which is nothing else than a theoretical expression for the *extension curve* of a single flexible molecular chain in thermodynamical equilibrium with its surroundings.

APPLICATION OF THE THEORY

It is interesting to note that experiment is in very fair agreement with these ideas. We have already mentioned the experiments of K. H. Meyer and L. S. Ornstein, which show the fundamental fact that the elasticity of rubber increases with increasing temperature. Other measurements due to L. Hock and to P. A. Thiessen show that the thermo-elastic properties of rubber do not at all contradict the ideas of statistical elasticity. The relation (4) contains the number n of the chain-members, which is closely connected with its molecular weight. In fact, W. Kuhn, to whom we owe very interesting calculations on this field, was able to derive the average molecular weight of rubber starting from its modulus of elasticity. He got values between 20,000 and 100,000, in very satisfactory agreement with other methods earlier applied. Another consequence is that the specific heat of stretched rubber measured at constant length must be different from the specific heat measured at constant tension. This also has been proved experimentally, and supports the ideas put forward in this article.

To get real quantitative agreement between experiment and theory it is necessary to improve our model in two directions:

(1) We have to consider that a macroscopic piece of rubber consists of *many* long-chain molecules connected together by van der Waals' forces. This means that the molecules are not independent of one another, but have the property of hindering each other during the extension.

(2) It is not necessary that the rotation around the single carbon linkages in one chain should be really free. There can be some steric hindrance, which allows only a certain amount of free rotation.

Both improvements are on the same lines by which one proceeds from the equation of state of an ideal gas to the van der Waals' equation.

Our second point is identical with the van der Waals' volume correction, while the first one brings into consideration the forces between the different molecules. From the theory of gases we know that at high pressures and low temperatures during the compression of a gas a new phase, namely a liquid phase, arises, and that in this region an equilibrium between two phases plays an important part in the behaviour of the system. In very close analogy it has been found that rubber also shows at high tensions and low temperatures a new phase, namely, the small rubber crystals, which were detected by J. R. Katz and afterwards were investigated by many other workers. Under these conditions, again, an equilibrium between liquid and solid rubber determines the behaviour of the system. This equilibrium was carefully investigated by G. v. Susich and by P. A. Thiessen and his collaborators. In gases the condensation process is subject to the Clausius-Clapeyron equation, and it is very easy to derive from the formulæ mentioned above a relation which corresponds exactly to the Clausius-Clapeyron equation and which seems to be in very good agreement with the experimental facts found by L. Hock, G. v. Susich, P. A. Thiessen and others.

There is no question that a lot of experimental and theoretical work has still to be done before we shall be able to survey the whole field of elastic extensibility of high-molecular substances, but we seem to be on the right path, and many other properties, such as the viscosity of high-molecular solutions and the dielectric behaviour of long-chain dipoles, will be elucidated by such studies.

Science, Logic and Philosophy*

By Dr. Harold Jeffreys, F.R.S.

MUCH in current discussion of scientific method turns on the ancient problem of idealism versus realism. As I understand it, an idealist holds that he is inventing a universe for himself as he goes along, while a realist holds that there is a "universe existing independently of our thought and our examination of it"—to use the words of Prof. Dingle—and that what we are doing in science is to find out something about this universe. Realism has been severely criticized, and some forms of it not too severely. Nevertheless, I do not believe that anybody is an idealist. If an idealist regards the world as a mere mental construct of his own, so are other people, being part of that world. Hence if an idealist argues with other people he must think that he is arguing with his own mental constructs, and all discussion should be futile for him. So far as my observation goes, people that call themselves idealists are at

* Based on a lecture given at the University of Leeds on January 31.

least as argumentative as the rest of mankind; whereas if their belief was genuine I should have expected them to retire into a state of psychotic isolation.

