by disabilities or diseases which either never cause death or kill only after a long time—*e.g.*, the common cold, psychological disorders, arthritis, chronic bronchitis, digestive disorders, etc. It is clear, therefore, that social medicine will not have fulfilled its purpose until it has also explored this field. The first step towards this is to discover ways and means whereby reliable statistics of sickness and disability can be collected on a national scale.

The only morbidity (sickness) rates at present available for England and Wales are those that refer to

notifiable diseases; and at present, of the diseases which are believed to cost most in human energy, tuberculosis is the only one generally notifiable. With regard to records of the other illnesses mentioned, we are only at the stage of considering how we can best procure them. It does not look as though compulsory notification would be a solution to the problem. General practitioners are so overworked and so beset with requests for certificates that they are unlikely to take kindly to anything which increases their secretarial work. To insist on further notification would be to risk a lowering of the present high standard in that respect, and so make all the records unreliable. Nevertheless, it is in association with general practitioners that the problems of chronic and low-grade illness will ultimately have to be solved. These doctors are better placed than the specialists for appreciating the role of circumstance in disease, since they are familiar not only with the homes of their patients but also with local customs and conditions of employment. They have the further advantage that they work in relatively closed communities. Unfortunately, practitioners are too busy to confirm, by statistical investigation, their impressions of what is happening around them. An arrangement, however, by which departments of social medicine placed social workers in the doctor's consulting-rooms to maintain their records and, at the same time, collected those required for academic purposes, might yield a vast amount of useful information in a relatively short period of time.

Meanwhile, there is a great deal of data on general health to be found in the records of medical examinations for National Service. From these the Medical Research Council has already compiled a valuable publication relating to twenty-year-old males, grouped

and tabulated by region, which provides a useful standard of reference. The present writer, going to the original documents, has found that they can also yield an occupational grouping. By separating the boot and shoe operatives from the remainder of those examined by medical boards in Northamptonshire and Leicestershire, it is possible to show that, at an age when their condition cannot be attributed to their work, they are already of a lower general physique than the average for the region. Moreover, since the records include men up to the age of forty-five years, it is possible to compare standards of fitness and disability for men of different ages, and to decide how far occupation influences deterioration with age. Females of certain ages are also included in the records, but the information concerning them is so far untouched.

It is important for our purpose that these records, and also those obtained by miniature mass-radiography units, are compiled on a uniform system for the whole country and include healthy as well as ill persons ; for that not only ensures that they are everywhere comparable, but also gives us the population at risk. This is unfortunately not true for hospital records, so that what at first sight might be thought to be valuable material for departments of social medicine to study has been found in practice to be of little use. The point of view of hospital specialists is so clinical that their notes generally contain little information about the social background from which their patients come. But a more serious defect from our point of view is that, for various reasons—shortage of beds, reluctance on the part of many patients to be admitted to hospital, and the " specialist " attitude of most hospital doctors—the patients admitted to hospital are not drawn from any recognizable population.

In Scotland the Registrar-General has published summaries of the sickness certificates issued for insurance purposes ; but this work is still in an experimental stage, and it is uncertain whether the diagnoses entered on these records are accurate enough to be of much use for our purposes. Nevertheless, once the thorny problem has been solved, social medicine should be able to analyse the wastage of vital energy due to non-killing diseases and " spot-map " these as accurately as fatal diseases.

It is not necessary to stop there. When the major problems of preventing wastage of life have been solved, the same outlook and the same methods could be applied to increasing health, as distinct from reducing disease. No doubt this is looking a long way ahead ; but it is worth while to do so, because, when it enters upon this third phase of activity, social medicine will need to be on its guard against a possible pitfall in its own methods. All statistical work involves the determination of " normal " values. Now the normal of any biological feature is nothing more than the most frequent measurement at a given moment. It is often, but quite mistakenly, supposed that the value corresponds to what is natural or " right " for the species. It may well be however, that the most frequent value for a given characteristic has been taken from a population that is not perfectly healthy. False standards have often been produced in this way and have been known to lead to considerable confusion. The very fact that the establishment of standards involves a great deal of labour usually means that these standards are kept in use long after the time to which they are strictly applicable.

At the moment, the task of fighting disease is so urgent that it takes precedence over studies of health ; but in

attempting this immediate task we are all the time perfecting the weapons we shall need for the later and greater work of building up health. Already, in the course of purely preventive work, we sometimes have an opportunity to set up new standards, higher than those derived from curative medicine. At present, when we have fixed or adopted a standard we are obliged to concentrate on those who fall below it, to define their environment, and to trace their distribution, in order to indicate preventive measures that may reduce their numbers. Exactly the same methods could be applied to those who surpass the standard adopted; and statistical analysis of their environment would enable us, instead of defining states of minimal vitality so as to eliminate them, to define states of optimal vitality in order to cultivate them.

NEUROLOGY

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THE central problem in neurology is the brain. This essay deals with certain aspects of the working of the brain in health and disease, in which notable advances have been made during the present century.

Electrical Activity of the Brain

One of the most stimulating advances in recent years has been in the study of the electrical activity of the brain. Electrical changes in the cerebral cortex were observed by the English physiologist Caton in animal experiments as far back as 1875, but Berger (1929) was the first to record the electrical activity of the human brain and to show that it was affected by the mental processes involved in thinking. By measuring the fluctuating electrical potentials with electrodes applied to the scalp, he was able to show that different parts of the human brain exhibit characteristic rhythmic changes of potential. These minute rhythmic potential changes could be observed with a sensitive galvanometer, or, using a modern valve amplifier similar to that in a wireless set, they can be recorded in the form of a series of wavy lines on a moving strip of paper. The record obtained in this way is known as an " electroencephalogram " (EEG).

The most striking of the electrical rhythms is one at about ten a second which is most prominent at the back of the head, in a region corresponding to the visual part of the cerebral cortex. This rhythm, which Berger called the " alpha " rhythm, is most pronounced when the eyes are closed, since visual atten-

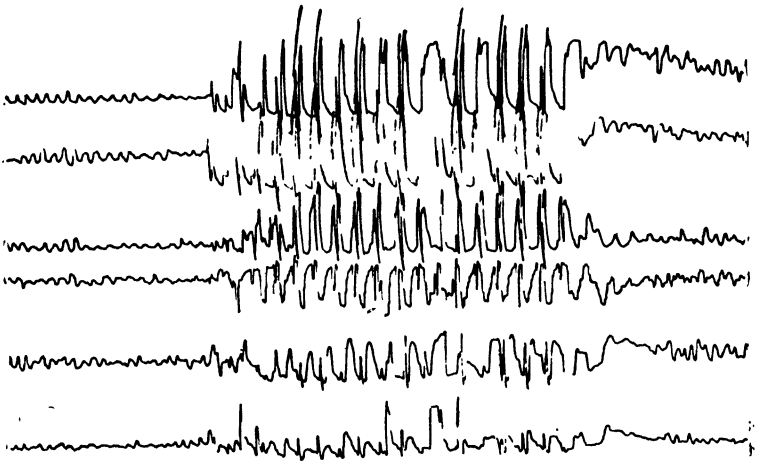

FIG. 1.—EEG of a patient with *petit mal* epilepsy, showing the sudden appearance of high-voltage wave-and-spike complexes during a seizure with temporary loss of consciousness.

tion, or even attention to a mental problem involving visual imagery, tends to make it disappear.

Berger's discoveries were treated at first with derision. This was due in part to the fact that he published his work in the psychiatric journals, which were not generally held by physiologists in very high repute. It appeared unlikely that an obscure psychiatrist could have avoided the many pitfalls in mastering a very difficult experimental technique and made one of the greatest physiological discoveries of the century; but his work was in fact very well controlled, and almost all his observations have been subsequently confirmed.

There was, however, another reason why physiologists were slow to accept Berger's results—they were not in keeping with the general trends of physiological thought of the time. Up to the beginning of the century the study of the nervous system had developed mainly, in the tradition of Descartes, along anatomical

lines. Around 1900 Langley and his students at Cambridge were busy unravelling the complex nervous pathways leading to and from the brain, while other investigators were studying the minute structure and arrangement of the nerve cells in the brain. It was in 1901 that Elliot Smith introduced the concept of the "neopallium" for the organ which effected the co-ordination of the impulses coming from the senses of touch, vision, hearing, and taste in the cerebral cortex. The idea of different organizational levels in the brain was already familiar from the clinical observations of Hughlings Jackson, and at this time Sherrington at Liverpool was developing the idea of the integrative function of the brain. The work of Pavlov, continuing through the 'twenties, on reflex activity as a factor in behaviour, added further to the concept of the brain as a kind of automatic telephone exchange in which the messages received from the outside world were co-ordinated and made to serve as a basis for the resultant motor activity of the organism as a whole.

In most of this work the emphasis was on the nervous system serving as a *passive conductor* of messages, transmitting impulses ultimately from the sensory organs to the effector organs, the muscles, and glands. In this setting it is not entirely surprising that Berger's results were hard to assimilate and to accept; for Berger was postulating that the brain acted as a *dynamic source of activity* itself. The electrical rhythms Berger revealed were apparently a spontaneous activity of the nervous tissue of the brain, and not dependent on stimuli received from outside sources or from the internal sensory receptors in the body. This was a new idea in physiology, an idea which is still being pursued at the present day. It appears that it may agree better than the older views of the brain with the

experimental data derived from the study of the forms of behaviour attributable to " instinct " and " drive," in which the brain plays an active rather than a passive role.

The application of the EEG to clinical problems has already proved fruitful. Epileptics observed during fits show increased electrical activity, and it is found that a particular kind of " spike-and-wave " pattern is associated characteristically with *petit mal* or minor fits. This is useful as a method of diagnosis, for it is not always easy, when a patient complains of " black-outs," for example, to know if these are really due to *petit mal* epilepsy, or if they are hysterical in origin, or due to some other cause. It is found, moreover, that characteristic abnormalities are often present in the EEG of epileptics when they are sitting at rest and not having seizures. When the fits or other symptoms are due to a tumour of the brain, this can sometimes be located at a very early stage and the patient cured by surgical removal of the tumour. Surgical treatment is also possible now for certain types of epilepsy in which the fits are due to a small patch of abnormal tissue (an epileptogenic focus) in the brain, which can be accurately located by means of the EEG. A superficial focus is shown by a " spikey " record due to the sudden discharge of large groups of nerve cells in the area.

A prolonged discharge of an area of this kind may be followed by a period of flattening of the record, due apparently to fatigue of the nerve cells. There is reason to believe that during this period the function of these nerve cells is paralysed. Penfield has suggested that the functional paralysis of groups of nerve-cells in the brain may be responsible for the periods of confusion and abnormal behaviour sometimes found in epileptics after a seizure.

The normal EEG depends on the age of the subject; young children show a preponderance of slow activity of relatively high voltage, but as they grow up the slow activity disappears and the alpha rhythm at ten cycles per second becomes dominant. These changes in the EEG may reasonably be associated with the maturation of the various functional mechanisms of the brain. In this connection it is significant that a high proportion of adults committed for crimes of violence were found by Denis Hill (1950) to have EEGs that were abnormal in that they showed a preponderance of slow activity of the kind found in young children. This suggests that the aggressive behaviour of these individuals may be due to a failure in the development of some of the cerebral mechanisms of the normal adult. An abnormal record of this type was found, for example, in a young man who in an aggressive outburst had murdered his mother. These observations have clearly opened up a big new field for development. They suggest that criminal behaviour may be due in some cases to forms of cerebral abnormality over which the subject may have very little control, and it is a big advance that the EEG gives a means of obtaining objective evidence of this.

The functional significance of the electrical activity of the brain is not yet known. It is a puzzling observation that the alpha rhythm is most prominent when the brain is relatively inactive, and perhaps the most interesting of the various theories which have been put forward is McCulloch's suggestion that the alpha rhythm may serve in the scanning of data reaching the visual areas of the cortex, acting in fact like the rhythmic analysis of visual data used in television. But such suggestions must be regarded as speculative until further experimental data have been obtained.

Biochemical Aspects

Brain metabolism. Most of our knowledge of the metabolism of the brain is of recent origin, for biochemistry is itself a comparatively youthful branch of science and, except for the older work of Thudichum on the fatty substances, very little was known before the present century. In recent years the study of brain biochemistry has developed very greatly, and one of the most striking facts that has emerged from this work is the remarkably high metabolic activity of the brain.

The metabolic activity of the brain *in vivo* can be calculated from: (*a*) the difference in the oxygen, glucose, and carbon dioxide content of the blood entering and leaving the brain and (*b*) the rate at which the blood flows through the brain. Neither of these measurements is very easy to carry out. The blood coming from the brain soon gets mixed with the blood from the bones of the skull and other structures, so that it is hard to get a satisfactory sample for chemical analysis, and no simple method of measuring the rate of the blood flow through the brain is known. These difficulties have now been so far overcome that satisfactory agreement has been obtained among the results of different investigators. The most accurate figures are probably those of Kety and Schmidt (1948), who obtained their samples of venous blood from a group of volunteers who allowed blood to be withdrawn through needles inserted into the internal jugular veins, high up under the ear, while arterial blood samples were obtained from the femoral artery. The rate of blood flow through the brain was obtained by introducing nitrous oxide (the dentist's laughing gas) through the lungs into the arterial blood and measuring the rate at which it appeared in the venous blood from

the brain. By these methods they found that in the normal resting state 54 millilitres of blood circulate through each 100 grams of brain per minute and in that time 100 grams of brain consume 3·3 millilitres of oxygen.

These figures indicate that in the normal resting state the brain accounts for more than one-fifth of the oxygen consumption of the whole body. Folch has calculated that in the infant and young child, in which the brain is relatively greater in size and development in comparison with the rest of the body, more than half of the total oxygen consumption is due to the brain, which therefore occupies a central position in the energy balance of the whole organism. The reason why the brain should need such a high expenditure of energy is still not known, but it is clear that the brain requires a continuous large inflow of energy even in the normal resting state, and this is not greatly affected by any kind of intellectual activity.

Metabolism in functional activity. Considerable progress has been made in the elucidation of the chemical reactions which take place in the brain. Information as to the changes occurring in the brain during life has been obtained by the chemical analysis of brain tissue taken from rats and other small animals instantly killed by immersion in liquid air. The intense cold freezes the brain solid in a matter of seconds and fixes the biochemical changes, which would be too transient to be observed by other means. In this way it has been shown that the energy derived from the oxidation of glucose is used to synthesize certain unstable phosphorus-containing compounds (the " high-energy " phosphate esters, phosphocreatine, and adenosine triphosphate). Stimulation of the brain by electrical

means or by the injection of a suitable stimulant drug is associated with the rapid breakdown of these phosphate esters and the liberation of inorganic phosphate. The evidence suggests that the "high-energy" phosphate esters serve as an energy reserve which is available to give energy for nervous activity. Besides the changes in the phosphorus compounds stimulation is associated with an increased consumption of glucose, a liberation of lactic acid, and the breakdown of a compound called acetylcholine, which may be connected with the electrical changes in the nerve cells. The following scheme illustrates one of the possible pathways by which the energy obtained from the utilization of glucose may be handed on through a series of coupled reactions and used for the functional activity of the brain (see also Fig. 3, pp. 280–281).

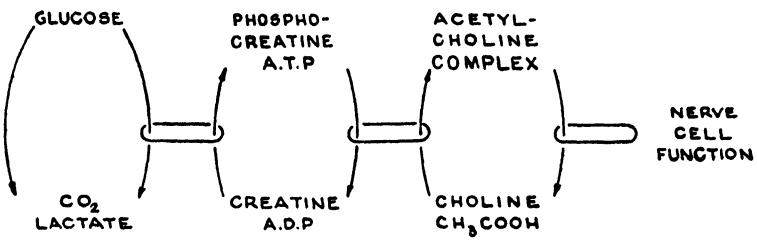

FIG. 2.—The diagram shows the method by which the energy derived from the utilization of glucose may be handed on through a series of coupled reactions and made available for the processes connected with the function of the nerve cell (Dawson and Richter, 1950: *Amer. J. Physiol.*, *160*, 203).

Further experiments by the liquid air fixation method have given evidence of biochemical changes in the brain during emotional excitement and during sleep. The brains of young rats killed by dropping them into liquid air while they are in the sleeping state show a significantly lower lactic acid content than in rats taken while they are awake, while rats which have

been excited give a brain lactic acid content considerably higher than the normal and more than three times the level of sleeping animals. Changes under normal physiological conditions have also been observed in the phosphate esters and in the brain acetylcholine, which is raised during sleep and lowered during excitement (Richter, 1950).

Another method recently developed for finding out about the metabolism of the brain *in vivo* is that of administering a radioactive element and measuring the rate at which it is incorporated into the various chemical compounds normally present in the brain. Using the radioactive isotope of phosphorus (32 P), Borell and his collaborators were able to show that there is a localized increase in the phosphorus requirements of a part of the brain-stem during coitus in the rabbit. By a similar method it has recently been found that stimulation of the brain, either by electrical means or by inducing emotional excitement under normal physiological conditions, affects the rate of incorporation of phosphorus into the fatty substances of the mouse brain (Dawson and Richter, 1950). These observations give evidence of the kind of changes which might conceivably be associated with such phenomena as memory, in which stimulation causes a more or less permanent change in the nerve cells.

Humoral transmission. The existence of the endocrine system of ductless glands was recognized by the nineteenth-century anatomists, but there has been a great development during the present century in our understanding of the physiological significance of the various endocrine glands and their control through the pituitary gland and higher centres of the brain. The isolation of the first endocrine principle—adrenaline—by the

Japanese Takamine in 1901, and the demonstration that it was a relatively simple chemical compound of which the chemical constitution could be determined, was a landmark in this field of investigation.

