the phenomena in high vacua, in the photoelectric investigations, in the systematic inquiry into thermoelectric and radioelectric effects, and in alternating current rectifiers. Naturally great experimental difficulties are encountered in inquiries of this kind and these are augmented by the inordinate desire to gain premature theoretical conceptions, especially mechanical conceptions of what takes place, when the phenomena in question are probably not at all amenable to explanations of this nature. We must learn to work by faith and inspiration. We must learn again to divine the truth. We must be led as such geniuses as Davy and Faraday were led in their experimental inquiries. What guided them in their fundamental discoveries was a species of intuition, and not complex mechanical theoretical conceptions made still more obstruse by intricate, yet nevertheless quite inadequate, mathematical equations and expressions. Theories have their place. They are useful but they must not be taken too seriously. They must not be believed too hard lest they enslave us and bind us to earth. It is highly desirable to keep the minds and spirits of our younger generation free and unhampered by too serious consideration of theoretical conceptions with which present-day scientific literature overabounds. Rather a jovial, happy-go-lucky spirit of experimentation is to be cultivated, for as the great poet Goethe has put it—"Grau teurer Freund ist alle Theorie und grün des Lebens goldener Baum."

In this spirit and in this sense our efforts toward opening new avenues of inquiry by stimulating novel ways of experimentation that are calculated to reveal undreamt of vistas, our society has established its new division of electronics and paralleled the same with electrothermics and electro-deposition. May this new field prove specially fruitful, may it provoke thought and discussion and yield experimental results of a basal character that shall call for additional, novel divisions of our society so that the ends for which it was founded may truly be realized.

SOME REMARKS ON MATHEMATICAL STATISTICS[1]

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IN casting about for a favorite topic on which to address you on this occasion, I was anxious to give much weight to your feelings as to what would be most appropriate considering both your interests and my limitations. It seemed fairly obvious that you would wish me to speak on a subject of which I have some special knowledge and to which I have made at least a few contributions. These conditions determined the general field in which to select a topic. In spite of these great limitations I wrote down a dozen or more specific topics, but finally selected a subject so broad that it leaves plenty of room for suitable limitations as we proceed. In this way it has come about that I am to make some remarks on mathematical statistics. Although much that I shall say may be an old story to many of you, I shall find something of a child's delight in "saying it again" if I can say it in such a manner as to interest you.

The statistician engaged in the collection of data has sometimes been pictured, like Sam Johnson's maker of dictionaries, as the slave of science doomed only to gather together the material with which others build and press forward to conquest and glory. While this picture may portray a modicum of truth, my experience of over twenty years in the examination of data obtained from a great variety of sources leads me to think that the genuine collector of material ordinarily enjoys the process of collecting more than he would enjoy the more difficult thinking involved in the analysis and interpretation of the data. The analysis of available data often lags far behind the process of collection, particularly in cases of organized research. It is in the process of analysis that mathematical statistics has become, to a very considerable extent, the servant of science.

The publication of the "Théorie Analytique des Probabilités" of Laplace in 1812 marked the culmination of the first great period of activity in the development of the principles underlying mathematical statistics. In fact, the publication of this monumental work of Laplace practically closed the first great epoch of the development of these principles; for, following this publication, relatively few outstanding results or central theorems of mathematical statistics were contributed until our current period of activity which started in the decade 1890 to 1900, with the development of generalized frequency curves and a theory of correlation. To be sure the DeMoivre-Laplace law of error was developed by Gauss and given its important place in the adjustment of observations, but there was, on the whole, relatively little progress.

The activity of the decade 1890 to 1900 may be properly regarded as the inauguration of the second great epoch in the development of mathematical statistics. As early as 1895 the fact had become

[1] Presidential address before the Iowa Academy of Science, May 1, 1931.

fairly well established from the analysis of frequency distributions of very excellent anthropological and biological data that the Gaussian or normal probability curve is inadequate to represent many important frequency distributions. When the problem of developing generalized frequency curves was finally attacked, the attack was made from several different directions. Thiele and Charlier in Scandinavian countries, Pearson and Edgeworth in England, Fechner and Bruns in Germany developed theories of generalized frequency curves from very different view-points.

As a result of these different view-points, there have been developed several different systems of frequency curves. While any expression of preference for a pet system of frequency curves is likely to lead to controversy, I shall have the temerity to say that the Pearson system holds first place and that it and the Gram-Charlier system have been used to such an extent that they may, from certain points of view, be fairly regarded as the major systems, whereas the translation system of Edgeworth, the transformation system of Kapteyn, the logarithmic normal curve and others may be regarded as minor systems.