The great difficulty about idealism is that every idealist must have his own separate idealism. If A and B are idealists, A thinks that he has invented B and vice versa. The relation between them is that between 'Alice' and the 'Red King'; but while Alice was willing to believe that she was imagining the King, she found the idea that the King was imagining her quite intolerable. This fundamental asymmetry makes it quite impossible for two idealists to agree. They may support idealism in similar forms of words, but each is advocating his own particular idealism, which is flatly contradictory to the other's; and if they think that they agree they must have used some other principle.

Nevertheless, idealism contains an important principle recognized by Karl Pearson[1], that any person's data consist of his own individual experiences and that his opinions are the result of his own individual thought acting on those experiences. Any form of realism that denies this is merely false in fact. A hypothesis does not exist until some one person has thought of it; an inference does not exist until one person has made it. We must and do in fact begin with the individual. But early in life he recognizes groups of sensations that habitually occur together, and in particular he notices resemblances between those groups that we, as adults, call observations of oneself and other people. When he learns to speak he has already made the observation that sounds belonging to these groups are habitually associated with other groups, and has inferred the rule that we should express by saying that particular things and actions are denoted by particular words; and when he himself uses language he has generalized the rule to say that it may be expected to hold for future events.

The use of language accepts the principle that generalization from experience is possible; and this is far from being the only such generalization made in infancy. But if we accept it in one case we have no ground for denying it in another. But he also observes similarities of appearance and behaviour between himself and other people, and as he is associated with a conscious personality it is a natural generalization to suppose that other people are too. Thus the departure from primitive solipsism is made possible by admitting the possibility of generalization. Further, it abolishes the asymmetry between different people, and it is possible for them to understand and agree with each other simultaneously, which appears to be quite impossible for two idealists. But it does not say that nothing is to be believed until everybody believes it. The situation is that one person makes an observation or an inference; but this is an individual act. If he reports it to anybody else the second person must himself make an individual act of acceptance or rejection; all that the first can say is that, from the observed similarities between himself and other people, he would expect the second to accept it. The facts that organized society is possible and that scientific disagreements tend to disappear when the participants exchange their data or when new data accumulate are confirmation of this generalization; but to take universal agreement as a primary requisite for belief is a superfluous hypothesis, and it cannot be applied in practice. It is impossible for a person to ask everybody's permission before he believes anything.

The need for an understanding of generalization is quite fundamental. It is not worth while to invent or postulate an object unless we find that we can co-ordinate many sensations by doing so. The Nautical Almanac's predictions of planetary positions, an engineer's estimate of the output of a new dynamo, and an agricultural statistician's advice to a farmer about the utility of a new fertilizer are all generalizations from experience. So are my expectations about the flavour of my next meal. Now if our reasoning is restricted to traditional logic such inferences are impossible. Traditional logic admits only three alternatives about any proposition: complete certainty, absolute denial, or blank ignorance. In pure mathematics it deals only with logical relations between nothing in particular, and is frankly admitted to have nothing to do with sensory experience until some extra hypotheses are supplied. In applied mathematics these are supplied, usually without mention of their origin, and results are deduced as exact consequences. Thus it is impossible to say anything at all until we can say it with certainty.

Generalizations from experience are not, however, made with certainty. To say that they are involves the fallacy known in logic as the excluded middle term. It appears in accounts of scientific method in various forms. Thus it may be said that a quantitative law is a description of the observations; but that refers only to the observations that have yet been made, and an infinite number of laws would fit any finite number of observations ([2], pp. 37–39). To infer any new observation from it we must select a particular law from this infinite set; and without some rule not contemplated in traditional logic there is no means of making such a selection, or therefore of preferring any prediction to any other. Conversely, if we say that we learn our laws from experience we can avoid the fallacy of the excluded middle only at the cost of admitting that there are valid inferences that are not made with certainty. We must, in fact, choose between three alternatives.

(1) Prediction is meaningless: then all practical men are wasting their time, the sun may rise in the west to-morrow, and human relations that depend on people understanding one another's language are impossible.

(2) Prediction is made deductively, not from experience, but from some general principle alleged to be logically certain; if experience plays any part at all it is only to fill in a few details. This is the attitude of many modern theoretical physicists. However, their general principles and their results differ even within the very limited field of knowledge where they have been applied; and at the most we can regard them only as guesses worthy of proper test by experiment.

