The amount suggested as his honorarium seems large only because such appropriations of public funds are so rare. After all, it is only the interest on $150,000, and, compared with the fortunes made by other inventors—the Fords, the Edisons, the McCormicks and their like—it seems absurdly small. But it is enough, for the needs of the scientific investigator are small and, assured for himself and his family against the necessity of earning a livelihood by immediately profitable work, he will be content—will count himself, indeed, among the luckiest of mortals.

That the action of Canada in the case of Dr. Banting, if taken, will be exemplary, is not too much to hope. It will call world-wide attention to the fact that there are discoveries and inventions which should not be made the basis of a monopoly by the issuance of a patent or copyright, though, on the other hand, they should not be allowed to go unrewarded.

National governments have a duty in this matter, and one which they rarely have recognized. For the most part they have left the maintenance of scientific research to the generosity of individuals or of the few private corporations which have arrived at realization of what "pure science" can do for them. This, however, implies either the acceptance of something very much like charity—the taking of favors for which thanks must be given—or the receipt of a salary that at any moment may cease.

A government, if conducted with sufficient intelligence, would change all this. It would establish facilities for determining just what men had rendered or were likely to render services so widely beneficial that everybody should be expected to pay for them. Then it should make due provision for acquiring a discovery or invention of general benefit and offering it freely to anybody in the country, or in the world, who wants to use it.

Once, at least, our own Congress did just this—it appropriated what it considered a sufficient amount to pay the inventor of "babbitt metal" what that excellent alloy was worth, made its manufacture and use free to all, and so prevented the imposition on all users of a tax continuing as long as a patent would run. If more of this wisdom were displayed, fewer enormous fortunes would be made, perhaps, but that would be no great calamity.—*The New York Times.*

SCIENTIFIC BOOKS

The Mathematical Theory of Probabilities and its Application to Frequency Curves and Statistical Methods. By Arne Fisher. Vol. I., *Mathematical Probabilities, Frequency Curves, Homograde and Heterograde Statistics.* Second Edition. The Macmillan Company, New York, 1922, pp. xxix + 289.

A Treatise on Probability. By John Maynard Keynes. The Macmillan Company, London, 1921, pp. xi + 466.

The literature of probability, honorable in the history of science as it is, is not so extensive but that the appearance of two major works on the subject within a year of each other is a notable event. It seems appropriate to review these two books together, because they represent so perfectly what have been, throughout the history of the subject, two diametrically opposed schools of thought about the theory of probability. On the one hand we have the point of view of the person who sees in the theory of probability one of the most potent tools the human mind has ever devised for penetrating deeper into the relations and laws of phenomenal nature. This is the point of view, in short, of the natural scientist who wishes to *use* the theory of probability in the conduct of the practical business of his life in the manner of approach of Laplace, Clerk Maxwell, Willard Gibbs, Karl Pearson and a host of the greatest figures in the history of science. On the other hand is the point of view of the person who regards the theory of probability as essentially only a branch of metaphysics, and finds its usefulness in the fact that it furnishes an entertaining and involved subject to speculate and talk about.

The first of these viewpoints is represented in the book, already well known to statisticians from its first edition, of the distinguished Danish mathematician and actuary, Arne Fisher. It is a sound treatise, of excellent workmanship, on the mathematical theory of probability and its application to practical statistical problems, developed mainly from the standpoint of the Scandinavian school of Thiele, Charlier, etc. It is extremely valuable to have the ideas of this school thoroughly and clearly presented to English and American students, as is done in Fisher's book. Furthermore, there is a freshness and originality in the author's mode of exposition which is highly stimulating and entertaining to the student. Whether the methods and ideas of the Scandinavian school will supplant those of the English school, which derives from Karl Pearson, seems doubtful, so far as concerns American workers, at least. But it is a fine indication of the healthy, vigorous condition of the subject to have these two lines of great activity flourishing at the same time. This second edition of Fisher's book is considerably expanded and improved over the first. It should be in every statistical library. Not the least entertaining feature about it is the commendably vigorous language in which Fisher flays Keynes and tacks his integument up for public inspection and ridicule.