Through the work of Elliott, Loewi, Dale, and others on the pharmacology of the " sympathetic " nervous system there developed the idea of the transmission of messages to different parts of the body by means of chemical transmitter-substances which are liberated at the autonomic nerve-endings to produce their effects by a chemical action on the muscles and glands. The significance of acetylcholine as a transmitter-substance was brought into prominence by the classical experiment of Loewi (1921), in which he showed that stimulation of the vagus nerve to the frog's heart caused the liberation of a chemical " vagus-substance " which resembled acetylcholine and could account for the action of the vagus in slowing down the rate of beating of the heart. It remained for Dale to synthesize these scattered observations into his brilliant concept of the " adrenergic " (or adrenaline-liberating) and " cholinergic " (or acetylcholine-liberating) divisions of the autonomic nervous system. Recent work has shown that the transmitter-substance of the adrenergic nerve-endings is mainly noradrenaline, but that does not affect the main hypothesis, which is now generally accepted.

The hypothesis of transmission by chemical mediators was applied by Brown, Feldberg, and others to the transmission from one nerve cell to another in the autonomic nerve ganglia and to the transmission from the motor nerve endings to the skeletal muscles. Chemical mechanisms of transmission have also been postulated for transmission in the brain and spinal cord, but here the evidence is very

much less satisfactory and, although the part played by specific substances is being actively investigated, this view has not found general acceptance: for the central nervous system the electrical-transmission theory still holds the field. It is clear, however, that the electrical potentials of the brain, as of the muscles and other organs, are electrochemical in origin, depending in fact on the movements of electrically charged ions. The electrical rhythms of the brain are sensitive to relatively slight changes in the biochemical milieu, such as may be produced by overbreathing or by drinking a few pints of liquid. It would appear that the significance of the electrical rhythms of the brain may be better understood when more is known of the biochemical mechanisms which give rise to them.

Mental Illness

Diagnosis. Mental disorders are of many different kinds, differing in their seriousness for the individual, in the way they affect society, and in the means that must be employed in planning for their prevention and cure. This diversity is expressed in the terms which are used to describe the various forms of mental disorder: the three main groups, comprising *mental defect*, the *psychoses*, and the *neuroses*, can each be subdivided into a large number of special forms of illness according to their underlying pathology, causation, or clinical features.

At the beginning of the century psychiatrists were greatly concerned with attempting to classify their patients into precise categories showing common clinical features. The delineation of the main diagnostic groupings achieved by Kraepelin and others was of the greatest value, but today it is recognized that a considerable proportion of the patients who are

mentally ill cannot be satisfactorily grouped merely on the basis of their mental symptoms into precise disease entities such as are found in general medicine. As Adolf Meyer pointed out, the same causative factor—such as alcohol, for example—may affect different people in different ways, giving rise to a variety of different clinical pictures depending on a complex interplay of constitutional, hereditary, and environmental factors, all of which must play a part in determining the characteristics of the ultimate mental breakdown.

Where social failure is a prominent feature, the condition can hardly be assessed without reference to the social standards prevailing in the group forming the personal environment of the individual, as may be illustrated by Binet's often-quoted report on a patient: " In Paris he is an idiot, but in the country he is just one of the normal inhabitants." A limited number of mental illnesses, such as Mongol idiocy, with a known or ascertainable pathology, can be defined. But other conditions, such as " hysteria " and " hypochondria," are clearly not disease entities in the same sense; they are reaction states describable only in psychological terms. In between these two extremes are the large group of " functional psychoses " (the manic-depressive psychoses, schizophrenic psychoses, etc.) which must still be described in psychological terms, since no underlying pathology has as yet been discovered. It would appear that the concept of multiple causality, through the interaction of a number of variable causative factors, may be nearest to the truth.

The conventional clinical diagnoses of mental illnesses are thus useful for practical purposes and of real significance where they refer to disease entities which can be clearly defined, but the present tendency

is to attach less importance to the labelling of patients into categories merely on the grounds of morbid behaviour appearances or psychological symptoms.

General paresis. Scarcely fifty years ago this was one of the commonest forms of mental illness ; it accounted for some ten per cent of all admissions to the asylums, and it was regarded as incurable. It occurs characteristically in the strong healthy man, at or near the prime of life. The onset can take a number of different forms, depending on the previous personality of the patient. It may be that he will show an increasing loss of interest in his work, an impairment of judgment, or a lack of ability in attending to his affairs. Delusions of grandeur are occasionally seen, or there may be evidence of loss of self-control. As the condition of the patient deteriorates there are increasing signs of dementia, and death usually occurs within three years of the onset of the disease.

In 1906 Wassermann first applied the complement-fixation test as a method of testing for the presence of the antibody arising from a syphilic infection. This was first done by animal experiments on monkeys infected with syphilis, but later he applied the method as a blood-test for the detection of syphilitic infection in man. As far back as 1857 it had been suspected that general paresis might have a syphilitic basis, but this had always been contested. The Wassermann reaction gave a means of testing the point, and it was soon established that the blood and cerebrospinal fluid of general paretics gave a positive reaction for syphilis in a very high proportion of cases. This was confirmed by the actual demonstration of spirochætes (the causative organism) in the brains of general paretics by Noguchi (1913). Subsequent work has shown that

general paresis develops in some four per cent of all cases of syphilis and starts ten to twenty years after the primary infection. The introduction of the Wassermann reaction did more than establish the syphilitic basis of general paresis; it gave for the first time a reliable laboratory test that could be used for the diagnosis of a psychosis at the onset, before the more florid clinical signs had developed. Treatment could thus be started before the disease had advanced so far that the brain had been damaged beyond repair.

The next important discovery in relation to general paresis was that of salvarsan by Ehrlich in 1910. It would hardly be possible to exaggerate the importance of the long and arduous series of investigations which finally led to the introduction by Ehrlich of the first really effective anti-syphilitic drug. By means of this the incidence of syphilis has been steadily reduced, and at the same time the incidence of general paresis has been made to fall.

Salvarsan and the other arsenical drugs are highly effective in the treatment of primary syphilis, but they are relatively ineffective when once the spirochæte has become established in the brain. Ehrlich's discovery gave a method of prevention, but not of cure, and it remained for Wagner von Jauregg (1917) to devise the first effective treatment of general paresis. The method he adopted, of treatment by infection with malaria, is now well known; if started early enough, it is successful in a high proportion of cases.

The idea of curing one disease by giving another was not entirely new, for physicians had previously noticed that mental illness could be suddenly and inexplicably cured when the patient happened to get some incidental fever. Wagner von Jauregg had even published a small book on this as a young man, but it remained

unnoticed until he himself brought it to light many years later. The story is told of how, as an elderly man, after election to the Chair of Psychiatry at Vienna, he went to the University Library to look up his earlier work, with the idea of pursuing it again. He found the little book with the pages still uncut. No single person had read it; and the idea which was to prove one of the greatest advances in medicine had remained lost and buried all those years. In recent years other methods of inducing a fever have come into use, and penicillin has proved to be a further aid, but the main principles of the treatment remain the same.

Manic-depressive psychoses. Elation and depression occur as normal reactions in the normal individual; they are pathological only when they occur without an adequate exciting cause. Many people are subject to periodic mood-swings from one extreme to the other, but when these are so severe that a depressive phase may lead to suicide for example, they necessitate hospital treatment and care. Psychotherapy may sometimes be helpful, but there are many severe cases in which " convulsion therapy " is much more effective. This method of treatment is relatively old (camphor injections were used in 1798), but it was revived and developed by von Meduna (1933), who used the synthetic drug leptazol as a method of inducing convulsions. A further advance was made by Cerletti and Bini (1937) when they introduced the method of inducing convulsions by passing an alternating electric current through the brain. They got the idea from observing the effects of electrical stimulation of the brain in pigs. Their electro-shock treatment was so safe and easy to carry out that it has now practically replaced the

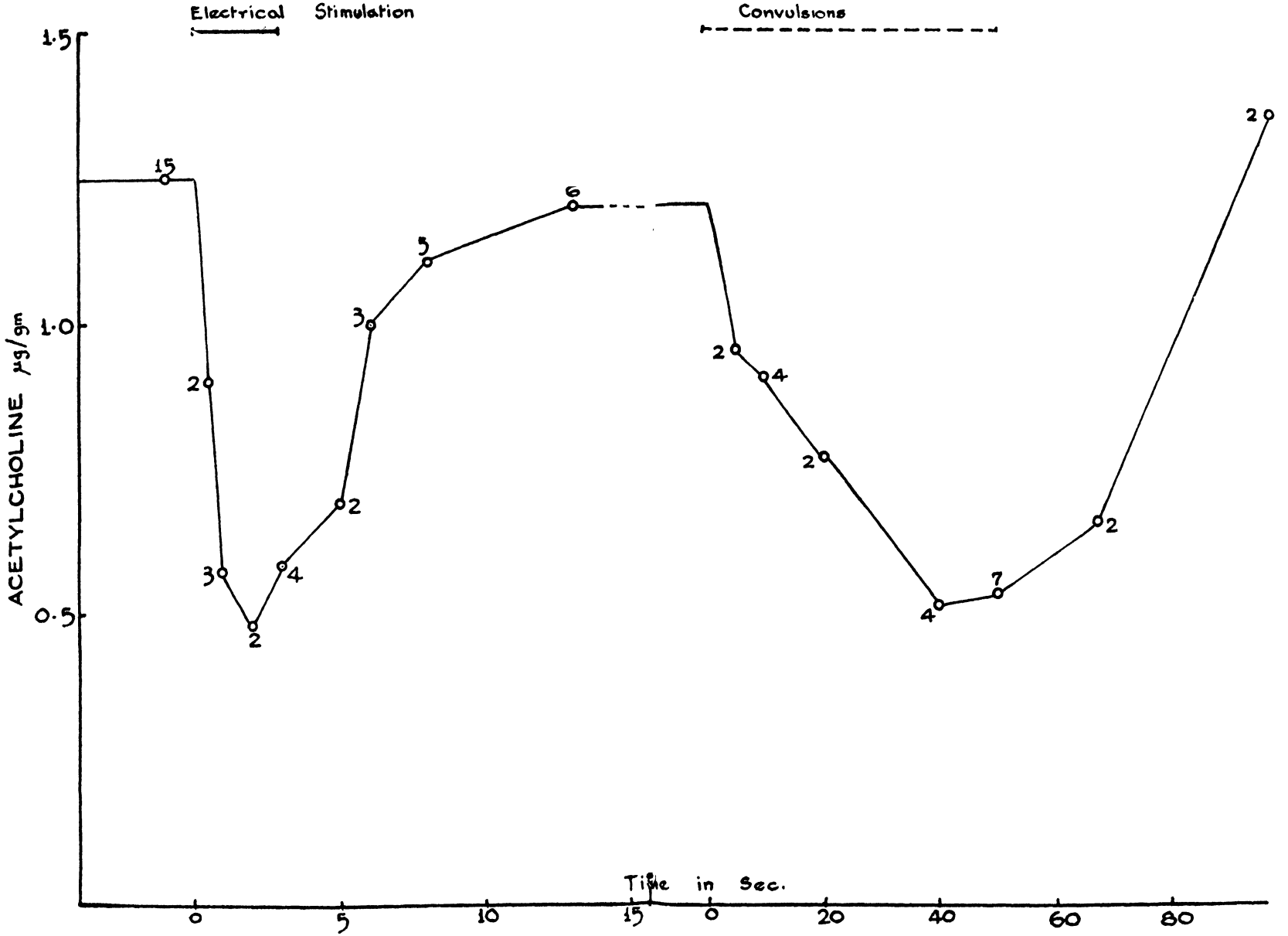

FIG. 3.—Showing the effect of electrical stimulation and subsequent convulsions on the acetylcholine content of the rat brain. Each point gives the mean value for two or more animals, the number of which is given alongside. Electrical stimulation was given for three seconds except for animals killed after a shorter period of stimulation. The latent period before the commencement of convulsions varied from eight to fifteen seconds for different animals. The values during and after convulsions are therefore given on a separate scale starting at the time when convulsions commenced (Richter and Crossland, 1949: *Amer. J. Physiol.*, *159*, 247).

older methods. An attack of acute depression will generally get better of its own accord in time but, apart from the danger of suicide, it may necessitate a long period in the mental hospital. The results of electro-shock treatment are often dramatic. By this means an attack of depression may be cut short, so that, instead of lasting for months or even years, the patient can be sent home in a matter of a few days. Indeed, the treatment can be given in the out-patient clinic right at the onset of the attack, so that admission to the mental hospital can be avoided. It may be noted that the electro-shock treatment appears to cure the *symptom* of depression rather than the underlying cause of the illness, for further attacks are still liable to occur. But even so it is an advantage if the attacks can be cut short and long periods of hospitalization with eventual chronicity can be avoided.

Schizophrenic psychoses. As is well known, the word "schizophrenia" means "split mind"; but the majority of schizophrenics do not conform to the popular idea of a Jekyll and Hyde type of mental splitting. They show rather a kind of mental fragmentation in which the normal processes of thinking are disintegrated and there is a lack of agreement between the thought content, conduct, and emotional tone. The schizophrenics account for some forty per cent of the permanent mental hospital patients. Many of them live for months on end completely withdrawn from reality, living an unreal fantasy-life of their own.

The true nature of schizophrenia is a problem that has not yet been solved, but research has revealed a few facts which may serve as pointers for further work. Recent investigations have confirmed the work of

Kretschmer in showing that schizophrenia is less common in those who are short and stocky in body-build than in those who are tall and thin ; constitutional factors must therefore play a part (Rees, 1947). Genetic studies have shown that the incidence increases with the closeness in blood relationship to a schizophrenic, with a dual incidence of seventy to eighty per cent in identical twins. These figures show that genetic factors are important, but since the incidence is not 100 per cent in identical twins, environmental factors must also operate. The chronic schizophrenic is generally less reactive than the normal subject to stresses of many kinds ; there may be impairment of liver function, and unspecific endocrine abnormalities are frequently present.

Of special interest are the observations made by Gjessing (1932) in an investigation of periodic catatonia (in which the mental derangement recurs periodically with relatively lucid intervals in between). He showed that the mental changes are associated with periodic changes in the nitrogen metabolism, and both can be influenced by the administration of thyroid gland, which is effective in some cases as a method of treatment. The " insulin coma " treatment of schizophrenia (Sakel, 1934) is also an advance. The patient is given increasingly large doses of insulin daily until a dose is reached which causes him to pass into a state of coma due to the lowering of the glucose level of the blood. The patient is then brought out of his coma by administering glucose, and this procedure is repeated daily for a period of several weeks. This is the most effective method of treatment available at the present time, and the results are at least encouraging, but it cannot be said that a really satisfactory method of treatment has yet been found for schizophrenia.

Neurosurgical treatment. Brain operations have been tried from time to time in the past, but the credit must go to Moniz (1934) of Lisbon for introducing the first effective method of surgical treatment for a mental condition. He got the idea from observing monkeys that had had frontal lobe operations, and the methods he developed depended on cutting the connections of the prefrontal areas of the brain.

The prefrontal leucotomy operation is particularly effective in those patients who are in a state of constant anxious tension. After the operation a change in the personality can generally be seen. The patient becomes much less concerned about his old worries and more childlike and care-free in disposition. With this change there may go a blunting of ambition, creative ability, and drive. In some cases patients who were previously meticulous about their manners and appearance have become slovenly in their habits and embarrassingly frank in the remarks they make in public. It is clear that the relief of tension is not obtained without some cost, and the operation does not remove other psychopathic traits that may be present; but yet in special cases it may enable a patient who was previously disabled for years by constant emotional tension to go back to his home and work again. The operation is not a panacea for mental illness, but in a small number of carefully selected cases it may be well worth while.

Apart from the prefrontal leucotomy operation, a number of other brain operations are now being tried, and the new information about cortical localization gained incidentally from this work has added greatly to our knowledge. These operations can be carried out with the patient fully conscious, using only a local anæsthetic for the incision through the skin and bone,

for no pain is experienced when the brain itself is cut.

Mental Defect

In this category are included those with mental disabilities present from birth or infancy, whatever may be the cause. Our understanding of mental defect has advanced with the recognition that intelligence is a strongly hereditary trait, depending apparently on a group of genes distributed in such a way that intelligence varies quantitatively throughout the population like height or weight, in the manner of a Gaussian distribution curve. Most people have roughly average intelligence, but there are small groups well above and well below the average. The group of lower intelligence extends into the mental defectives, where it constitutes the large class of " subcultural " or physiological defectives (Lewis, 1933). These " subcultural " defectives have no characteristic physical abnormalities ; they include most of the " high-grade " defectives, and they come for the most part from homes in which the parents' intelligence is also well below the average. Often there are more than one from the same family. Apart from measurements of intelligence there is no real line of demarcation between the defectives of this group and the general population ; the distinction is mainly a social one, depending on the ability to earn a living and to be self-supporting. While some need institutional care, others are able to obtain employment as domestics, hotel porters, etc. The capacity of such individuals for improvement by education is very limited, and the problem would appear to be mainly one for the sociologist and geneticist.

Besides the undifferentiated defectives there is a mixed group comprising most of the low-grade defec-

tives—anencephalics (born without brains), mongols, cretins (with thyroid deficiency), spastic diplegic idiots (in whom nerve cells of the cerebral cortex have failed to develop), and others with recognizable physical abnormalities. Here the condition bears no obvious relation to the intelligence of the parents, and we are dealing more often with accidents such as birth injuries and infections which have caused damage to the brain. In this group an advance has been made in the recent recognition by Gregg (1941) that German measles in the pregnant mother can give rise to defective infants. Important from another point of view was Fölling's (1934) recognition of a form of mental defect associated with a metabolic abnormality shown by the excretion of abnormal amounts of a chemical substance called phenylpyruvic acid in the urine. Biochemical investigation has shown that the essential metabolic error is apparently a congenital inability to metabolize phenylalanine. The condition is inherited as a rare recessive defect with a gene frequency of about 1 in 200 in the population.