(3) Generalizations from experience can be valid inferences, but are not made with certainty. But in that case traditional deductive logic is admitted to be inapplicable to either ordinary life or scientific method.

There are no other alternatives; nobody believes the first; the second is very doubtfully applicable at all, and has not been applied to the fundamental problems; the third is generally accepted, but asserts the inadequacy of deductive logic. It demands the notion of a degree of reasonable confidence, of which certainty and impossibility are the extreme possible values; but the former, while it may be approached by a generalization or an inference from one, is never quite attained.

The need is indeed obvious without this discussion if we consider the kind of inferences that we actually draw. Suppose that we measure the distance between two points once and get 18·1 mm.; we measure it again ten times and get values from 18·0 mm. to 18·2 mm.; we measure another distance ten times and get values from 17 mm. to 19 mm. What will we infer about the distance that we should get in a further measurement? Obviously we cannot infer a single exact value in any case; but we can say that in the second case the next value is very likely to lie between 18·0 mm. and 18·2 mm.; in the third it is much less likely; in the first we have no idea at all except that the most likely result will be 18·1 mm., unless we have independent evidence about the accuracy of our readings; if we have not, the next observation may depart by any amount. Everybody that has ever made observations is fully aware of these facts. Very few, however, have grasped the consequence, that any theory of inference that can make only exact predictions or none at all is for that reason inadequate. We must have a theory that deals with the probability of variations of different amounts. Meanwhile I shall use the adjectives "naive" to denote any theory, whether realist or idealist, that maintains that inferences are made with certainty, and "critical" for one that admits that they are not, but nevertheless have validity.

Again, suppose that we repeat the second and third of the above series of measures a day later, and get in each case an average value of 17·5 mm. Are we to say that this is a variation similar to what we have already found, or that the distance has changed? Evidently in the third series it is more likely to be of the old type than in the second; but where are we to draw the line? In neither case can we assign any definite value and say that any discrepancy less than this is certainly of the kind that we have already had and that any larger one certainly represents a change of distance. Yet we must draw some sort of a line somewhere, even though we cannot be sure of drawing it in the right place. Such problems are of everyday occurrence; but traditional logic has nothing whatever to say about them. That does not say that they cannot be treated mathematically; it is perfectly possible to generalize mathematics to give it greater elasticity, and modern physicists have done much in that direction. They have, however, shown a curious reluctance to recognize the inadequacy of traditional logic itself, being apparently saturated with the naive realistic ideas of applied mathematical teaching. Yet traditional logic denies the possibility of learning from experience, and it is right if we restrict this to mean learning with certainty; if we believe that we can learn from experience it must be with different degrees of probability.

The notion of probability as a statement of a degree of reasonable confidence goes back at least to James Bernoulli, and is quite explicit in the works of Bayes, Laplace, and a number of other writers. Other definitions have been attempted, but they all lead either to the introduction of superfluous postulates or are insufficiently general to cover the ground. I mention only one. Some writers speak of 'mathematical probability' as meaning simply that if there are n possible results of a trial and m of them include the event considered, the probability of the event is m/n. (See, for example, the series of reviews in NATURE of January 8, p. 55.) In earlier works, such as those of Laplace, this was stated with the proviso 'provided that all the ways are equally likely'. Thus it was not a definition of probability, since the notion occurred in the statement; it was a rule stating how to estimate probabilities in certain circumstances, the idea of probability itself being taken as already understood. If the proviso is dropped, however, we find that the 'mathematical probability' of a coin coming down heads is $\frac{1}{2}$ even if it has been loaded with lead on one side and has already given ten heads in succession. The definition is simply a concession to traditional logic and discards the essence of the matter; and traditional logic admits no compromise. It says that the result of a trial, or of any number of trials, is simply unknown and there is no more to be said.