Pearson provided at first four types of curves, then increased the number to seven and finally in 1916 to twelve. We have two types in the Gram-Charlier system. However, the latter system involves infinite series and might on this account be regarded as consisting of many types. We not infrequently hear the question, What is the primary object of generalized frequency curves which seem rather complicated to many scientists and non-mathematical statisticians? My answer is first that the development of generalized frequency curves from plausible underlying laws of probability represents the results of the desire of the human mind to produce a sort of theoretical norms to serve as models for describing and analyzing both observed frequency distributions, and the expected distributions of results from repeated observations. Next, the type of frequency curve that will fit an observed distribution is likely to suggest appropriate assumptions for a mathematical treatment. Finally, it seems that to seek the laws of distribution of statistics is the outgrowth of a natural movement to which a distinguished botanist, the late Professor J. Arthur Harris, aptly directed attention in the following words of an address given a short time before his death: "Apparently all the physical and biological sciences move in their development toward a final period, not yet surpassed in any of the sciences, of mathematical description and analysis, and of the formulation of mathematical laws."

We turn next to a brief mention of the development of the theory of correlation. In mathematics, the bulk of our work and energy in the study of relations has been put into the consideration of very precise relations known as functional relations. Thus, we have given much consideration to situations in which x and y are so related that when x is given, y can be calculated or is at least determined. A fair amount of energy has also been put into the consideration of the association of elements under the assumption that they are independent in the probability sense of independence. We thus consider, in the main, what may be called cases of perfect dependence at one extreme and of perfect independence at the other. Such mathematical concepts have served very well the needs of a wide range of physical applications. But a new kind of mathematical concept was needed to deal with the region between the two extremes. In this intermediate region there lies apparently a large field of investigation that I may perhaps appropriately call the field of correlation theory. In the usual field of mathematical relations, the primary question is: given an assigned x what corresponding value of y is determined? In this new field the corresponding question is: given an assigned x, what is the probability that the corresponding y will take a prescribed value, or what are the probabilities that y will take any one or more of a certain prescribed set of values?

Correlation theory was early extended to deal with n variables in correspondence where n is any positive integer. The geometry of this situation has led into space of $n+1$ dimensions, and has served to illustrate Karl Pearson's remark that a knowledge of space of four or more dimensions is essential to an exact study of the diseases of defective children.

Many of the formulas of correlation theory are identical with the formulas of hyperspherical geometry. In this connection let me recall to you the address of Professor Dunham Jackson before this Academy three years ago in which he gave an elegant exposition of correlation by appealing to the geometry of n dimensions.

While I think I am appreciative of the usefulness and importance of the method of correlation, it is only frank to say that mathematicians are rather generally inclined to regard the method as having an empirical flavor. Although certain norms in the nature of mathematical functions suggested by laws of probability are involved in the theory, there is involved also a good deal in the line of curve fitting which has some resemblance to the fitting of empirical curves to data. The main purpose of one of my papers published about ten years ago under the title "Urn Schemata as a Basis for the Development of Correlation Theory" was to remove from correlation theory something of its empirical character. The suggestion of this development came from the fact that the most fundamental theorems in the mathematics of statistics, say the Bernoulli theorem and the DeMoivre-Laplace theorem, are derived from urn schemata involving pure chance. In this connection it should be said that the late Professor Tschuprow, a Russian mathematician, took an important step in advance in his book on correlation published in 1925, towards connecting correlation analysis more closely with the abstract theory of probability.

We turn next to a brief consideration of recent progress in the solution of problems of random sampling. Most of our statistical investigations are in the nature of inquiries by taking a sample from a large and possibly infinite supply of material. To give a very simple illustration, we make a statistical inquiry about a characteristic of a whole class of men by taking a random sample of, say, 100, 1,000 or 10,000. Of course, this is not absolutely unlike the experimental procedure in the most exact sciences. Thus, the physicist and the chemist take small samples of a large supply of material. The difference lies in the degree of variability of the material. The individual items of the random samples taken by the statistician are often decidedly variable compared with the samples ordinarily taken by the physicist or the chemist. However, there seems to be no sharp line of distinction between statistical and non-statistical data. The data of the biologist are in many cases decidedly variable which is tantamount to saying the statistical elements are prominent.