Considerable interest attaches to the recent suggestion of Yannett and Liebermann (1946) that mental defect may result in some cases from an incompatibility of the blood groups of the parents. It is known that incompatibility with respect to a blood constituent known as the " rhesus " factor (since it was discovered in rhesus monkeys) can give rise to infants that are stillborn or born with jaundice, and it would appear that when jaundice occurs other abnormalities including mental defect can also result from this cause. It is certainly a new idea in this field that normally healthy parents, who can have healthy children by other individuals, may have a subtle form of incompatibility which prevents their having healthy children by each

other. In such cases artificial insemination has been successfully employed, and healthy children have been produced in this way.

Neuroses

Included under this heading are conditions, such as the anxiety states, obsessional neuroses, and the many forms of hysteria, which do not generally necessitate mental hospital care, but which may yet be a serious handicap and may even be completely disabling. It is usual to distinguish between the neurotic, who himself suffers through his disability, and the " psychopathic personality," such as the sexual pervert and the delinquent, who causes others to suffer ; but the distinction is not really fundamental, and ultimately both are problems for society.

Mental conflict can give rise to physical disease, as is seen in the large group of conditions such as peptic ulcer, now recognized as " psychosomatic." Conversely, physical factors may influence many forms of neurosis. In the nineteenth century there was a good deal of confusion between the conditions that are organic and those that are functional, but the clinical advances such as the introduction of Babinski's sign (in which movements of the toes are used to detect breaks in the nervous pathways to and from the brain) have given more effective methods for differentiating between the two.

The brilliant contributions of Freud to our understanding of mental mechanisms raised high hopes that psychoanalytical methods might sweep away a great deal of the mental illness of the past. Psychoanalysis was applied enthusiastically as a method of treatment in every kind of mental illness, but the results were often disappointing. Even in the milder forms of

neurosis, which appeared most likely to respond to psychoanalytical treatment, experience has shown that an understanding of the mechanism does not necessarily confer a cure, and the process is extremely time-consuming as it may have to be continued for a matter of years. It would appear that the patients who are suitable for full-length psychoanalysis must be selected with great care, and the method is of limited application as a method of treatment. Many psychiatrists therefore prefer to use a more active form of psychotherapy in which suggestion and other methods are employed and good results can be obtained in a shorter time.

Physical methods are now very generally used as a supplement to psychotherapy in the treatment of the neuroses. Sedation with phenobarbitone and similar drugs is used on a large scale, and " drug abreaction " or " narcoanalysis " (in which an emotional experience is relived while in a comatose state) has been found of value as a means of releasing emotional tension due to an injurious emotional experience. Modified insulin and electrical shock treatment are also methods in use. There are many forms of physical treatment of special application, such as vitamin and hormone administration to correct deficiencies where they are present, and even salt administration, which is effective in those forms of " tropical neurasthenia " due to salt deficiency. Physical methods are also used in the treatment of alcoholic addiction. The value of active psychotherapy, using suggestion and other methods, is well established, and this is generally needed as an adjunct even with the physical methods of treatment.

Environmental factors are clearly important in the causation of mental maladjustment, and the psychoanalytical approach has focussed attention on psycho-

logical factors operating in early life. These are now well known and, as Adrian has remarked, " nowadays it is almost the rule for a convicted prisoner to ask for more favourable consideration because of the psychological mismanagement of his early childhood." It has been noted that maladjusted individuals are often the children of parents who have been divorced, and divorce has been blamed for a good deal of delinquency and neurosis in later life. There is good reason for believing that parental disharmony and conflict can have a deleterious influence on the children, but there is less evidence that divorce as such should be held responsible, and it may be questioned if it is always better for the children that their parents should live together in disharmony than that they should be divorced. The majority of divorced parents re-marry, and it would appear that the significance of divorce as a causative factor has been rather exaggerated in some quarters.

Research on experimental neurosis in animals has shown that neurotic behaviour can result from conflicts and frustrations occurring in animals; and here the psychopathological mechanisms of the psychoanalysts are hardly applicable. The investigators engaged in this work are generally very careful not to claim that the experimental neuroses in animals are the same as may occur in man, but the view is widely held that they may in fact be very similar. Animals show great individual variation to stress. As Pavlov (1927) remarked, " the same influence may produce a profound disturbance in some individuals and show no trace of effect in others, according to the power of resistance of the nervous system in each case." It is common knowledge among animal-breeders that qualities such as " vice " in horses and viciousness in the

bulls of the dairying strains are strongly hereditary. Recent large-scale investigations of neurosis and psychopathy in service casualties during the war have again emphasized the importance of hereditary factors in the causation of these conditions in man. This was found, for example, by Slater in his studies on sexual perversion, which may go with congenital endocrine abnormalities.

Retrospect and Prospect

The application of new electrical and biochemical techniques during the last fifty years has added greatly to our knowledge of the electrical mechanisms and intermediary metabolism of the brain. It may be reasonably expected that some of the fundamental mechanisms of cerebral activity, such as the process of learning, may be revealed by further work along these lines. This may also give a basis for a more rational approach to the problems of nervous and mental disease.

The magnitude of the problem of mental illness is not generally appreciated. Statistics show that of every thousand children born, eighty will have a " nervous breakdown " and thirty will spend some part of their lives in a mental hospital. Many more will fail to grow to their full mental stature, owing to the development of disabling forms of neurosis. Official figures for 1948 show 181,019 occupied beds in the overcrowded mental hospitals and institutions of this country. Including mental defectives under statutory care, there were 249,093 persons notified as mentally ill or defective on January 1, 1948. These figures may be compared with the 230,249 occupied beds in hospitals for all other diseases put together. Similar figures have been recorded in other countries throughout the world. The problem of mental illness is in-

creasing rather than decreasing, for the greater expectation of life achieved by research in other branches of medicine is resulting in an increase in the number suffering from senile dementia.

Research in this field has been held back by the old idea that " insanity " is incurable. Advances during the present century have shown that many forms of mental illness can be cured or prevented by medical means. The greatest single advance is probably in the prevention of general paresis, which less than fifty years ago was one of the commonest and most degrading forms of mental disease. Through the work of Wassermann, Ehrlich, Wagner von Jauregg, and others, this has become a condition that can now be accurately diagnosed, prevented, and cured; and thereby a whole ocean of human misery has been relieved. More could hardly be asked of science than that. Less obvious in England than in other parts of the world is the benefit derived from the work of Hopkins and others on the vitamins in relation to the causation and treatment of the nervous and mental symptoms of pellagra, beri-beri, and other forms of nutritional disease. The results of convulsion therapy and the insulin coma treatment for schizophrenia offer encouragement for further work, but the main problem of the functional psychoses still remains to be tackled.

The modern views on mental illness have emerged only slowly from the background of superstitious belief in which the subject was for so long enwrapped. The influence of the old Biblical teachings, which regarded insanity as the direct handiwork of the Devil, can still be detected in the irrational tendency of the general public to regard mental illness as a kind of personal disgrace—an attitude not held to the same extent about

any other form of disease. Many have sought to find " the cause of insanity " as if this were a single condition to be cured by the discovery of some magic formula. At the beginning of the century the " toxin " theory, which attributed mental illness to the action of toxins from the bowel, etc., was challenging the older theory of hereditary degeneracy. The successive application of different methods of investigation led to Folin's suggestion that mental illness might have a metabolic basis, and to Goodall's view that it was due to a virus infection. Mott obtained evidence of endocrine abnormalities, which he believed to be responsible; the psychologists have regarded most forms of mental illness as psychological in origin; the geneticists have thought in terms of morbid genes, and recently the electrophysiologists have inculpated electrical " dysrhythmia " and such factors as " alterations in the feed-back relationship of reverberating neural nets." There are certainly elements of truth in many of these views, and they need further investigation; but since it is now known that there are many different forms of mental illness, none of these hypotheses is likely to be universally applicable. The contemporary tendency is to consider that mental illness is not due to any one cause, but that the clinical picture is generally determined by the interaction of a number of different constitutional and environmental, metabolic, genetic, toxic, infective, and psychological causes.

If one may speculate as to the direction in which present research is leading, it would appear that further progress may be made by the application of genetics to the improvement of mental health. It is easier to plan for children who are mentally sound and healthy than to repair a brain that is maldeveloped through some inherited error, or already damaged by disease. A

number of genes, such as the ABO, MN, Rh, and P blood factors can now be identified; the extension of this method by the use of known genes as markers, or by the direct identification of morbid genes, might contribute significantly towards the prevention of mental illness by making possible the suitable choice of favourable combinations and avoidance of those that are pathogenic and likely to lead to mental illness. The early recognition of hereditary metabolic errors might also enable them to be corrected by biochemical means, as in the treatment of diabetes with insulin, so that the development of mental illness could be avoided.

There is a common notion that mental defectives are happy individuals—" O happy, happy moron. He doesn't give a damn! " Since there is no known method of measuring happiness, we have no scientific evidence on this point; but, so far as can be judged from personal observation, it may be doubted whether mental defectives are generally happy. Too often they make a little hell for themselves and for everyone else who has to deal with them. Real happiness requires mental qualities of the highest order, intelligence, sensitivity, and energy as well as other qualities less readily defined. Attempts, such as have been made in the past, to eradicate mental illness by sterilization are for theoretical as well as practical reasons, unsatisfactory and ineffective: none of the main forms of mental disease could be eradicated by such means. Yet in this country, as in other parts of the world, there is a growing appreciation of the need to plan for quality rather than quantity in the population. It may be noted that the Japanese Eugenics Bill of 1948 (amended 1949) takes account for the first time of those who are " mentally unfit to be a parent." The need is, however,

for positive rather than for negative measures, so that the majority of the population are drawn from those with the finest mental qualities that the country can produce, instead of being mainly drawn, as at present, from those of lowest intelligence and ability. A good deal of further knowledge is required, but even at the present time much might be done by encouraging larger families for those who are up to the average and above the average in talents and ability. Thus it should be made economically possible for professional men and women to marry younger and to bear the additional cost of bringing up and educating more children. Conversely, the present high fertility rate of border-line defectives might be reduced without great hardship by social measures designed to encourage the segregation of the sexes and later marriage in this group. Such measures should not only reduce the incidence of mental defect, but they might also go a long way towards improving the general mental quality, educability, and stamina of the community. It is clear that mental health is not just an isolated matter for the psychiatric specialist, but a problem of wide public interest in which all the biological sciences are involved, and which is of concern to all who are engaged in the social field.

Our knowledge of the brain and mind, and the factors affecting mental health, is still very limited. It would appear likely that in years to come, when it is time for the science of the twenty-first century to be reviewed, there will be no field in which the present century will seem more backward and our present ignorance more profound.

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PSYCHOLOGY

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Psychology as a Natural Science

BY Aristotle and his followers the word *psyche* (generally translated " soul " or " mind ") was used to mean the principle of life, manifested most completely in its higher or conscious aspects. Hence psychology may best be defined as the *study of mental life*. From the time of Aristotle down to about a hundred years ago psychology was treated almost exclusively as a deductive branch of natural philosophy. But by the end of the nineteenth century it was already claiming a place as an inductive branch of natural science.

The change was due to two main influences: first, the work of the great German experimentalists, Fechner, Helmholtz, and Wundt; secondly, the introduction of a biological standpoint by British contributors, such as Darwin, Spencer, Huxley, and their numerous followers. In our own country, during the opening decades of the new century, the leading exponents of this double point of view were McDougall at Oxford, who laid greatest stress on the biological approach, and Myers at Cambridge, whose early work at the new laboratory did so much to convert British psychology into an experimental subject. Even at the present day, most of the leading psychologists in Great Britain are pupils of these two pioneers.[1]

[1] In America the establishment of psychology as an experimental science similarly owes much to E. B. Titchener, who began his academic life as a student of philosophy at Oxford and later emigrated to the United States.

Down to the end of the First World War, the chief concern of psychologists in this country was to emancipate general psychology from the domination of philosophy, much as a hundred years before physiologists in Germany had struggled to free themselves from the domination of medicine. And during the years that followed, as we shall see, individual psychology has had a similar struggle to resist the efforts of the doctors to treat the study of individual personality as a branch of psychiatry. At the same time, psychology has unquestionably learnt much from philosophy and medicine; and has borrowed freely from the criticisms and constructions of both.[1]

In 1900 the unquestioned leaders of psychology in Great Britain were still philosophers by training. Sully at London was Grote Professor of Mind and Logic; he wrote as the last representative of the utilitarian school, which owed its fundamental doctrines to Bentham and James Mill. Ward at Cambridge, and Stout, who went from Cambridge to Oxford and from Oxford to St. Andrews, inclined rather to the Scottish view of mind, treating it as a unique entity with active powers. All three, however, were decidedly sympathetic to the introduction of experimental methods.

[1] Cf. Flugel, J. C., *A Hundred Years of Psychology* (1933), especially Part III, "1860 to 1900." In 1827, as a result of the activities of Bentham and his "rationalist" friends, University College, London ("London University" as it was originally called), was eventually founded to provide a university education for those who, owing to their inability to conform with religious requirements, were debarred from proceeding to the ancient universities. Owing largely to its origin, this College has been free to adopt a more empirical approach to problems of the mind. Thus Carpenter's *Principles of Mental Physiology*, which (quite unfairly) was accused of scandalously reducing man to a "physiological automaton," was dated from "The University of London, 1874." The first psychological laboratory in this country was opened by Sully at University College in 1897, with Rivers, and (three years later) McDougall, in charge. Cambridge followed soon afterwards.

But elsewhere the battle still raged between the protagonists of the new science and the philosophers of the older persuasion. Kantians, Hegelians, and even a few members of the newer associationist school, joined in holding that consciousness from its very nature could not be subjected to such methods as had been adopted in chemistry, physiology, or biology.[1]

Nor were the representatives of the well-established sciences themselves entirely free from doubt. But in 1906 the President of the British Association for the Advancement of Science, Sir Edwin Ray Lankester, Professor of Zoology at University College, chose for the title of his address the word " Psychology," and boldly claimed that " the emergence of this subject as a definite line of research seems to me one of the most important features of scientific progress during the last quarter of a century." The establishment of a separate subsection of Psychology a year or two later, and of a full section in 1921, may be taken as indicating a final victory for the scientific psycho-logist.

In the field of general psychology British work has largely been concerned with problems which Continental workers had to some extent neglected—such as those of learning, of the emotions, of the higher mental processes, and of the physiological basis of mental life. In the field of individual psychology it has made unique contributions of its own. For obvious reasons

[1] At Oxford in my day Prof. Prichard used regularly to set his students an essay on the question " Is Psychology Possible ? " and left no doubt that in his opinion the only possible answer was " No ! " When I sought permission to work under McDougall for Greats, my tutor assured me that all that was to be known about the human mind could be found in the works of Aristotle. Today Oxford has its Institute of Experimental Psychology and its own Professor of Psychology.

general psychology has been chiefly studied at the older universities, and individual psychology in London.

I. *General Psychology*

The Psychology of Learning. During the first few years the types of experiment carried out in the newly improvised laboratories were concerned largely with the sensory processes. At Oxford, McDougall endeavoured to find fuller confirmation for the British three-factor theory of colour-vision, originally propounded by Young and subsequently developed by Helmholtz. At Cambridge Myers carried out investigations on auditory sensations, with a special interest in musical appreciation.

Since British psychology had been typically a psychology of association, it is not surprising that some of the earliest experimental studies were concerned with the formation of associations. In London Sully [1] was especially interested in child psychology; and, partly owing to the growing interest of educationists, an increasing attention was paid to the subject of learning in its two main branches—memory and habit-formation. Memory was still largely regarded as essentially the association of ideas; habit as the association of movements.

Experiments on habit-formation in animals were begun by Darwin in the nineteenth century, and continued during the present century by Lloyd Morgan. This led to a trial-and-error theory of learning; and a visit of Lloyd Morgan to Harvard gave rise to the more systematic methods introduced

[1] Sully had published a *Teacher's Handbook of Psychology* as early as 1886. Thanks largely to his work and that of teachers who had worked under him, the periodical *Child Study* reported, between 1900 and 1914, a number of simple investigations on the learning processes of school-children.

by Thorndike. The experiments of Hobhouse are not so well known; but they form in many ways a remarkable anticipation of Köhler's experiments on animal insight. Woodworth, who for a short time was Sherrington's assistant at Liverpool, joined with Thorndike in carrying out a long series of experiments on the alleged transfer of training. These experiments were later repeated by Sleight and others in London, and did much to overthrow the fond belief of educationists in the idea that mental faculties could be directly improved by exercise. Later still, at Cambridge, F. C. Bartlett carried out a suggestive series of experiments on the whole process of remembering.[1]

Physiological Psychology. Two hundred years ago Hartley had put forward the fruitful notion that the so-called associations of ideas were essentially physiological associations between " nerve-processes " in the brain. It was not, however, until Bain published what was in effect the first text-book on psychology[2] as such that a systematic endeavour was made to treat the physiology of the nervous system as an essential foundation for the study of the mind; and, even at the beginning of the twentieth century, Sherrington was still able to declare that " we know far less about the brain than we do about the mind, and the study of the latter is often the key to the former."

During the present century the outstanding contribution to physiological psychology in this country—it is tempting to say, in the whole world—were the series of remarkable investigations carried out by Sir Charles

[1] Morgan, C. Lloyd, *Habit and Instinct*, 1896; *Animal Behaviour*, 1900. Hobhouse, L. T., *Mind in Evolution*, 1901. Bartlett, F. C., *Remembering*, 1932.