The question is, then, can we construct a mathematical theory of probability, in the ordinary sense of the term, that will be consistent, and will meet the requirements of scientific procedure—and incidentally of ordinary life? It turns out that we can. The fundamental idea is simply that of the probability of a proposition, given the data; we need the postulate that such probabilities can be arranged in an order*, and a few minor postulates and conventions that make it possible to assign numbers in a one-one correspondence to probabilities[2,3]. The principle of inverse probability follows as a theorem, and enables us to compare the probabilities of different hypotheses on the same data. The results are consistent in the sense that if we compare the probabilities of two hypotheses at any stage of our knowledge, and other information is afterwards attained, the ratio of the final probabilities will be the same in whatever order the new pieces of information are taken into account. (It is known that complete consistency cannot be proved even for pure mathematics.) Inconsistency can arise if we speak of the probability of a proposition without reference to the data, which is still liable to be done on account of an inadequate notation that does not mention the data explicitly; but with a correct notation (due in principle to W. E. Johnson) no such trouble can arise. The essential point is that to say that we can learn by experience is the same as saying that the probability of a proposition is a function of both the proposition and the data. With this theory we can proceed to discuss what confidence we may attach to hypotheses, given observed data, if we are to follow consistent rules, and not to have different standards of validity depending, say, on whose hypothesis we are discussing.

Ordinary and scientific reasoning both admit learning from experience and therefore recognize the inadequacy of deductive logic; and everybody admits the significance of the statement "on data r, p is more probable than q". Different people may disagree in particular cases about which is the more probable, but they agree that the statement has a meaning. We start with the agreement and postpone consideration of the differences. It turns out that they can be attributed either to incomplete working out of the results (which can happen in pure mathematics) or to allowing our beliefs to be influenced by our wishes. Science has got on fairly well without any formal statement of its principles, but it is definitely advantageous in practice to have such a statement, which will at least allow these complications to be placed where we can see them and may possibly enable them to be removed, since they are perfectly capable of being tested; and on the theoretical side it liberates inference from experience from the charge of being indistinguishable from a set of arbitrary assumptions. It turns out that the apparent assumptions are all closely connected and that the process is much more coherent than might have been thought.

One immediate result is that the traditional idea of causality must be discarded as possibly wrong and certainly useless. With the possible exception of mere counting, a scientific law is never exactly verified. This fact appears to be quite unknown to philosophical critics and to be conveniently forgotten by most scientists when talking about the basis of science. We must either choose our laws to fit the data exactly or be satisfied with a compromise. At the times when Euclid and Newton worked, the unexplained discrepancies were some hundreds of times those that ultimately led to modifications of their laws; but in spite of this the laws were taught as having deductive certainty. Scientific success does not consist in exact correspondence, but in the improvement of approximation. However carefully we measure, our measures never repeat themselves exactly; we may speak of 'experimental error' in some metaphysical sense, but the solid fact is that the measures vary—unless we use so coarse a measuring scale that all variations are swamped by the step of the scale, and if we do that even such a law as the additive property of distance is not verified. Even the simple law that the length of a solid body is constant is not exactly verified. But what is true is that the variation is irregular, and we can consider the hypothesis of 'random variation'; that is, that there is a part of each measure which cannot be foretold at all from previous measures.

On this hypothesis we can find a distribution of the probability of the remaining or permanent part. But this hypothesis is not certain, and we are entitled to consider the alternative that only part of the variation is random, the rest varying at an unknown rate the most probable value of which has to be estimated from the observations. It might account for any fraction of the total

* The postulate is actually that "on data r, p is more probable than q" has a meaning and that this relation is transitive; when a practical statistician gives advice on the "best" way to proceed in order to achieve a desired result, he is assuming that "p is more probable on data q than on data r" has a meaning. But the latter can be proved from the other postulates and need not be taken as a separate postulate.

variation*, and the prior probability of its possible values has to be distributed uniformly within the range permitted by the total variation to express this. We can then compare the total probabilities, given the observations, that the whole of the variation is random and that part of it is systematic in this sense. It is found that if the new term as found by a least squares solution is more than a certain amount, the observations increase the probability that part of the variation is systematic; if it is less than this amount, they decrease it and support the proposition that the whole of the variation is random. The critical value is usually two to three times the standard error as usually estimated, and therefore is in good agreement with the rule that has been found to work well in practice.