Which may suggest that the present reviewer holds the second book on our list in rather low esteem. Such is in fact the case. Leaving wholly aside, as unimportant, the flippancy, super-smartness and debonair conceit so manifest in the style in which the book is written,[1] the thing which makes it not only an unreliable guide, but in the reviewer's judgment a positively pernicious one for at least that large group of students who wish to make practical use of the theory of probability in scientific research, is its abandonment of the experiential basis of probability, and the substitution in its place of the thesis that the basis of probability is simply a logical relation, independent in respect of its ultimate philosophical validity of any experience whatever. The author rejects completely the possibility of numerically measuring a probability, except in one particular narrowly defined case. The whole thing is essentially a postulational performance. Keynes sets up certain fundamental postulates, which bear no particular relation to any known phenomenal universe, then proceeds to develop a system of consequences of these postulates, and finally takes as the criterion of validity the logical consistency of the resulting system. This process is, of course, well known in mathematics, and has served in some hands and in some fields a philosophically useful purpose. The reviewer *guesses* (he has no intention to waste the time necessary to check over the symbolic logic to prove it) that Keynes's system *is* logically consistent, if the initial postulates are granted. But this is a sterile triumph so far as the application of probability to scientific research is concerned.

Of course the book is not all bad. No book is. I can not resist quoting one passage, which seems destined to become classic, as an example of the author's powers of clear and penetrating thought, subtle reasoning and lucid exposition. It is this (p. 36):

"When we say of three objects, A, B and C, that B is more like A than C is, we mean, not that there is any respect in which B is in itself quantitatively greater than C, but that, if the three objects are placed in an order of similarity, B is nearer to A than C is. There are also, as in the case of probability, different orders of similarity. For instance, a book bound in blue morocco is more like a book bound in red morocco than if it were bound in blue calf; and a book bound in red calf is more like the book in red morocco than if it were in blue calf. But there may be no comparison between the degree of similarity which exists between books bound in red morocco and blue morocco, and that which exists between books bound in red morocco and red calf. This illustration deserves special attention, as the analogy between orders of similarity and probability is so great that its apprehension will greatly assist that of the ideas I wish to convey. We say that one argument is more probable than another (*i.e.*, nearer to certainty) in the same kind of way as we can describe one object as more like than another to a standard object of comparison."

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[1] Which leads to such choice remarks as the following (p. 180): "It may, however, be safely said that the principal conclusions on the subject set out by Condorcet, Laplace, Poisson, Cournot and Boole are demonstrably false. The interest of the discussion is chiefly due to the memory of these distinguished failures."

SPECIAL ARTICLES

ON THE EXISTENCE OF AN ANOMALOUS REFLECTION OF X-RAYS IN LAUE PHOTOGRAPHS

SPECTROMETRIC observations[1] upon crystals of potassium iodide have pointed to the existence of strong diffraction effects which could not be explained as "reflections" from any imaginable atomic planes. The positions[2] of these X-peaks, as they have been called, have been defined for various angles of diffraction and their wave lengths determined as equal to that of the characteristic radiation of iodine. Possibly related effects[3] have also been observed in the powder photographs from several metals. Very recently a Laue photograph[4] to show the presence of these anomalous reflections has been offered.

Inasmuch as the existence of such diffractions not obeying established laws must of necessity have a great influence upon the interpretations of X-ray phenomena, the study of their properties becomes of importance. Their failure to appear under the prescribed conditions may have even greater significance. The writer has obtained a number of Laue photographs of potassium iodide and no effect corresponding to these X-peaks appears on any of them.

The X-peaks are supposed[4] to show themselves in a Laue photograph taken with the incident X-rays parallel to a cube face as four *spots* symmetrically placed about the center and lying in the same zone as the (100) and (130) reflections. Their distance from the undeviated image will be[4] one centimeter if the crystal-to-plate distance is 2.5 centimeters. The recorded photograph was said to be produced by an

[1] G. L. Clark and W. Duane, *Proc. Nat. Acad. Sci.*, *8*, 90 (1922).

[2] G. L. Clark and W. Duane, *Proc. Nat. Acad. Sci.*, *9*, 131 (1923).

[3] L. W. McKeehan, *J. Opt. Soc. Am.*, *6*, 989 (1922).

[4] G. L. Clark and W. Duane, *J. Opt. Soc. Am.*, *7*, 455 (1923).