[2] *The Senses and the Intellect*, 1864, followed some years later by *The Emotions and the Will.*

Sherrington and his numerous fellow-workers on the integrative action of the nervous system. He began by detailed work on the localization of functions in the brain; but his most important contributions were studies of the conflict, inhibition, co-operation, and composition of reflexes. The reflex was regarded as a convenient experimental abstraction. The essential notion was the idea of the nervous system as organized in a " hierarchy of systems within systems," the higher and more comprehensive controlling the lower and more specialized. How suggestive this idea could be was shown by McDougall's *Physiological Psychology* (1905), which was largely an attempt to interpret Stout's doctrine of apperceptive systems in terms of corresponding neural systems.[1]

In this country the later work of the Russian reflexologists and the American behaviourists has had considerable influence on the views of contemporary psychiatrists; but to psychologists it has seemed to be largely based on the over-simplified hypotheses of their own predecessors in the nineteenth century. Pavlov, we are told, while still a young student of theology, happened one day to pick up a book by G. H. Lewes (the friend of George Eliot) on the physiological basis of the mind. He was so impressed with the idea of a physiological approach that he decided forthwith to devote his life to an experimental study of the subject. In describing his results he based his explanations on the somewhat over-simplified associationist theory which Lewes and other

[1] Sherrington, C. S., *The Integrative Action of the Nervous System*, 1906. Sherrington, C. S., *et al.*, *Reflex Activity of the Spinal Cord*, 1932. A note should also be made of Head's important *Studies of Neurology* (1920), which greatly modified the over-simple notions of aphasia, and the earlier work of Gaskell and Langley on the autonomic system.

English writers of that day commonly adopted. In America, Pavlov's doctrine of reflex association (the theory of the acquired or " conditioned " reflex, as he preferred to name it) achieved for a time a considerable vogue, owing to the widespread acceptance at that time of the so-called " behaviourist " standpoint. But in Britain the attempt to reduce all mental activity to terms of stimulus and response, and all higher processes to the effects of mere conditioned reflexes, was felt to be a reversion to the crude assumptions of the nineteenth-century physiologists. The ingenious techniques and innumerable experiments of the Pavlovian school undoubtedly contributed a vast accumulation of suggestive facts ; but the basic principle had already been clearly stated by Stout (though in a less uncompromising form), in describing how automatic reactions could be built up on the basis of innate reflexes by what he called " motor association." [1] During the last ten years even Americans have tended to discard the sweeping simplifications introduced by Watson. In their place various forms of " purposive behaviourism " now seem to be effecting a kind of synthesis of Watson's behaviourism with McDougall's purposivism—doctrines which twenty years ago appeared diametrically opposed.

Higher Mental Processes. In describing the results obtained with his ingenious experiments on associative reactions, Galton had urged the need for extending experimental techniques to a still higher level in the hierarchy of mental processes, and investigating what he

[1] *Manual of Psychology*, 1899, 5th ed., pp. 221 ff. Stout further recognized, what is implicit in the conditions of Pavlov's experiments but was not explicitly recognized until the later stages of such work, the need for motivation.

called the " higher thought processes." Those who followed up this line of work were led to consider other kinds of mental connection and other kinds of mental content, in addition to those assumed by the associationists. Ward, Stout, Lloyd Morgan, and their pupils agreed in recognizing more elusive types of content besides mere sensations and images (for example, relations, relational schemes, and meanings), and higher types of cognitive process besides simple apprehension (for example, judgment). In particular, it appeared that a distinction must be drawn between *unconscious* mechanical association (such as association due to temporal contiguity) and *conscious* associations (such as associations of similarity and contrast, and relations of space, time, etc.). Ward and others even proposed a classification of such relations—later adopted with modifications by Spearman.

Early experimental work in this country was greatly influenced by Binet's studies of reasoning and thought-processes. During the period 1900–18, however, the most active work upon these problems was that carried out under Külpe at Würzburg. A number of British psychologists—Watt, Flugel, Pear, Valentine, and Burt—studied for a while in Külpe's laboratory; and on their return commenced more systematic experiments on the psychology of relations, attitudes, controlled associations, apperception, and reasoning processes generally. Many of the tests introduced for this purpose—analogies, reasoning tests (with words), apperception tests (with pictures), completing verbal and non-verbal matrices—have since been widely used.[1]

[1] Cf. Burt, C., " Experimental Tests of Higher Mental Processes," *J. Exp. Ped.*, I, 1911, pp. 93–112; Spearman, C., *The Nature of Intelligence and the Principles of Cognition*, 1923.

Gestalt Psychology. Even older British associationists, like Hartley, J. S. Mill, and Alexander Bain, had realized that neither perception nor thought can be wholly explained in terms of mechanical association alone. A little inconsistently, they usually invoked some kind of supplementary principle, such as that of so-called " mental chemistry," whereby the combination of associated processes mysteriously gave rise to entirely new qualities. In Germany the issue was most clearly formulated by von Ehrenfels, who pointed out that, in dealing with melodies transposed from one key to another, what the hearer perceived was, not the isolated notes associated with one another, but a melody or pattern with a quality of its own—a " Gestaltqualität," as he described it. Von Ehrenfels, however, described the phenomenon rather as a problem waiting to be solved than as something at last explained.

An attempted solution was offered by Stout in terms of what he called " noetic synthesis " guided by " schematic apperception." This, he held, was characteristic of mental processes at every cognitive level. Thus the sharp lines previously drawn between the three main levels of perception, learning, and thought or reasoning began to fade a little, and intellectual processes came to be regarded as manifestations at different planes of the same type of cognitive activity. Early experiments by McDougall on the ways in which we attend to visual stimuli—*e.g.*, to regular but meaningless dots or to ambiguous diagrams and pictures—demonstrated that what we see is nearly always a total pattern rather than the isolated elements, and that the nature of the pattern varies with the attitude, interest, and other inner conditions of the observer's mind. This McDougall interpreted by supposing that, in the process of perception, the mind unconsciously interprets the raw sensory data

in terms of apperceptive systems built up in the course of past experience. The meaning which the stimulus has for the observer is thus in point of fact something that he has read into, imposed upon, or as it were projected on to, the unmeaning stimuli before his eyes. Since such apperceptive systems differ widely from individual to individual, it followed that the manner in which a given person might perceive some ambiguous figure, such as an ink-blot or a blurred drawing, would throw a revealing light on his mental systems, attitudes, and interests.[1]

After the First World War, when Köhler and Koffka visited England, their accounts of the phenomena of Gestalt were readily accepted. Owing, however, to the strong atomistic tendencies in current German psychology and the strong associationist tendencies of American behaviourism, their doctrines seemed far more revolutionary in Germany and America than they did in this country. To British psychologists they appeared to be providing, by means of a series of brilliant and ingenious experiments, a detailed verification and amplification of the general principles already clearly formulated by Ward, Stout, and their various followers. Perhaps for that very reason, until quite recently, British psychologists have tended to neglect this field of experimental research.[2]

The Psychology of the Emotions. Down to the end of the nineteenth century the discussion of all psychological problems was in the main both static and

[1] Stout, G. F., *Analytical Psychology*, 1902 ed., esp. Bk. I, Chap. iii, and Bk. II, Chaps. v–viii. McDougall, W., "The Physiological Factors of the Attention Processes," *Mind*, N.S., XII, 1903.

[2] Cf. Köhler, W., *Gestalt Psychology*, 1935. Wertheimer, M., *Productive Thinking*, 1945. Thouless, however, has made some important experimental contributions: cf. Stout, G. F., *Manual of Psychology*, 5th ed., 1938, especially Thouless's *Appendix* and Stout's *Supplementary Note.*

intellectualistic. It is true that the larger text-books of the time, like those of Bain and Sully, devoted a good deal of their space to the discussion of feelings and emotions; but the treatment was still mainly descriptive, and divorced from the study of conduct or, as it was commonly called, of will. They and other associationists treated human conduct as the resultant of a rational balancing of anticipated pleasure over anticipated pain, with certain modes of action becoming eventually habitual as a result of repeated association. Except for a few uninformative investigations on physiological accompaniments—changes in breathing, pulse-rate, and the like—the whole subject was almost entirely ignored by the experimentalists. Psychology as a whole was commonly described as dealing with the analysis and classification of conscious "states." On the other hand, the evolutionary doctrine, which treated man as an animal similar in innate constitution to other mammals, strongly emphasized the view that human action was by no means the calculated outcome of intelligent perception or the conscious anticipation of pleasurable and unpleasurable consequences, but was largely due to unreflective impulses inherited as part of each man's inborn mental constitution, and serving simple biological purposes. The emphasis was no longer on mental "states" but rather on mental "functions." Even sensation and perception, which had commonly been treated as a passive reception of "impressions," were now described as modes of mental activity.[1]

In the field of emotions the protagonist was Mc-

[1] The general emphasis on activity is due largely to critics of associationism—T. H. Green, Ward, and Stout. But Herbart (whose standpoint greatly influenced Stout) had emphasized the need for turning psychology into a dynamic rather than a merely static science.

Dougall. His *Social Psychology*, which is largely a study of human instincts, was first published in 1908. By 1942 it had reached its twenty-fourth edition; and over 100,000 copies had been sold. As the best-seller among all psychological writings, it has had a widespread influence. McDougall held that what were popularly called emotions are merely the subjective aspects of curtailed instincts; and contended that the study of the emotions, both innate and acquired, was essential to a proper understanding of human incentives and motivation. More than any other work, his book has overthrown the old utilitarian philosophy of pleasure and pain as the primary motives, and has laid afresh the foundations of sociology, economics, and political theory.

Largely owing to a misunderstanding of what McDougall meant by " instinct " and " purpose," the doctrine was forcibly attacked by the " behaviourists." However, with the recent decline of behaviourism in America, McDougall's concept of human instincts, often re-christened " needs," " urges," or " drives," has come once again to the fore.

One of McDougall's fundamental tenets was that individuals are largely unconscious of the motives that actually animate them. To understand how individual conduct develops, he argued, we have to trace its genesis to the earliest days of childhood; study the effects of the " conflict of impulses that is characteristic of the early levels of mental life "; examine the development of the complex emotional dispositions or " sentiments " that the child acquires, first for his parents and immediate relatives, later for himself (his " ego-ideal "), and finally for abstract ideals and ethical conceptions. To demonstrate the nature and importance of unconscious mental processes, he carried out numerous

experiments in hypnotism and other forms of suggestion, and successfully showed that, both in artificial and in natural circumstances, a person might be wholly unaware of the hidden impulses that prompted his actions. Here he was largely influenced by von Hartmann's work on *The Philosophy of the Unconscious*, by F. W. H. Myers' speculations about the subconscious, and by the work of French writers, such as Charcot and Janet, in the field of abnormal psychology.

Following the suggestive work of British neurologists like Jackson and Sherrington, McDougall conceived of human personality as being organized, on both its cognitive and its affective or conative sides, into a hierarchical system of sub-systems and sub-sub-systems, which might be broadly represented as forming successive levels of increasing complexity and comprehensiveness. The lowest level was that of the unconscious spinal and subcortical reflexes; above this were the simple sensori-motor circuits, accompanied by sensation and simple feelings of pleasure and unpleasure; the perceptual level consisted initially of the innate instincts with their accompanying primary emotions; above this again was the associative level consisting of half-unconscious habits, attitudes, or memories, acquired during the individual's life-time, with their accompanying secondary emotions; and the highest level of all—the relational level—comprised conscious interests and rational sentiments, with the self-regarding sentiment controlling the whole system, as a generalissimo commands his army. This provided a useful scheme for the study of personality as a whole, particularly in its affective and conative aspects, much as the theory of general and special abilities did on the intellectual side.[1]

[1] Cf. his *Social Psychology*, 23rd ed., pp. 431 f.

The success of Sherrington in studying the reciprocal relations of reflexes, and of statistical psychologists in applying factorial methods to the study of individual behaviour,[1] led to an attempt to re-formulate Herbart's notion of psychology as a dynamical science, with " conative tendencies " taking the place of what Herbart called " presentations." Such tendencies were conceived as analogous to measurable " forces " or vectors, capable of counteracting (" inhibiting ") or reinforcing (" facilitating ") one another. The " resultants " in observable behaviour were regarded as analysable " components," combining in accordance with the parallelogram law.[2] The direction of the tendencies was defined in terms of external situations, observable before and after the change; and the " equilibrium " was regarded as a condition of adjust-

[1] See below, Section II, p. 313.

[2] Herbart, indeed, in his later writings himself insisted that " So long as it considers man as standing alone, psychology will remain one-sided." McDougall repeatedly contends that an individual mind " may be defined as a system of purposive forces ", and that man is essentially a social being set in the midst of a wider collective system of mental forces, which has " the power of moulding all its component individuals " (*The Group Mind*, 1920, p. 9). For individual psychology, the corollary of the view I have stated in the text appears to be that, in assessing emotional or moral characteristics, our measurements must be based on observations of stimulating situations and observable behaviour, not on impressionistic ratings of individual traits. The first attempt at a factorial approach to the whole problem is reported by Burt, C., " Primary and Secondary Factors Underlying the Emotions," *Brit. Ass. Ann. Rep.*, 1915. Edgeworth (who had been one of the earliest to suggest what is now called factor analysis) had attempted a mathematical treatment of behaviour (a *mécanique sociale*) based on the older notion that " as electro-magnetic force tends to a maximum energy, so pleasure force tends to a maximum energy," and introduced the valuable conception of a psychic " field of force," in which the individual or individuals are set (*Mathematical Psychics*, 1881, pp. 13, 18). A suggestive application of the vector-treatment of " psycho-neural tendencies " to educational and ethical problems will be found in Maxwell Garnett's *Education and World Citizenship*, 1921.

ment in which outer as well as inner forces were involved, not merely as an equilibrium within the isolated mind. This conception, which no doubt somewhat over-simplifies the actual facts, was found extremely suggestive in dealing with such problems as delinquency, emotional disorder, and motivation generally.[1]

II. *Individual Psychology*

The Work of Galton. During the twentieth century, however, by far the most striking lines of advance, at any rate in Great Britain, have been made in the spheres of applied rather than of general psychology. Here once again, by their encouragement quite as much as by their own original contributions, both McDougall and Myers were largely responsible for the rapid progress made. Contrary to early expectation, the most fruitful field of work turned out to be, not the study of mental hygiene, mental training, or the principles of mental work, but the study of the mental differences between individuals.

As a special branch of study, differential or individual psychology was first placed on a genuinely scientific foundation by the highly original work of Sir Francis Galton. During the earlier half of the nineteenth century the crude and speculative attempts of the phrenologists had brought the whole subject into

[1] So much attention has been paid to McDougall's doctrine of innate instincts that his critics have overlooked the fact that, being an anthropologist who had done valuable field-work, he laid equal emphasis on cultural influences as well. Much of what is typical in the view of Ruth Benedict and Margaret Mead, which seemed new in other countries, will be found in the writings of British anthropologists like Marett, Rivers, Myers, McDougall, and particularly cultural anthropologists like Malinowski (cf. his *Argonauts of the Western Pacific*, 1922, and *Sex and Repression in Savage Society*, 1927).

discredit. Jevons had gone so far as to declare that a scientific study of the individual mind was impossible. Galton, by devising appropriate experimental techniques, like that of mental testing, and new statistical procedures, like that of correlation, showed how these more complex problems could be investigated with rigour and precision.

Influenced largely by the biological theories of his day, he had turned his attention more especially to the investigation of mental inheritance. After a long series of original contributions of his own, he secured the establishment in 1901 of a Biometric Laboratory at University College, with Karl Pearson as its Director.[1] The new laboratory had the sympathetic support of James Sully, who had charge of psychological teaching at the College, and was the author of what was for long the most widely read text-book on *The Human Mind*; and it worked in close co-operation with the laboratory in Sully's own department. At the same time McDougall, hitherto a junior lecturer, was made a Reader in Experimental Psychology—a post which he continued to hold jointly with his Wilde Readership at Oxford.

One early result of the informal collaboration between the biometric and the psychological departments was the development of a scheme for an anthropometric survey of the British Isles, similar to that undertaken between 1875 and 1883 for physical characteristics, but now designed to include assessments of mental characteristics based on specially devised and standardized tests.[2] McDougall became responsible for drawing up a scheme of testing; and with this end in

[1] Cf. Pearson, K., *Life of Galton*, 1920, especially Vol. II.

[2] *British Association for the Advancement of Science: Annual Report*, 1908, pp. 372 ff.

view set his research students to work on preliminary studies of suitable techniques. One or two had been trained as teachers; and at their suggestion it was agreed that the first practicable step might be a series of trial surveys carried out on children in the elementary schools.

Child Psychology. Galton himself had long advocated the introduction of psychological methods into English schools. Towards the end of the nineteenth century, with his help and that of officials at the London Education Department, Sully had founded a Child Study Society (1896), with a journal publishing results collected by teachers and other workers. And about the same time a practical problem arose which gave a further impetus to these schemes.

By the terms of a recent Act of Parliament (the Defective and Epileptic Children Act of 1899) pupils who were unable to make progress in the ordinary school classes were liable to be certified as mentally deficient, and to be transferred to special schools. It was a common assumption in those days that the vast majority of the population were born with average intelligence and those who were not must be suffering from some kind of congenital disease or some pathological condition of the mind. Consequently, the diagnosis of defective pupils was left in the hands of school doctors, who relied mainly upon measuring the size of the head and looking for what they termed " nerve-signs " and " stigmata of degeneracy." [1] Surgeons even professed to cure mental deficiency by

[1] A good illustration of this procedure is to be found in the small volume on *Mental Faculty*, by Francis Warner (Professor of Physiology in the Royal College of Surgeons), 1892, especially pp. 52 ff.

cutting the skull to allow the brain to develop more readily; as Tredgold reports, "the mortality was exceedingly high, and the survivors showed no demonstrable improvement." Psychological investigations subsequently showed that such criteria were almost entirely worthless and that no sharp line was discoverable between the defective on the one hand, and the normal on the other; with rare exceptions the school defectives simply formed the dull tail-end of a normal and normally distributed group. Of those transferred to a special school on the doctor's certificate, over 30 per cent (it was stated) turned out not to be mentally defective in any acceptable sense; while a large proportion of those who were irremediably handicapped were missed and left in the ordinary school. The strongest critics of the medical approach were the school-teachers, who felt that the doctors knew far less about the minds of children than they did; and a rapid demand arose for a more adequate method of detecting those who were certifiably deficient.