The method can be stated as saying that, if we have no previous knowledge beyond the fact that the new parameter is worth considering, we take the prior probability that it is zero as $\frac{1}{2}$, and we accept it as genuine if the posterior probability is less than $\frac{1}{2}$ (though not with much confidence unless it is considerably less). This is what I have called a 'simplicity postulate' ([2], p. 250). Some objection has been made to it by alleged naive realists, who would apparently accept every estimate uncritically as an exact determination. I can only say that every competent statistician does reject new parameters below about the limit that I find, and that nobody who has ever accepted two estimates as consistent because they agreed within the standard error of their difference is a naive realist. Anybody who rejects the simplicity postulate must apparently believe that the Nautical Almanac Office, in predicting the positions of the planets, would get better agreement with future observation by fitting polynomials exactly to the whole of the observations and then extrapolating, than it gets by the actual method of finding the minimum number of parameters by least squares and calculating according to the law of gravitation. But if anybody really believes that, he would add to the clarity of discussion by saying so explicitly. If, on the other hand, current procedure is admitted as valid, it is thereby admitted that probability must be introduced at some stage before we can get practical results. But if it has to come in sooner or later anyhow, we may as well have it at the start; and then it is found to solve most of the problems that have led to controversy, since the apparent postulates turn out to be either superfluous or legitimate inferences from experience.

* It is because it often accounts for nearly all of it that a physicist can think that there is evidence for strict causality. In such a subject as agriculture no such confusion is possible. Fisher's term "the analysis of variance", though used by him only in relation to a particular technique, goes straight to the root of the general problem.

[1] Karl Pearson, "The Grammar of Science".
[2] Jeffreys, "Scientific Inference", 1937.
[3] Jeffreys, *Proc. Roy. Soc.*, A, **160**, 330–335 (1937).

[*To be continued.*]

Obituary Notices

Mr. W. H. B. Cameron

THE untimely death of Mr. W. H. B. Cameron on February 16, at the early age of thirty-six years, came as a great shock to all who knew him.

A native of Ulster, Mr. Cameron entered Queen's University, Belfast, as Sullivan scholar from the Royal Belfast Academical Institution, and graduated in 1923 with honours in physics and mathematical physics. During the next three years he was successively Musgrave demonstrator in physics and Musgrave research student at that University, and undertook research work in spectroscopy under the direction of Dr. R. C. Johnson. His work on spectra associated with oxygen and nitrogen, for which he was awarded the M.Sc. degree, was followed by work on the nitrogen afterglow, and on the production of various spectra in the presence of neon and argon. During this work he discovered the bands of carbon monoxide now generally known as the Cameron bands, and also some new bands of silicon oxide.

Mr. Cameron joined the staff of the Physics Department at the University of Sheffield in 1926, and continued there his spectroscopic researches, working first on the band spectrum of sulphur, and later carrying out the construction of a novel grating spectrograph. Later work was done in collaboration with his colleague, Dr. A. Elliott, on intensity measurements of the first positive band spectrum of nitrogen excited by various methods. This was followed by a joint analysis of the visible emission bands of chlorine, recently published, in which it was shown that the bands are emitted by singly ionized chlorine molecules. Further work on the continuous spectra excited in chlorine by active nitrogen was in progress when he was stricken by his fatal illness.

Mr. Cameron's activities, however, were by no means confined to his scientific work. He had been leader of the University Rover Crew from its inception, as well as resident tutor at Crewe Hall and supervisor of University lodgings since 1936. During his time in Sheffield he was actively associated with St. Andrew's Presbyterian Church. He was secretary and past president of the Sheffield Physical Society, and an associate of the Institute of Physics.

In all phases of his varied activities, Mr. Cameron's work was characterized by unflagging patience and