Eventually in 1913 the London County Council decided to appoint an official Psychologist—the first appointment of its kind in the world. At McDougall's laboratory at Oxford and later at Liverpool, what would nowadays be called a child-guidance centre had been informally established on a small scale, largely as an experiment; and teachers had been encouraged to bring backward and difficult children for psychological testing and examination. In London such a centre now became part of the school inspectorate. The duties of the Psychologist were broadly phrased: they were, not merely to examine and report on problem cases—backwardness, delinquency, emotional disorder, and borderline defectives—but also to carry out psychological inquiries into practical educational prob-

lems.[1] As a result, the schools of the London County Council were virtually thrown open as fields of research to properly qualified students of psychology, the majority of whom at that date were themselves school-teachers. The outcome of their work was the series of standardized tests of intelligence and school attainments that are now so widely used all over the country today.

The occurrence of more specialized disabilities, and later on the need for improving the scholarship examinations and methods of entrance to trade schools, schools of art, and technical institutes, inspired an increasing number of studies on special aptitudes. The methods eventually worked out consisted partly in the construction and application of special tests, and partly in the elaboration of schemes for studying the entire personality of the individual child. To assess every conceivable trait would plainly be impossible. Hence efforts were made to draw up a short list of independent —*i.e.*, uncorrelated—key-qualities, from which other traits might plausibly be inferred, and which could be regarded as comprising the " highest common factors " entering into various trait-groups.

With this end in view a rigorous statistical technique was gradually developed. Some such procedure was first hinted at by Galton in his attempts to find a more suitable set of physical characteristics for identifying criminals (in place of the rough and ready Bertillon scheme); Galton's ideas were elaborated by Edgeworth and Pearson into a method of deducing " index

[1] Cf. Burt, C., *Annual Report of the L.C.C. Psychologist*, 1915; also *Report on the Distribution and Relations of Educational Abilities* (L.C.C. Report No. 1868), 1917. Mention should also be made of the work carried out at Birmingham under Prof. Valentine and Sir Peter Innes, and at Edinburgh under the lead of Prof. Thomson and the Scottish Council for Research in Education.

characters " or " factors " from a table of inter-correlations. As applied to mental rather than physical characteristics, the method ultimately earned the somewhat formidable title of " factor analysis." Thanks to the experimental studies of a long succession of investigators, mostly teachers in the London schools, a basic scheme of mental factors, both intellectual and emotional, was gradually established.[1]

In America, owing to the powerful influence of the behaviourist school, the search for purely objective methods of child study led to a comparative neglect of the subjective approach. In this country, however, the objective assessments were supplemented by observational and introspective procedures; and the real centre of inquiry continued to be the inner mind of the child—his developing interests, his changing outlook, his imagery and ideas, his motives, conscious and unconscious, and even his dreams and daydreams.[2] The psychologist, instead of impersonally examining the youngsters referred to him as a doctor would examine his patients, joined in the child's games, parties, and outings; and, by stage-managing standard situations, and by working out a play technique, he endeavoured to gauge, not so much the child's re-

[1] For the early history of this field of work, see Board of Education *Report on Psychological Tests of Educable Capacity*, 1925, Chap. i.

[2] Gardner Murphy, in his *Historical Introduction to Modern Psychology*, draws attention to this difference in the form of " child study " pursued in the two countries. The subjective and even sentimental interest in the child's own point of view is plainly seen in the publications of the Child Study Society, in Sully's early *Studies of Childhood*, in Nunn's influential text-book *Education: Its Data and First Principles*, and later in the writings of Susan Isaacs and C. W. Valentine. Under Sir Percy Nunn the London Day Training College (now the Institute of Education) became, after the death of Sully, the chief centre for scientific work in the field of child psychology.

actions to artificial stimuli like tests and official interviews, as his normal reactions to the circumstances of his ordinary life.

Not unnaturally, in 1913 the appointment of an educational rather than a medical psychologist had been vigorously opposed by the doctors. Defective and retarded children, they declared, were " mentally ill " ; and " the idea of relying on mental tests " was dismissed as " merely relying on a new-fangled and transitory fad." In 1928, when the establishment of child-guidance clinics for school children was under consideration, the chief medical officer of the Ministry of Health, Sir George Newman, insisted that they must be under the direction of a medically qualified person. As a result, the direction was usually placed in the hands of a psychiatrist, who, however intimately acquainted with the mental diseases of adults, had little knowledge of children and no training in psychology. Testing was now recognized as part of the psychologist's duty, but he was supposed to be concerned with that alone ; the field of " personality " was claimed as the province of the doctor. In practice, this meant that the child's personality was largely interpreted in terms of psychopathic classifications and fanciful speculations drawn from psycho-analytic literature, instead of being based on objectively verified conclusions derived from sustained first-hand research. As a consequence, research in child psychology suffered a serious set-back ; able students who were interested in the subject, assuming (as they thought) that no senior posts would be open to mere psychologists, no longer came forward for such training.[1]

[1] Scotland (where such clinics have usually remained under the direction of a psychologist) has suffered less in this way ; and during the past fifteen years probably the best work in the field of educational and child psychology has been carried out by the Scottish Council

Industrial Psychology. In an informal way, many head teachers in the past have made a practice of offering advice to pupils on the choice of the most suitable career. In 1910, the Advisory Committee for Juvenile Employment was instituted; and, even before the outbreak of the First World War, the Education Department of the London County Council, with the aid of its inspectors, teachers, and psychological workers, gave special consideration to the possibility of using scientific methods in selecting appropriate occupations for school-leavers. Several heads of large industrial and commercial firms had also shown an effective interest in this novel method of approach. As a result the idea of vocational psychology rapidly took shape, and developed a twofold aim—to find the best job for each person and the best person for each job.

During the First World War a committee of the British Association was set up to examine the ways in which psychologists could assist in the national effort, and, among other problems, considered the possibility of introducing psychological procedures for selecting officers and allocating recruits. In Great Britain, however, though a number of suggestive experiments were carried out, nothing extensive was attempted. When the United States joined the war they mobilized their psychologists almost as soon as they mobilized their Army. By 1918, nearly two million American recruits had been systematically tested; and the favourable reports on the results so achieved supplied a strong impetus to the idea of applying psychological procedures to the study and classification of adults.

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for Research in Education. Great progress has been made in the United States. For a general survey of current knowledge and views, see Carmichael, L. (ed.), *Manual of Child Psychology*, 1948.

Towards the close of the war an Industrial Health Research Board was established in Great Britain, with a psychological committee to investigate conditions of work in munition factories; and, when the war was over, Myers gave up his laboratory at Cambridge to found the National Institute of Industrial Psychology in London. The Psychologist to the London County Council was invited to become the first Head of the Vocational Guidance Department in the Institute, holding the two posts in conjunction; and in this way methods that had been worked out for the educational guidance of children were adopted to assist in the vocational guidance of adolescents and adults.

The work of the Institute, however, was not confined solely to vocational problems. The psychology of advertising, of industrial training, of time-and-motion study, of accidents, rest pauses, incentives, and fatigue were all made the subject of systematic and helpful research.[1]

Military Psychology. Soon after the outbreak of the Second World War, elaborate plans were made for applying psychology to military problems; and methods of personnel selection were rapidly developed. Each of the three main fighting services—the Navy, the Army, and the Air Force—had its own Department of Personnel Selection. But the work of the psychologists was by no means limited to this special type of problem.

The vast amount of data collected in this way has as yet been only partly analysed. There can, however, be little question that the results, when fully published, will make an extremely important contribution to the study of the individual mind. Mean-

[1] Cf. Smith, May, *An Introduction to Industrial Psychology*, 1943.

while, the success of such schemes in war-time has given a valuable impulse to the acceptance of psychological methods as one means of solving social problems in time of peace.[1]

Criminal Psychology. Towards the close of the last century the notion that crime was a pathological condition gained wide acceptance, particularly among medical writers. This view was due largely to the work of Lombroso, and was popularized in this country by Havelock Ellis; in the young, habitual crime thus came to be regarded as a symptom of a special form of mental deficiency—moral imbecility or moral deficiency, as it was variously termed; in the adult it was held to be a symptom of moral illness—moral insanity, as older writers had called it. Doubt was first thrown on these conceptions by the investigations carried out, between 1900 and 1913 under Pearson's direction, by Goring and others on adult convicts. Here again McDougall's teaching had a decisive influence. As an anthropologist he contended that the term morality covered a highly complex resultant, varying considerably with the cultural pattern prevailing in the society and more particularly the family in which the child was brought up. These and other arguments seemed finally to dispose of the notion that the " moral sense " is an innate mental faculty.[2]

In this country, however, most of the scientific work on the causes of crime or delinquency has been carried out among children of school age. By the terms of his

[1] Vernon, P. E., and Parry, J. B., *Personnel Selection in the British Forces*, 1949.

[2] The doctrine, however, continued to be accepted by medical writers—*e.g.*, Tredgold, A. F., *Mental Deficiency*, 1908, pp. 122 ff., and Chap. xv (" Moral Deficiency and Criminal Aments ").

appointment it was part of the duty of the London school psychologist to examine and report on cases, not only of intellectual deficiency, but also of alleged moral deficiency. A few small groups of this type had already been investigated both at Oxford and at Liverpool; and the success attending psychological study and training of such cases appeared to demonstrate that moral disabilities, unlike intellectual disabilities, were easily accessible to appropriate measures if dealt with at an early age. At the same time, an examination of the pedigrees of young delinquents showed that criminal tendencies as such were seldom if ever inherited, though an underlying temperamental instability and an excessive strength of certain common instincts might be.[1]

Later surveys indicated that, in point of fact, only an extremely small percentage of juvenile delinquents were suffering from abnormal or pathological conditions. To describe the entire group as morally defective or mentally ill is at once erroneous and misleading. Most instances of youthful crime, so it appeared, were simply normal reactions to abnormal social situations —the consequence of natural instinctive impulses taking non-social forms and tending to harden into habits as a result of repetition and success. Indeed, the most important single factor appeared to consist not in personal or pathological, but in social conditions— namely, the emotional and moral influences surrounding the child, either at home or in the play-groups with

[1] Cf. McDougall, W., *Social Psychology*, Chap. viii. At Oxford, as early as 1907, McDougall and Keatinge (Reader in Education) encouraged teachers to bring occasional cases of delinquency for special study, and this was continued by Burt in Liverpool and London. A convenient bibliography of recent work will be found appended to H. Mannheim's *Juvenile Delinquency in an English Middletown* (1948).

which he spent his leisure hours. But in general the causes of delinquency were found to be multiple, differing in different cases; and it became clear that the treatment should be made to fit not the crime but the individual.

The Psychology of the Psychoneuroses. Down to the closing decade of the nineteenth century, the French schools, under Charcot, Liébault Bernheim, Ribot, Janet, Binet, and their pupils, had taken the lead in psychiatry. About 1890, however, the more objective methods developed by Galton, Wundt, and their followers in the field of normal psychology were introduced into the investigations of abnormal conditions which were started by Kraepelin and Sommer; and Germany thus became the recognized centre of the best psychiatric work. British psychiatry was at that time still dominated by the organic and materialist views held by writers like Mercier and Maudsley. But from 1900 onwards, Rivers, who was then working under Sully at University College and was later joined by Head, began to apply some of Kraepelin's theories and methods; and about the same time, McDougall, influenced both by Kraepelin and by Janet, attempted to direct attention to the psychological causes and nature of functional nervous ailments, and to attack " the inferences drawn from the popular assumption that disease of the mind is disease of the brain." As a natural corollary to his work in other fields, he endeavoured to substitute a " dynamic theory," based on a study of emotional conditions, together with their effects on suggestibility and dissociation. The hypothesis of unconscious forces, and of their mutual conflict and inhibition (concepts largely drawn from the Herbartian school, but re-formulated in the light of

neuro-physiological research), played a large part in his discussion of causation and treatment.

A wealth of material supporting a somewhat similar view was gathered by Freud and his followers; and, as so often happens, it is not the original statement of a new view, but rather its over-statement, that eventually attracts publicity, and often wins the day. To psychiatrists in this country, brought up in the materialistic and intellectualistic tradition of the medical schools of that period, Freud's doctrines seemed not merely new but revolutionary: the majority, indeed, violently rejected them. To psychologists, on the other hand, he seemed to be largely re-stating—in terms of a somewhat out-of-date associationist psychology, supplemented by a set of extravagant and fanciful concepts—many of the essential theories that McDougall and others had long been putting forward.[1]

On the return of Dr. Ernest Jones from Canada, a

[1] In his book on *Psycho-analysis and Social Psychology*, McDougall complains that the medical psychoanalysts had accepted without acknowledgment views that he himself had been preaching. The resemblances were due partly to the fact that both Freud and McDougall had been influenced by very much the same sources (Charcot, Janet, and Kraepelin), and partly to the fact that several of Freud's basic conceptions had been taken, directly or indirectly, from English sources—of course, not without some modification; free association was taken from Galton's work; views on sex, sex-perversions, dreams, and the like from Havelock Ellis; the idea of affinities between dreams and myths had been suggested by Rivers; and many of Freud's somewhat antiquated views on anthropology are to be found in the writings of British anthropologists of the late nineteenth century. It is surprising to find British medical writers supposing that such conceptions were unfamiliar to "academic psychologists" in this country until Freud introduced them. The main objection which British psychologists have advanced against the various psycho-analytic schools is that medical writers have done so little to secure objective evidence for the rather startling doctrines they have put forward: see Sears, R. R., "Survey of Objective Studies of Psycho-analytic Concepts," *Soc. Sci. Res. Council Bulletin*, No. 51, 1943.

British Psycho-Analytic Society was formed; and the new theories were studied and discussed by a small band of younger non-medical psychologists, like J. C. Flugel, Susan Isaacs, and Burt. After the First World War had broken out, the investigation of so-called shell-shock cases suggested the view, at first strongly opposed by the members of the Maudsley school like Mott, that the conditions observed resulted, not from organic disturbance of the central nervous system, due to " shock " in the literal sense, but from " functional " disturbances, which were at bottom essentially emotional disturbances. Not only McDougall, but Myers, Rivers, Bernard Hart, and Pear, devoted much study to such cases; and their work helped to convert later medical psychologists to something approaching a modified psycho-analytic doctrine.

After the war was over, an institution, known as the Tavistock Clinic, was established in London to deal with such disorders in civilian life, and included an active children's department. At first the postgraduate training for those hoping to specialize on child guidance was centred at the London Day Training College (now the Institute of Education). Subsequently the Tavistock Clinic obtained premises adjoining University College, and worked for a time in close co-operation with the Psychological Department there; it was thus able to provide a practical training centre for students taking the Academic Diploma in Psychology as a qualification for such work. Similar institutes, clinics, and out-patient departments were subsequently established, not only in London, but throughout the country; but, as in the case of delinquency, though causes are now better understood, there is as yet little or no convincing evidence as to the value of different forms of treatment.

During the following years the views of Freud, Jung, and Adler became the subject of rival popular cults both among the general public and among medical enthusiasts. For a while the emphasis on the purely mental aspect became so strong that, by a strange paradox, it was not the practising doctors but the academic psychologists who appeared to be noting the importance of physiological and biochemical influences in emotional disorders and in mental backwardness. The final tendency, however, has been to absorb what there was of value in all three schools into the regular teaching of psychology, so that at the moment the leading representatives of British psychiatry are coming more and more to decline adherence to any one school, and to adopt a more eclectic and impartial standpoint.

Conclusion

In spite of the criticisms levelled against it in the early days from philosophers on the one side and physiologists on the other, general psychology appears at last to have established itself as a distinct department of natural science. Applied psychology has already begun to influence our everyday life. Within both branches the conflicts between the different schools now seem to be giving way to a generally agreed body of doctrine. There still remain a large number of problems where prolonged scientific inquiries have so far proved unsuccessful—the study of incentives, the construction of tests for temperamental characteristics having the same value as tests of intelligence, the inheritance of mental characteristics, and the structure of personality as a whole. If the same amount of time, money, and ability is now devoted to psychology as has hitherto been devoted to physical science, still more rapid progress will be achieved.

SOCIAL ANTHROPOLOGY

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SOCIAL anthropology is the youngest of the sciences concerned with the nature of man. It is truly a child of the twentieth century. Though its foundations were laid at the end of the nineteenth century, the men who made an incipient science of it lived to see the First and some of them even the Second World War. It is indeed only in the last thirty years that its methods, problems, and data have become clearly defined.

A survey of the work that is now being done by professional social anthropologists reveals an extraordinary variety of interests. One very active line of research is the study of the relations between personality, as understood by the clinical psychologist, and culture. But we also find social anthropologists making intensive studies of land tenure and economic organization in China, of class structure in an American town, and of legal and political systems in African societies. The sceptic might well ask whether social anthropology is anything more than a jumble of border-line studies in which the concepts and techniques of other disciplines concerned with man and society are applied in a special manner. Nevertheless, there is a distinctive subject-matter, dealt with by distinctive methods, which marks social anthropology off from neighbouring disciplines into whose territory social anthropologists trespass when a particular problem leads them there. This is most clearly exemplified in the characteristic scientific record of the social anthropologist, the field-work monograph.

A field-work monograph is a record of observations

made in a living society during a limited period of time. Its raw material is the actual behaviour of human beings, and this distinguishes it from apparently similar writings of historians, whose source material is the documentary relics of the past.

A field study in social anthropology is distinguished from analogous investigations in allied disciplines by the kind of behaviour recorded, the methods of observation used, and the framework of theory to which these observations are related. Social anthropology is primarily concerned with two aspects of behaviour—custom and social organization. A simple example is the daily sight in the streets of Cambridge during certain periods of the year of a great number of young men and women wearing drab black gowns. This is a fact of custom as well as of social organization. It is an example of a very widespread social institution by which a particular age, sex, occupational, cult, caste or class section of a community is distinguished by an external badge of status, such as some article of clothing or perhaps some pattern of tattooing or other marking on the body. Both custom and social organization are observed in the behaviour of individuals, but they pertain essentially to the collective life of society. Custom is the behaviour that is standardized, expected, and often enforced in a particular situation in the community. Social organization is the system of arrangements by which the social relations of the members of a community with one another are regulated and ordered. Social relations are in part matters of custom, and custom is in certain respects the expression of social organization. The two aspects of human social life in fact overlap, and their relationship is one of the major studies of social anthropology. But custom and social organization

are the means by which human purposes are achieved and human needs satisfied. This brings social anthropology into contact with psychology on the one hand and on the other with the study of the natural environment to which man adapts himself by means of material goods such as tools and weapons.

In theory, social anthropologists have always regarded the whole range of human societies from the rudest pygmy forest dwellers to the most sophisticated denizens of the great modern cities as coming within their scope. In practice, however, they have paid attention chiefly to what are conveniently called primitive societies—*i.e.*, societies which lack the art of writing, have no knowledge of science based on the experimental method, and generally have a non-accumulative subsistence economy. This is partly a matter of tradition, but there are sound objective reasons for it as well. It is easier to observe an alien society dispassionately than one's own, and primitive societies are small in size as well as relatively simple and homogeneous in their economic and social organization. A primitive society can be seen as a whole, and the interconnections between its parts and among its customs can be more easily apprehended than in our complex modern civilization. But it is a basic hypothesis of social anthropology that all human societies are comparable as varieties of the same species, just as all mankind is physically of one species. Comparisons between primitive social life and our own are constantly made by social anthropologists, and some have applied the methods and theories of social anthropology to the study of our own institutions or of selected modern communities in Europe, Asia, and America, as we have remarked.

The foundations of social anthropology were laid

in the last thirty years of the nineteenth century, principally by E. B. Tylor and L. H. Morgan. Tylor's great work, *Primitive Culture* (1871), demonstrated that a "science of culture" based on the comparative method was possible. He insisted that the facts of culture were "within the range of distinct cause and effect" as certainly as any other facts of nature; and he defined culture as "that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society." The controversy about the evolutionary theory was then at its height. Tylor, like most enlightened scholars of his time, found in the theory of evolution a frame of reference which gave order and meaning to the curious and variegated facts about the cultures of exotic races which were then becoming known. Comparing the customs of the "lower tribes" with the civilizations of the "higher nations," he held that the former represented "an early condition of mankind" out of which the latter had gradually evolved along the path of progress. The fundamental uniformity of the human mind postulated by this theory has remained a basic premiss of modern anthropology. The assumption of a unilinear evolutionary sequence for all human social institutions has, however, been abandoned during the past thirty years. It is not susceptible of proof, nor does it help to explain either the features that are common to the various types of human society or those that distinguish one society from another.

Tylor laid down the main lines along which the study of culture—that is, the total social outfit regarded as a body of custom—has developed ever since. Mainly through the work of British and American scholars his evolutionary scheme was gradually transformed into

a more rigorously historical method of analysis applied to limited geographical regions. This is the basis of *ethnology*, the science which studies the historical connections between the customs of different peoples who have influenced one another by contact, and the changes that take place in custom over periods of time. Tylor also perceived that certain aspects of custom can be explained only by psychological principles. Thus he, and following him Frazer and others, resorted to the current psychology of their time to explain the most general features of magical and religious beliefs. They attributed these beliefs to crude logic based on the association of ideas. Anthropologists have continued to follow this trail, guided by the advances taking place in psychology.

Next to Tylor, the most remarkable figure among the pioneers of social anthropology was Lewis H. Morgan, an American lawyer who made a lifelong study of the American Indians. His *Systems of Consanguinity and Affinity* (1871) gave to the world one of the crucial discoveries of anthropology. He made an extensive comparative survey of the customs of designating kinsfolk found among the peoples of the world, and discovered the wide prevalence of what he called the classificatory principle. By this principle a person's collateral kinsfolk are classified with his lineal kinsfolk. Thus the father's brother is not called by a special term—uncle—as among ourselves, but by the same term as that used for the father, and his children are described as brothers and sisters. On the mother's side, her sister is called " mother," and her brother is designated by a special term which is extended to every man who she calls " brother." The rule applies to all relatives, and may be so far extended that all men of the father's generation are called " father,"

all women of the mother's generation " mother," and all men and women of one's own generation " brothers" and " sisters."

Morgan's work brought to light also the widespread occurrence of the custom he first found among the Iroquois, by which descent is reckoned through the mother and her female ascendants, not through the father as in our civilization. In his most famous work, *Ancient Society*, published in 1878, he married the results of his studies to the evolutionary theory and produced a bold conjectural scheme of the stages through which human society had passed in its alleged evolution from primitive savagery to its highest development in modern Christian civilization. He took over a theory put forward by a Swiss scholar, J. J. Bachofen, in his book *Das Mutterecht* (1866), to the effect that in the earliest period of human history the matriarchal family was the rule. Morgan maintained that, after a primordial stage of promiscuous mating, the first form of the human family was the marriage of a group of brothers with a group of sisters. As paternity could not be established, kinship and descent came inevitably to be reckoned through the mother. Other forms of marriage succeeded until, with the advent of the institution of private property, the patriarchal and later the monogamian family came into being. He believed that classificatory kinship was a survival and proof of earlier forms of marriage. He maintained that there was a parallel evolution in forms of government, which, he believed, was originally based on the genealogical connections of persons in the *gens* and the tribe ; with advancing civilization it was transformed into truly political forms based on territorial and property relations. Morgan took this theory largely from Sir Henry Maine's book *Ancient Law*

(1861), and it is only within the last decade that the division into two types of political society implied in this argument has been shown to be untenable.

Morgan's fantastic theory of the evolution of the family has been cast aside during the past thirty years (except in the U.S.S.R.) as a result of accumulated field observations. It has been shown that some form of recognized conjugal union is found in all human societies, and must have been present from the beginning of human social life. Indeed, the elementary family may be regarded as a biological necessity for human survival in view of the long infancy of man. Thus Morgan's greatest contribution was not these theories but the preliminary elucidation he gave of the major problems of social organization in primitive society. The comparative study of kinship and family systems is the basic subject-matter of the study of primitive social structure; and Morgan was the first to show that these institutions have to be considered in relation to the whole social system of a people, and not in isolation as examples of the perversity of savage customs.

The evolutionary theory held the field till well into the present century as the main frame of reference of a galaxy of anthropological scholars—men like Frazer, Robertson Smith, Westermarck, and others. In the long run, however, the greatest service of these scholars has proved to be their thorough sifting and collation of all the information then available about primitive social life, which is so well exemplified in Frazer's work. The information was taken from the writings of explorers, missionaries, and others who had visited primitive societies. The division that existed between the observers who supplied the data and the arm-chair scholars who made the theories goes far to explain the

unscientific speculations and controversies of the period. The unsystematic and often biased character of the information encouraged the indiscriminate comparison of customs isolated from the total context of social life.

A change which determined the whole course of anthropological studies in the twentieth century took place at the turn of the century. In 1883 Franz Boas, a young German scientist, made a geographical expedition to Baffin Land. He became interested in the social life of the Eskimo of that region. Further expeditions to the north-west coast of America under the auspices of the British Association for the Advancement of Science followed, and by the end of the century Boas, now teaching in America, had established the method of systematic field work by professional anthropologists as the basis of American anthropology. In 1886, A. C. Haddon, an English zoologist, visited the Torres Straits on a zoological expedition. He took the opportunity of collecting ethnological information from the natives of the area, and soon turned to ethnology as his main study. In 1898 he organized the famous Cambridge Expedition to the Torres Straits. He was accompanied by W. H. R. Rivers, already eminent as a psychologist and neurologist, and C. G. Seligman, a leading young pathologist, as well as by C. S. Myers and W. McDougall, both of whom later took their place among the foremost psychologists of this century. For the first time in the history of anthropology a group of highly qualified natural scientists went out to make a study of primitive peoples in their own habitat. Within the next ten years Haddon, Rivers, and Seligman had laid the foundations of scientific field work in this country.

By 1910 a new procedure, modelled on that of the natural sciences, had become established in the

anthropological sciences. Whether the problem was the establishment of historical connections between and within cultures—*i.e.*, ethnology—or the analysis of how primitive social systems work—social anthropology—it was accepted that the data had to be obtained by scientific observation in the field on the part of those who made the generalizations. The test of an hypothesis was in the field, not in the study; and the scientific monograph of today is the direct outcome of this movement.

It is of interest that the influence of Boas and his pupils and followers, both in America and on the continent of Europe, was in the direction of ethnological studies, whereas the British tendency was towards social anthropology. This owed much to a theoretical impetus from France. There E. Durkheim and his colleagues were developing a method and theory of comparative sociology in which great use was made of information obtained from primitive societies. Their method was to regard a society as a working whole at a given time, in which the customs and institutions of the people fulfilled social needs or expressed conditions affecting the whole social system; and they stressed especially the collective ideas and sentiments manifested in custom and the part played by these in maintaining the solidarity of a society. The most famous application of this theory was Durkheim's study of Australian totemism (1912), a subject much discussed then because of the prevailing view that the Australians were representative of a very early and primitive type of human society.

Durkheim's theories, and especially his psychological assumptions, provoked vigorous criticism. But his insistence on seeking the meaning of customs and institutions in the part they play in maintaining the

integration of society had a profound influence. Combined with the new approach to field work, it culminated in researches which set the standard for all contemporary research. These were the field studies of Rivers, B. Malinowski, C. G. and B. Z. Seligman, and A. R. Radcliffe-Brown in this country, and of R. Thurnwald in Germany, which were carried out during the first two decades of this century.

The outcome of this work was the functional theory of culture and social organization which, with various modifications and extensions, has inspired the most important researches of the past thirty years. The stress of these researches has been on the intensive study of one society at a time with the aim of eventually reaching scientific generalizations by comparing, not isolated customs and institutions, but systems of organization and principles underlying social usages.

Functional anthropology is based on the postulate that the customs and institutions of a living society are all interdependent and, in a simple and homogeneous society, constitute a relatively integrated system. Every custom and institution, whatever its history, has a functional value in relation to the whole system—*i.e.*, it is a means to the achievement of personal and collective ends within the limits set by environment, the accidents of history, the biological constitution of man, and the necessity of maintaining an ordered social life. It follows that custom can only be fully understood in terms of the activities and relations of individuals and groups. The full exploration of this connection has led to the development, notably in the United States, of a new branch of social psychology, the study of basic personality structure.

Two complementary directions of research have emerged in the work of functional anthropologists.

Malinowski laid special emphasis on the significance of culture as a unique human phenomenon, the basic function of which is the satisfaction of biologically determined needs. Universal biological needs, he maintained, constitute the nucleus of universal complexes of customs and institutions. Thus the need for food underlies the system of production, exchange, and consumption, with all the elaboration of customs and institutions that go with these processes, which together constitute the economic aspect of culture. In the same way it is the need for reproduction that underlies the institution of the family, and this is the source of the kinship systems first described by Morgan. Knowledge, myth, religion, and art arise out of secondary needs, derived from the conditions of human existence. Instead of an evolutionary connection between "low" cultures and "high" cultures, a common morphological plan is postulated, and this is held to be the most important subject for anthropological study.

The researches associated with this approach have taken the form of intensive studies of particular complexes of institutions in relation to a people's total culture. Malinowski's own most famous work, *The Argonauts of the Western Pacific* (1922), is a study of a remarkable custom of circulating valuables of no economic use for the sake of prestige in a group of Melanesian islands. He showed that this brought about the exchange of essential economic goods and entailed great skill in seamanship as well as an elaborate mythology and special magical beliefs and practices. Malinowski proved that magic was not a form of bogus science, as Fraser had argued, but a means of assuring optimism and confidence in the face of natural and technical elements which lie outside the bounds of

practical control. Magic is not expected to achieve technical results. These are gained by the application of empirical knowledge based on a correct appreciation of causal relations that are never confused with magic.

This analysis was further developed in some of Malinowski's later works and in the field researches of Firth, Fortune, Evans-Pritchard, and others. These researches disposed of the widely disseminated theory of Lévy-Bruhl, who held that primitive peoples have a prelogical mode of thought. By this he meant that they were incapable of grasping the principles of natural causation and rational connection. Instead, he maintained, they hold beliefs in mystical causation and in connection through the " participation " of objects, persons, and events in one another. This theory was subjected to the test of field observation in a notable study of Azande witchcraft and magic by Evans-Pritchard. In this he demonstrates that, given certain premisses which cannot be disproved by the methods of observation of nature and of human life available in primitive cultures, the rites and beliefs of magic and witchcraft follow logically and make up a coherent, internally consistent system of thought, functionally related to the strains and stresses of social life. The key to magico-religious beliefs and practices lies in the fact that they are culturally determined means of regulating moral relationships and inter-personal adjustments. They also serve as a means of co-ordinating economic activities and as sanctions of political institutions, as Malinowski contended, and as Firth has shown in his field studies in Polynesia and Richards in her investigations in Central Africa.

Another important line of research inspired by Malinowski was the study of primitive economic

organization, notable contributions to which have been made by Firth, Richards, and Forde. This work has shown that the alleged communism of primitive societies, accepted even by so great a scholar as Rivers, is a fiction. It has shown also that rational calculation in the use of scarce resources, adjustments to supply and demand, and the other modes of economic behaviour developed to so high a pitch in Western capitalist countries are found, in principle, in the pre-capitalist economies of primitive societies. But economic relations and activities are not depersonalized as in our society. They are intrinsically bound up with kinship and political institutions and more obviously directed by cultural values, magical and religious beliefs, and other non-economic elements than with us. Such widespread institutions as gift-exchange, the ostentatious destruction of surplus food or other goods, and the economic privileges of chiefs, have been shown to have both an economic value as a means of ensuring the provision of costly capital equipment, and a social value as institutions which maintain a relative equality of consumption over a stretch of time in these societies.

The development of dynamic psychology by McDougall, Shand, and especially Freud, aroused a lively interest among anthropologists in the late nineteen-twenties. Seligman showed that typical Freudian dream symbols occurred in the dreams of most races. Starting from the observation that among the matrilineal Trobrianders it was the mother's brother and not the father who was the harsh and hated-source of authority in the family, Malinowski claimed that the Freudian Œdipus complex was a product of the peculiar family structure of Western civilization. In the ensuing controversy between him and the well-

known psycho-analyst Dr. Ernest Jones, honours were evenly divided. But from this and parallel controversies in America sprang a most fruitful series of field studies. Those of Margaret Mead in Samoa, Melanesia and Bali are very well known. Though her work has been criticized by more pedestrian colleagues for its impressionistic manner, she has described, with an insight not easily matched in the literature, customary attitudes and behaviour patterns in primitive societies which closely resemble what are commonly regarded as idiosyncracies or temperamental dispositions in our society. Her work has thus afforded striking proof of the determination of attitudes connected with sex, parenthood, and most social roles by the values specific to a culture. In seeking to understand how culture determines attitudes and dispositions, Margaret Mead, Bateson, Kluckhohn, and other American students have turned their attention to the comparative analysis of child-rearing in different societies. Drawing largely on psycho-analytic theories for guidance, they have begun to accumulate observational material which shows that the emotional attitudes involved in the customary beliefs and activities of any society are laid down in the course of child-training during the first years of infancy. An impressive attempt to construct a systematic theory to show how custom, social organization, and the genetic development of the normal adult personality are inter-related is that of Kardiner and Linton.

These researches, for all their apparent diversity, have one interest in common. They are all concerned with the phenomena of culture viewed as a coherent body of custom transmitted from generation to generation within a given society. The postulate, which followed mainly from the work of Radcliffe-

Brown and Malinowski, that, as the former put it, " any persisting culture [is] . . . an integrated unity or system, in which each element has a definite function in relation to the whole," was carried to an extreme conclusion by Ruth Benedict. Taking a cue from the new psychology on the one side, and from the German philosophers of history on the other, she developed a theory stressing the unique qualities of each culture and proposing a typology of cultures. Her types are constructed by attributing to a culture a wholeness analogous to that of a personality as seen by the psychologist and then reading into it a pattern which is evaluated as the cultural representation of a temperamental or personality type. Dr. Benedict's rare literary talent and intuitive virtuosity have given her ideas a considerable vogue with the lay public and a strong influence among American students of the relationship between culture and personality. This movement is, however, becoming increasingly literary and historical. It has not stimulated intensive field work or yielded hypotheses capable of being tested by field observation.

The complementary tendency has been concerned with the study of social organization. Its basis has been the study of primitive family and kinship systems. Kinship, as Morgan argued, underlies political organization in the simplest societies, and this in turn is the framework of legal institutions. Law, however, rests in the last resort on moral premisses which, in primitive society, are maintained to a very large extent by religious sanctions. The study of primitive social organization has thus inevitably included this whole range of subjects and their interconnections. Scholars of all nations have contributed to it, but the most important results have been due to the inspiration of

W. H. R. Rivers and his first pupil, A. R. Radcliffe-Brown.

Radcliffe-Brown's theoretical frame of reference is largely derived from Durkheim. His field work began in Australia in 1905, when he made the significant discovery that the conjugal family is as vital to Australian social organization as the much-discussed division into clans and marriage classes. He showed also that classificatory kinship is based on a precise gradation of ties of sentiment as well as of clearly defined rights and duties in accordance with the exact genealogical distance between persons.

In the course of the next thirty years Radcliffe-Brown and his pupils developed a systematic method for analysing kinship systems, and a theory which reduced the bewildering variety observed in kinship systems to the combinations of a limited number of general principles. These principles have been shown to hold for Australia by Radcliffe-Brown himself, Warner, and others; for Polynesia by Firth; for Melanesia by Margaret Mead; for the American Indians by Eggan and his colleagues; and for African societies by myself, Evans-Pritchard, Forde, and others. Thus it is a general principle in all kinship systems that children of the same parents are legally and socially identified with one another, and this explains many rules governing the relations of kinsfolk. It underlies the very widespread occurrence of unilineal descent groups in which rights of inheritance and succession are commonly vested and which form the basis of clan organization. Recent studies of the structure and functions of these descent groups have thrown new light on the nature of primitive political and legal institutions. It has long been known, chiefly from the work of Frazer, that sacred insignia, and taboos which

associate the well-being of rulers with the well-being of the society, are common features of primitive political organization. It is becoming clear that these are connected with the existence of marked fissiparous tendencies in most primitive societies, often arising out of the latent opposition between unilineal descent groups. The sacred sanctions, which stand above mundane affairs, affirm the common interests of all the groups and factions in a society and so act as a unifying force. It has also been established that primitive political systems fall into three main types: those in which political and legal institutions are co-terminous with kinship institutions; those in which they appear as a machinery of adjustment between unilineal descent groups; and those in which they are vested in a recognized political authority with supreme power in a defined territory.

Among many other problems which these studies have brought nearer to solution is the perennial and controversial problem of incest. The range of prohibited kin has been shown to be determined not by biological connection or by psychological attachments due to familiarity or to unconscious motives, as writers like Westermarck and Freud have argued, but by social regulation. Close and familiar blood relatives may be preferred mates where fictitious clansfolk are prohibited. But there appears to be a general principle that prohibited degrees are laid down in such a way as to ban sexual relations which might conflict with the right and duty of parents and their coevals to exercise discipline over the generation of their children.

The problem of incest is closely connected with that of exogamy, the rule which prohibits marriage within a defined genealogical or social group, and with its converse, the rule of endogamy. Ethnographic informa-

tion from Australia and North America suggested the idea that the rule of exogamy was rooted in totemic beliefs. In their simplest form these beliefs and the ritual ceremonies in which they are expressed claim that there is a bond of kinship between species of animals, plants, and other natural phenomena, on the one hand, and social groups like clans on the other, with the corollary that the latter regard their totems as sacred or at least show them respect by abstaining from eating or injuring them. Speculative hypotheses couched in the usual evolutionary terms were put forward to explain this association, notably by Frazer (1910), who attributed it first to magic aimed at ensuring a plentiful food supply and security from dangers, and later to savage ignorance of the male role in conception. The ensuing controversies came to a head in Durkheim's famous book, *The Elementary Forms of the Religious Life* (1912). Rejecting the evolutionary and intellectualist theories of Frazer and others, Durkheim argued that Australian totemism was a rudimentary form of religion. He claimed that it consisted of a coherent body of cosmological beliefs which served to organize the relations of human social groups to their environment and at the same time acted as a symbolic affirmation of the collective moral sentiments upon the common acceptance of which the integration of the society rested. Totemism was thus placed in the framework of a general theory which linked primitive religion with the social structure and the natural environment.

This theory was applied by Radcliffe-Brown to field observations he had made in the Andaman Islands (1922). He developed the hypothesis that ritual observances of a totemic kind are symbolic means of investing with social value objects (such as food

sources), occasions (such as seasonal changes), critical stages of life (such as puberty), and general cosmological ideas (such as fertility) upon which the common interests of a community are focused. This theory, which helped to bring the study of ritual ideas and practices down from the cloudy altitudes of psychological speculation to concrete matters of daily bread, has stimulated valuable field investigations in Australia, America, and Africa. French writers have followed Durkheim more closely. Griaule and his co-workers, for example, seek to discover what might be termed the metaphysical ideas implicit in the religious and magical beliefs of primitive peoples. They have shown that these beliefs imply quite elaborate conceptions of the nature of human personality, human origins, and the universe at large. These researches have shown that there is much more in common between all systems of religion, both " high " and " low," than nineteenth-century scholars believed. The notion of an evolutionary sequence culminating in Christianity is not justified in the light of recent research.

In this province of research there have also been advances in our understanding of primitive legal and moral institutions. Malinowski and Radcliffe-Brown led the way by discovering that a system of law, of however rudimentary a kind, is found in every society. Most societies recognize certain actions as public wrongs for which punishment is meted out by the community as a whole, and others as private wrongs for which redress is sought by the wronged party on his own initiative. But public wrongs often include what we should call sin and sacrilege, whereas homicide and other acts which we regard as crimes are often regarded as private wrongs, to be settled between the kin groups of the principal parties. The work of Rattray, Meek,

and Schapera has brought to light highly developed systems of civil law concerned with marriage, movable and immovable property, and personal status in Africa. In Indonesia, too, Dutch scholars have recorded and analysed highly developed native legal systems.

The rapid spread of Western civilization throughout the world during the past fifty years has meant that anthropological field studies have to be carried out to an increasing extent among peoples whose way of life has been affected by European contact. The effects of European culture contact on the social life of primitive peoples were first dramatically brought out by Rivers (1912) in an essay describing the depopulation of parts of Melanesia. He attributed this in part to the breakdown of the indigenous cultures and the consequent psychological apathy of the natives. Much further investigation of this problem has since occurred, and it now appears that physical and economic factors, such as introduced diseases and plantation labour, play a larger part in the decline of primitive populations than Rivers thought. Nevertheless his hypothesis has not been invalidated altogether. Where a period of population decline has been followed by a rapid upward trend, as among the Maori, this has been shown to be associated with a cultural renascence.

The study of culture contact has now been extended over the whole range of custom and social organization. A large number of field studies have been made in Africa, Oceania, Indonesia, and America. In all these areas it has been shown that culture contact generally results in a breakdown of native morality and a deterioration of domestic life. This happens in spite of rising standards of economic well-being; and the spread of Christianity and education through the efforts of missionaries is no safeguard. At the same

time, as Schapera has found in South Africa and other investigators have confirmed from every part of the world, certain indigenous institutions and customs are tenaciously preserved even in the most adverse conditions of culture contact. Indigenous kinship systems and beliefs in magic and sorcery appear to have a specially strong hold on non-European peoples. This can perhaps be correlated with the fact that the economic and emotional insecurity of the individual is greatly increased by the changes due to culture contact. This leads to the retention and sometimes even elaboration of institutions which in the past provided security for the individual and some measure of stability for the society. The many problems raised by these students are, however, only beginning to be investigated.

Social anthropology is now at a stage of very rapid growth. The methods of field work and the theoretical approach developed in the past thirty years are already being applied in America by Warner and others to the study of Euro-American civilization, and by Chinese and Indian and African scholars in the study of their own societies. This expansion of scope has been accompanied by great advances in field technique as well as in the working hypotheses of the subject. It is, indeed, quite possible that three separate but interdependent disciplines may emerge from the research work now in progress: a discipline devoted to the historical and comparative study of culture viewed as a body of coherent custom; a discipline concerned with the analytic study of social organization; and a discipline seeking to elucidate the data of culture and of social organization in the light of the generalizations of psychology and psycho-analysis.

The simple comparative procedures of the nine-

teenth-century founders of anthropology have thus been replaced by intensive analytical methods based on direct observation. But it must be admitted that this great advance in the use of scientific method in social anthropology has been accompanied by a serious philosophical loss. The concept of evolution had a grandeur and sweep which gave a universal setting to the facts discovered by anthropologists. No frame of reference of equal philosophical magnitude and generality has been found to replace it in our thinking about human social life. To do so is one of the major tasks ahead.

If this is to be accomplished it is necessary to understand clearly why the idea of social evolution, as the nineteenth-century writers used it, is not now accepted by social anthropologists. It was far too sweeping a conception to apply to the facts of human social life. The nineteenth-century writers failed to realize how brief was the span of human existence in comparison with that of geological time in which the processes of biological evolution took place. They did not distinguish clearly enough between biological evolution and historical development. Biological evolution culminated in *Homo Sapiens*, who is qualitatively distinguished from all other living species by the fact that he lives in organized social groups which survive by means of the social outfit which Tylor called " culture." It is the possession of such cultural products as tools, language, and moral rules that marks off man as a unique species. This fact, the result no doubt of the anatomical and physiological features distinctive of man, may well be regarded as the outcome of biological evolution. But with the establishment of modern man on the earth there began the processes of historical development; and these belong to a different order

of natural events from those of biological evolution. They involve changes in culture and in social organization only, not changes in the biological heredity or even the basic social capacities of man. They are not comparable in kind or degree to the changes exemplified in the fossil record, even over the relatively short period during which man and his hominid precursors have been in existence. The progressive development of technology and knowledge by some branches of the human race is due not to natural selection or some other biological cause but to invention and discovery.

But the main reason why the concept of evolution in the biological sense has been found to be out of place in the scientific work of social anthropologists lies in the facts of observation, not in philosophical or methodological considerations. The facts show that historical development in culture and social organization does not necessarily and invariably follow a uniform or even a progressive line. This is particularly the case with the more complex elements of human culture such as language, art, and religious beliefs. The customs and beliefs of extant primitive peoples are not, as Tylor imagined, examples of earlier stages of the customs and beliefs of more advanced societies ; nor is there evidence of forces which change the former into the latter. Magic, religion, and science represent not, as Frazer argued, different stages in the progressive development of human thought, but different components of a mixture which turns up in all societies ; and this is true also of many other facts of human social life. As to the conjectures about the very remote past of human social organization and culture indulged in by Morgan, Frazer, and their contemporaries, these appear simply nonsensical in the light of present field observations. " Group marriage " and the " primal horde " are sheer

fantasies without the slightest demonstrable basis in the realities of human social life. It is the uniformities behind the overt diversities of human culture and social organization that excite the interest of anthropological science today. It is the conclusion that historical development takes unexpected courses, often in directions that would be called retrogressive by nineteenth-century writers, that challenges the assumptions of the uncritical evolutionists.

The new frame of reference for anthropological science will have to be worked out on the model of the experimental natural sciences, not on the evolutionary speculations of the founders of anthropology. It must provide hypotheses that can be tested by direct observation of living human societies. By comparing these observations with what can be discovered about extinct societies by archæological research and about societies with a documented history by historians, it may be possible in time to establish the laws of change and development that regulate the growth of human social institutions.

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SOCIOLOGY

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I

AT the end of the seventeenth century Thomas Burnet wrote, " *There is nothing secret that shall not be brought to Light*, within the compass of *Our World*. . . . All I say, betwixt the first Chaos and the last Completion of Time and all things temporary, this was given to the disquisitions of men ; in either hand is Eternity, before the world and after, which is without our reach : But that little spot of ground which lies betwixt those two Great Oceans, this we are to cultivate, this we are masters of." (1) The diligent application of the dry light of science to the immediate world of nature has since proceeded apace, the oceans of ignorance and eternity receded, mystery and the numinous were driven to the confines of the negligible and unknowable, and only one area of all nature, that where the theologian and mystic believed the eternal to intersect with the natural order, remained secret. That *terra incognita* was the world of man.

Now, however, that dark continent is entered. The psychologists have broached the privacies of the individual self, the anthropologists brought back a new learning from the diversities of human society, and the sociologists pioneered the understanding of the totality of human interaction. By 1900 the frontiers of society in time and space were tolerably well known. Not merely were the inhabitants of the earth all brought within the view of anthropology and human geography, but the long battle of archæology against obscurantism

and scepticism had been won, the vast age of fossil man accepted, and the triumphs of Schliemann on the site of Troy had revealed societies unknown to the Old Testament or the Greek historians. In 1900 Evans began work at Knossos and exposed the unrecorded civilization of Crete. Scientific archæology was vindicated as the gateway to the oldest history of man, and the values of every artefact, however repulsive or trivial, was recognized in its documentation of a specific culture.

And the maxim that the sciences of man should neglect nothing was taught also by anthropology. In 1898–99 A. C. Haddon had led to the Torres Straits an expedition, whose members included Rivers, Seligman, McDougall, C. S. Myers, and others, who were greatly to further the study of man. With their meticulous work the era of field anthropology, which still continues, was opened. Psychology too, through the work of Freud, was about to react to a new impulse, while on its experimental and metric side tools were being forged of the greatest value to all social research.

Against this background the advance of sociology, concerning itself with the scientific appraisal of all human associations, seemed assured. In what follows it will be suggested that, to a considerable degree, that assurance has not been disappointed.

II

The difficulties which confront the sociologist, whether engaged in the analysis of a particular institution or in the problematic business of synthesis, are often very different from those which confront the natural scientist. Human society is a constant flux in which permanencies, recurrences, and such isolates

as the physicist studies are of rare occurrence. Human groups are usually smaller than the atomic or molecular populations with which physicists and chemists concern themselves, and in consequence their statistical regularities are harder to detect and less consistent—altogether apart from the fact that the members of human groups are psychologically complex individuals whose range of potential behaviour is extremely great. Nor is the test by successful prediction, so widely accepted in natural science, easily available to the sociologist for the verification of his theories.

For example, the alleged scientific prediction of Marxism, that through its appropriate politico-economic organizations the proletariat will succeed to the political, economic, and social hegemony of the world, may well come true. This, however, would not necessarily prove the correctness of Marxist sociology, any more than the triumphs of Islam " proved " the truth of Mahommedan theology, for sociological theories can function independent of their truth or falsity as political ideologies, and succeed *as such*. Again, the sociological prediction of an increase of juvenile crime in a given area may be confirmed not by such an increase, but by the very expectation stimulating the vigilance and zeal of the police in making arrests. Prediction can also deceive in another way: as in daily life many social predictions are of the order of, " if nothing is done about X then Y will happen." Very frequently something *is* done, either by awareness of the prophecy changing people's attitudes or by conscious policy decisions. The accurate appraisal of the value of the prediction is then rendered extremely complex.

The immediate relation of sociology to public policy

is also something with which only a limited number of natural scientists have had comparably to concern themselves.[1] But the sociologist has always been aware that social (and/or governmental) need oriented much of his research—sometimes illegitimately—and also that social research can lead to unpopularity and disapproval by offending widespread moral presuppositions, by revealing scandal which it is the advantage of powerful interests to hide, or, most subtle of all, by proving intensely held Utopian hopes illusory, and hence arousing a public anger.(2) Nor are public bodies much aware of the needs of "pure" and long-term research. The Listener Research Department of the B.B.C., the Government Social Survey begun in 1941, do useful work, but they are largely parasitic to techniques developed elsewhere. The whole area of marketing research carried on by, and for, advertising interest is even more clearly parasitic, while its social value is as small as its income is great. The needs of an employer may produce unconscious bias in the researchers; though sociology has learned much from what lies behind the pretentious language of, *e.g.*, industrial sociology and psychology,(3) it is difficult to avoid the impression that the illegitimacy of pecuniary aspiration on the part of the employed is a tacit major premiss of such work.

And, as the sociologist is himself a member of society, it may reasonably be questioned whether he can avoid serious bias, usually below the level of consciousness, in his work. It is frequently pointed out that social theory and research differ from country to country in their content, and this is taken as evidence of a geographically varying subjectivity. Perhaps it

[1] This is not to say that moral and social issues *should not* concern the natural scientists. I believe they should.

is so; one distinct branch of social science, the Sociology of Knowledge (*v. infra*), deals *inter alia* with this social conditioning of thought. Nevertheless, by being aware of the problem such error can largely be avoided, and the attempt to avoid it is surely better than leaving social relations to ignorance and prejudice. An overt problem is already half-solved.

Nor is the divergence significant; there is so much to examine that many different approaches, stimulating and challenging each other, are necessary and valuable. And while some sociological theories are wrong, their errors, as has so often happened and *must* happen in all science, are instructive and can show the way to new problems and concepts. Far more serious than the existence of national schools in sociology is the borrowing of the name to defend a creed with the prestige of a science in such usages as Christian, or Catholic, or Islamic Sociology. Good work has of course been done by sociologists of various religions, even in the field of religious sociology,[4] but much that calls itself Christian Sociology is as scientific as the claims of a "Radio-active Hair Tonic." Politics may also dictate a bias, as under totalitarianism, but all science everywhere is increasingly less immune from such corruptions.[1]

One consequence of all this is the tendency of some sociologists to avoid dispute or attention by studying only trivial or marginal topics. This has especially been an American failing. Another way out is the study of deviant or marginal groups (delinquents, minor

[1] Sociological thought in the U.S.S.R., where this tendency is most developed, seems to manifest itself mainly in re-statements of Marxism—of which N. Bukharin's (executed 1938) are the last of any interest [5]—and in socio-historical studies, often of a high order. No doubt attitude research is conducted, though unfortunately not published.

ethnic enclaves, etc.) or the study of those who cannot answer back (the unemployed, the poor, and so on). Such studies are often valuable, but their range is by definition not exhaustive.

Unfortunately this timorousness has sometimes resulted in the belief that general ideas and large-scale theoretical models are in themselves vicious and to be avoided.

The sociologist, however, has one great advantage; concerned to understand human society, there is open to him understanding in terms of his own humanity. The methodological importance of this was first made clear by the German, W. Dilthey. This " sympathetic intuition," as Ginsberg calls it, was correctly appraised by Max Weber, whose " *verstehende Soziologie* " turns on the need to validate by methods of the same order as in the natural science the " insights " obtained by sympathetic reconstruction and common human understanding.

III

The science of statistics, one of the principal tools of sociological proof, had by 1900 reached a stage where it could satisfy all the needs of an infant sociology. Karl Pearson, building on foundations earlier laid by Gauss, Poisson, and Quetelet, and closely linked with Galton, had advanced the calculus of probability, established the convenience of the normal distribution curve, and given Galton's study of correlation its expression in the correlation coefficient. The Royal Statistical Society, in existence from 1834, had already seen much social statistics. Tedious calculation had been mechanically relieved since the first Hollerith machines had been used in Austria and the U.S. census

of 1890. The technique of sampling—enormously economic of effort, time, and money—had been known in social research since Eden in 1800 had forecast the result of the 1801 census. In 1906 A. L. Bowley advocated the renewal of the practice, with the area of error under accurate control.[6]

Population studies, closely allied with the biometric studies of Galton and Pearson, progressed in the nineteenth century. The fact of a declining birth-rate was clear by the 1890s, and the biometricians had long agitated public attention with the threat to human quality inherent in the differential birth-rate of different social classes. In 1883 Galton founded the new science of Eugenics to combat these alleged ill effects. In France, A. Dumont argued that the combination of great social mobility with widespread social aspiration was inimical to fecundity, while the work of Newsholme and Stevenson convinced other English writers that grave economic and political problems would accompany what had been proved to be a genuine decline in reproduction.[7]

Did population, as Carr-Saunders suggested, perhaps tend to approach an optimum; was there a biologically inherent pattern of population growth—the "logistic curve"—as Pearl contended?[8] These were the demographic problems of the 1920s. The advent of Fascism, strongly opposed to contraception and demanding fecundity as a private and public virtue, along with Kuczynski's device of the Net Reproduction Rate, which made possible detailed—and gloomy—forecasts of population in terms of given trends, shifted demographic interest from the biological to the social factors in population movement, and to the question how, by policy, these movements could be changed.[9] The threat of a declining and ageing popu-

lation drove even the British Government, which had not maintained the level of information obtainable on fertility in the 1911 census, into a Royal Commission on Population, whose report[10] confirmed existing sociological and demographic opinion on Britain's ageing population, perhaps soon absolutely to decline.

Demographic studies are skeletal, and the flesh on the bones of society has been studied primarily in social surveys. What is still the greatest of English surveys, Booth's study of the London poor, appeared in its definitive form in 1902.[11] It was the result of careful team work and use of officials: it was also political dynamite. Sampling methods were established in this field by Bowley at Reading, and the series of such works in Britain has continued in York, Tyneside, Becontree, Kinlochleven, and many other places. The fact that both London and York were later re-surveyed was especially valuable as establishing the social survey in time as a documentation of social change.[12]

Partial surveys of specific groups are also numerous, especially in America since Thrasher's *The Gang*,[13] while the two famous volumes of *Middletown* (1929) and *Middletown Revisited* (1937) by R. S. and H. M. Lynd have added, by the wholeness of their view and their concern with *all* the life of their area of study, something new to sociology. More recent American studies have tended to concentrate on limited sectors of life such as status and class, or on child-rearing habits whose reflection is supposed to be found in typical adult behaviour.[14] The fulfilment of the dream of the pioneer Scots sociologist, Patrick Geddes, whereby a satisfactory theory of urban society and its dynamics would be attained, is—despite all this busy, ingenious, and important work—still unfulfilled.

The professional sociologist's interest in class, status, and social mobility is comparatively recent. The study of mobility begun by Ginsberg [15] was little followed up in England, though research in this field has been resumed at the London School of Economics. In America ingenious objective scales of status have been developed.[16] What is needed is a re-thinking of the concepts of class and status, and the re-statement of what is living in the Marxist theory of class and class dynamics.[17]

Such investigations have been facilitated by the application of sampling and interviewing techniques to research into attitudes. This has its commercial side in the political polls and in market research—both, the writer believes, liable to abuse, and the former often justified by a false theory of democracy as being, ideally, a perpetual referendum.[18] Not dissimilar in aim, but vastly so in method, have been some attempts to introduce anthropological methods to the study of civilized societies.

In 1937 Mass Observation was founded in England for this purpose, but its methods were never satisfactorily established or formalized for scientific appraisal, and its puppy-like sniffing at all the world produced little of permanent value.[19] (Far more interesting and modest non-quantitative work has been that of F. Zweig in London and among coal-miners.[20]) But anthropology has had other influences, more profound than those of Mass Observation, on sociology.

The field work of B. Malinowski was in the Haddon tradition (*cf. supra*), but gave rise to a number of sociological concepts which could be applied far beyond the boundaries of primitive society and which coincided with a parallel development in American anthro-

pology. We may refer to these as Functionalism [1] and the Culture Concept.

In a pre-literate society social explanation in terms of historical change and growth is difficult and often impossible. Sometimes, as with the Maori or in parts of West Africa, historical reconstruction can be attempted, but this is rarely possible. Explanation must often, it follows, be in terms of the mutual dependence of institutions, the fulfilment of needs by institutions, and the existence of institutions as being explicable only in terms of their present operation, the society studied being taken as an organically related and self-subsistent system of parts. Such a view has much in common with the political teaching of Burke and organic theories of the State, and, as everything turns on the direct or indirect fulfilment of social need and simultaneous causal interdependence, can accurately enough be described as a theory of social functionalism.

This theory excises time from sociology and tends to reinforce conservatism in ideology. The excision of time, and hence of any true study of social change, may be necessitated in anthropology by the absence of records; it is not justified in the study of literate societies, and it carries the reaction against such sociologists as Spencer, who interpreted all social life in evolutionary terms, much too far.(21)

The culture concept is difficult clearly to define; in brief it is the view that a society is by its nature so unitary that the abstraction of any institution does a fatal violence to comprehension, and that each cultural configuration so permeates all behaviour in a society as to shape and dominate all action.(22) If this view

[1] Behind functionalism was the social theory of Durkheim (*v. infra*), who also greatly influenced the anthropology of Radcliffe-Brown.

is accepted the comparative study of institutions is possible, but trivial: the real unit of study is culture as the totality of social inheritance in a society. The writer believes that the attitude provides a valuable tool of simplification, but no more, and that the role of functionalism and the culture concept in sociology has been to distract attention from major theoretical issues and comparative researches, and to impose a false rigidity on the diversity of material, especially in the much-discussed field of the relations between culture and personality, and the relevance of Freudianism to social science.

W. H. R. Rivers was probably the first non-Freudian writer to perceive the significance of psycho-analytic psychology for sociology.[23] The direct relevance of Freudianism was limited,[24] but the thesis that the cultural elements are dominant in individual personality, and that they are implanted in the individual by treatment in the earliest months and years of life, and by the attitudes which he has to this treatment and those who administer it during those years, is now widely used in research. That this procedure makes manifest unsuspected social bonds is clear, but its final status is very dubious, and in the hands of journalistic writers, claiming to reveal the inwardness of the character of some great and complex civility, can do nothing but harm.[25]

In another field sociology and psychology have coincided. Before 1914 the American sociologist Cooley and the Germans Max Weber and von Wiese had turned attention to the study of the structure of immediate human group contacts and behaviour. The mapping of such situations in groups of children and the changes that take place in these situations over time has been developed by Moreno,[26] and the tech-

nique has been extended to larger and adult societies. The measurement of " social distance " (an odd sort of space in which the distance A–B is by no means necessarily equal to the distance B–A) and of types of leadership has also been considerably developed.[27] The latent and practical contents of the truism that behaviour, individual or group, is largely a function of the totality of relations in the group, have been demonstrated, with formally unnecessary if perhaps helpful pseudo-mathematical references to vectors and topology, by Lewin,[28] and this has given a new impetus to the study of the structure and dynamics of small groups, with interesting consequences for, *e.g.*, industrial sociology.

The social conditioning of behaviour can be studied in many different fields. In the realm of ideas and intellectual systems French sociology and German sociology have developed in a largely complementary way. The French contribution to this " sociology of knowledge " has mainly been carried on by Durkheim (*d.* 1917) and his followers. On the whole they have studied the intellectual worlds of pre-literates and primitives as distinct from the German concern with historic and European societies. These intellectual worlds are manifested by common, collective symbols owned by everyone throughout a society. These " collective representations " have been studied in the practices of sacrifice and magic, the evolution of values, and, in a highly literate and complex society, through the study of the total thought structure of China by Granet.[29] The work of Lévy-Bruhl, arguing that there are fundamental functional differences in the minds of primitives, belonged to this tradition, but is now no longer accepted. A more promising approach to the understanding of the logics of other societies is

that of the Frankforts, which is largely grounded in the philosophical writings of Ernst Cassirer.[30]

In Germany the sociology of knowledge began with Marx, who vigorously used its techniques to expose the social roots in economic and class interest of the arguments of those he condemned. Max Weber tried to put the subject on a more scientific, and less debunking basis—especially in his well-known study of *The Protestant Ethic and the Spirit of Capitalism*—and certainly he established that there was a two-way relation between ideology and socio-economic motives and forces as against the more naïve of the Marxists. He elaborated this theme to show cross similarities in the growth of different societies manifesting like social relations, and attempted to demonstrate the consequences of various societal factors by their presence or absence in different areas of study, using to reveal these consequences simplified ideal models of social situations as guides to the causal forces at work.

Very different was the heroic comparative study of institutions conducted in England. Here the idea that society itself could be incorporated in the majestic account of the unfolding of life given by Darwinism, and the wide range of social knowledge that was the consequences of imperial expansion and the earliest onset of capitalist industrialism produced a school at once evolutionary and comparative, and yet in time fully aware of the dangers of that naïve evolutionism which saw an identity of biological and social struggle, and believed in a simple one-to-one series of stages of social development more rigid than the very geological strata which revealed the biological " record of the rocks." In this place nothing can be said of the now under-rated work on magic and religion of Sir James George Frazer, though in the sociology of religion few

things are more urgent than a re-statement in contemporary language and reconsideration in the frame of contemporary knowledge of the main thesis of his *Golden Bough*. We must turn rather to the sociologies of L. T. Hobhouse and the Finn, E. A. Westermarck.[31]

The work by which the latter is best known is on marriage and the family. Here his negative contribution was the vitally important disproof of theories of primitive promiscuity; his positive work was greatly to advance the study of such phenomena as taboo and exogamy—though his analysis of these last has not been final—and to make clear the functional dependence of marriage on the family and the nullity of the converse opinion. His work on *Moral Ideas* remains of interest in detail, but has not endured so well as his *History of Human Marriage*.

The work of Hobhouse was diverse—philosophical, psychological, and political as well as sociological. He maintained that social development was in fact progressive in terms of a definition of progress founded not in moral values, but objectively, in terms of scale and efficiency, of mutuality and freedom. These latter concepts have been condemned as being subjective, but there is no reason why mutuality and freedom should not be defined in terms of concrete behaviour. The optimism of which Hobhouse has been accused is irrelevant to his scientific standing, and is at any rate severely moderated in his writing.

He works out in, for example, his masterly treatment of public justice in its transformation into positive law and institutionalized judgment[32] what was to become a complex and fairly complete system of developmental phases in what was probably an evolutionary sequence. This order is one of the occurrence of correlations of institutional, moral, and intellectual factors which vary

together and as specific groups in terms of economic, political, and social regimes. These factors are shown to be functionally interconnected and to reveal the growth of mind in society.

These constellations were investigated empirically, and the total picture set out in Hobhouse's *Social Development*. The earlier empirical investigation by Hobhouse, Wheeler, and Ginsberg [33] into the relations of material culture to social forms among primitives has become a classic of its kind. Unfortunately further work of this sort has been rare in the last thirty years, but it is to be hoped that the new and enormous apparatus of cross references accumulated in America (the Yale Cross Cultural Index) will enable such research to go forward on a wider basis and with more refined techniques. In England Morris Ginsberg has interestingly continued and developed the sociology of morals with a more rigorous logic and more contemporary frames of reference than those of his teacher.[34] Ginsberg's intellectual stringency and demand for scientific rigour have combined with his faith in comparative study to keep the latter a live issue in contemporary sociology.

Hobhouse's study of the growth of public justice is relevant to a particularly interesting field of sociology which must necessarily proceed on the comparative method: this is the sociology of law. The German historical school of the nineteenth century, legal historians like Maine and Maitland in England, the Marxists, and great American judges such as Mr. Justice Holmes, had all from their different positions perceived that law was a social product, reflecting the processes of time, the fears, desires, and hopes of men. But it is not merely the social moulding of law that must be studied; law in turn has its impact on society,

and this impact is sometimes a vital clue to the property relations and the tacit practical morality of a culture. The sociology of law, however, is more than theoretical, for, if legislatures are willing to learn, the need for the making of law can often best be revealed by sociological research into concrete aspects of life, and the law that is enacted made more realistic by appropriate social study. Finally, the sociology of law should turn to study the actual conduct of the courts and their workings as a very special sort of human group, and to the legal profession and its diversities as institutions developing ends far from their overt and publicly desired purposes. Alas, little work has been done in this direction.[35]

The making of law is the work of political associations, chief of which is the State—the particular object of study by the political scientist. The sociological study of political associations other than the State, and the contribution the sociologist can make even to the specific study of the State, are potentially important, but little has yet been done. Considerable interest, however, has attached to the work of the Italian sociologist Pareto, who claimed to be able to show that political *élites*—*i.e.*, ruling groups and the members of the social area from which these groups are directly recruited—conformed to two types, static or innovatory; that between these there was an alternation, and that their succession could be shown to conform to fairly simple principles of the fulfilment of function and accessibility to new members. There is no doubt that, despite the intervention of Pareto's own anti-democratic and pessimistic political opinions, this analysis is of permanent value.[36] Less valuable, though iconoclastic, salutary, and amusing, is Pareto's account of the irrational springs of social action which

he termed " residues," and their concrete manifestations. More important is his study of the types of social behaviour and the limits of rational—or, as he terms it, logical—conduct.

With Pareto we are on the fringe of certain activities often identified by outsiders as sociology proper, but regarded with disfavour by sociologists themselves. Pareto himself produced a philosophy of history (though he would have resented the phrase), and the philosophy of history as practised in this century by Pareto, Spengler, Toynbee, or by contemporary Marxists, is the subject rather of public vogue and fashion than of scientific consideration either by sociologists, busy with their smaller, more manageable tasks, or by the philosophers, who today think of the philosophy of history as being co-terminous with the epistemology of history—the study of the status and logic of sentences about the past.

Pareto's political views have been mentioned. O. Spengler, whose *Decline of the West* enjoyed a vogue in the 1920s, is now thought of as a bombast and ignoramus whose pretentious historical determinism excused the rigidities and brutalities of Fascism. This is not unfair, but there *is* more than this to be said, though the wheat could perhaps be winnowed from the chaff only with disproportionate effort. Toynbee, learned to an amazing degree, is still at work on his *Study of History*, but it is clear already that his theology, coupled with his great philosophical naïvety, makes difficult any appraisal of these case-studies of every known civilization and the not quite inevitable pattern of their fate. It is quite possible that his work could be stated with more acceptability, and that the sociologist's suspicion of the sombre poetry of these volumes is a fault in susceptibility to truth in strange guises.

One thing is certain ; it is not sociological truth, though much detailed and suggestive sociology is embodied in the actual exposition.

Marxism also is a poetry and a faith : the drama of a persecution, a judgment, and a reward for the long-degraded " ancient lowly." But it is also an analysis of the economic process of capitalism in time which, however corrected and modified, must be central in any attempt at a general theory of our society. There is embedded in Marxism a series of sociological propositions concerning the limits set by technology and economic organization to social action, and concerning the relations of ideology and the economic order, which has profoundly influenced all social scientists and historians of whatever political complexion. If a sociological theory comparable to the range and comprehensiveness of Newtonian physics—a synthesis after all still unique in any science—is ever achieved, those propositions, no doubt re-formulated, will be present in it.

IV

Sociology is only one, though the most ambitious, of the sciences of man. It is much more akin in method to psychology and anthropology than it is to economics, except as that science has been studied from the viewpoint of institutions by Veblen in America, Hobson in England, and the historical school in Germany. Many sociologists have longed to see their study reduced to an ideal model as complete and simple as that of the " perfect market," which so long dominated economic thought. The vast development of statistical economics and national-income studies, the exposition of the failure of the theorems of orthodox market analysis to

account for the reality of business practice or of long-term unemployment and the trade cycle, have all shown that this achievement was premature. Economics had to be re-made by Keynes and the new analyses of monopolistic markets in the 1930s, and has more closely approached sociology as its faith in its logical consistency and autonomous status has waned.

Nevertheless, the desire for a system of the social world to match what is believed to be the system of the physical world remains.[37] Whether it can ever be gratified—in principle the present writer doubts it—the search, as in the past work of Durkheim, Weber, Hobhouse, and Pareto, will itself be fruitful in the suggestion of new areas for research and new tools for its conduct. It is only by contact with general ideas that a science advances, and a new period of such ideas and of synthesis seems due in the sociology of the mid-twentieth century.

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(Note: These references are intended to corroborate certain points in the text, or to suggest further reading. Only two of the works commended are at all technical, but the reader who desires a general introduction to sociology before pursuing the subject in these books would find either M. Ginsberg, *Sociology* (Home University Library), or W. J. H. Sprott, *Sociology* (Hutchinson) helpful. The latter is the simpler, but both are short.)

(1) Thomas Burnet. *The Theory of the Earth*, 1689.

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