A few years ago R. A. Fisher proposed the word "statistic"—the singular of the word "statistics"—for any result such as an average or an index calculated from a sample. We shall adopt this suggestion in the present paper, although the plural "statistics" in this sense is objectionable. A statistic varies from sample to sample. One of the main concerns of mathematical statistics is with the problems of variation and distribution of a given statistic. In attempting to draw inferences about the class of material from which a sample has been drawn by means of such a statistic as an arithmetic mean, a standard deviation, a correlation coefficient, or a regression coefficient, the essential inquiry is about the sampling fluctuations of the statistic, at least to the extent of knowing its probable error or some other appropriate measure of sampling fluctuations. Probable errors were first worked out for a few important statistics on the assumption that the distribution of a statistic would follow the normal probability curve. The problems were difficult even under this simple assumption. Student published in 1908 on the distribution of the ratio of the discrepancy of the mean of a sample from the mean of the population to the standard deviation of the sample drawn from a normal distribution. The distribution of this important ratio, which I call the Student ratio, has made it clear to statisticians that, with small samples, it is invalid to assume that almost any useful statistic is normally distributed. To carry the new knowledge about the distribution of the Student ratio into practical statistics, we now have a new probability table that should ordinarily be used by the practical statistician in place of the normal probability table in testing the significance of means obtained from rather small samples.

At the beginning of the present century, the exact knowledge of the distribution laws of statistics seems to have consisted in that of the arithmetic mean of items drawn from a normal distribution together with that of the mean-square for items from a normal distribution. Further than this, the distribution of the arithmetic mean for items from a uniform distribution was probably known. To this meager knowledge considerable has been contributed since the year 1900. In 1915 R. A. Fisher succeeded in giving the theoretical distribution of correlation coefficients obtained from samples of n-pairs drawn from normal distributions for any value n.

With the beginning made by Student in 1908, we have been for the past fifteen years in the period of unusual activity in attempts to find properties of the distribution laws of various important statistics. As a result of this activity we know a good deal about the nature of the distribution of the most important statistics for random samples drawn from a normal parent distribution. We know a little about distributions of a few statistics calculated from samples drawn from some very simple non-normal distributions; but, on the whole, we may say that large problems still await exact solution in cases in which the parent distribution is not normal.

In the foregoing remarks I have tried to give the main lines of a picture of some of the recent activities in mathematical statistics. As closing comments I wish to turn in another direction and submit for your consideration a few remarks on the place of statistical methods and views in science. In this connection I shall draw freely on certain views admirably expressed by Josiah Royce growing out of Maxwell's classification of the methods of science.

Maxwell recognized three main methods in science: The historical method, the mechanistic method and the statistical method. By the historical method science deals largely with individual events. An example of this method would be the scientific treatment of a comet by observations, or the classification and meaning of a geological specimen. By the mechanistic method, science deals mainly with invariant laws to which all events of a certain class conform, and when such laws exist, they can be used to compute and predict future events. This method is concerned with the individual event in so far as it was predicted by the law. Thus, the mechanistic method is concerned with the exact prediction of the individual comet but this concern is very different from that involved in the historical view of the comet. The triumphs of mathematical astronomy, and of analytical mechanics, furnish some of our most striking examples of the success and satisfaction that go with the mechanistic view. No one has any doubt about the importance of the achievements resulting from this view.

In taking the statistical view, the main motive does not center about the individual event nor about the invariant law. Its concern centers about the average and probable constitution of a set of variable items and about the probability that this average will remain stable within certain limits of approximation. The world of the statistical view consists of individual items, but the main interest lies no longer in each individual item. The statistical view is concerned mainly with a set of items. It may turn out that the occurrences conform to a law, but the law sought by the statistical method is an account of the collection of events in terms of averages and probabilities.

Royce illustrated the way the statistical view contrasts with both the historical and mechanistic views by considering how each point of view applies to an event such as is expressed in the assertion: "A killed B." For a strictly historical point of view this event is a unique occurrence—possibly a free-will act. It falls under moral and criminal laws. For a strictly mechanistic view of things, the killing resembles the appearance of a comet which could have been predicted. Unique as it is, it is supposed to have been essentially predictable. The argument would perhaps run somewhat as follows: If you had known the precise configurations of all the physical particles in the world at some appropriate moment and the laws governing these particles then this killing could have been calculated in advance. It is a mere case of law—a point of some life line.

But from the statistical point of view the killing of B by A is an event against which insurance provision could have been made in advance—not because it could have been predicted that A would kill B, but rather because this individual occurrence could not have been predicted and because the death rate of men of B's age can be statistically calculated with approximate and probable accuracy, so as to make a policy insuring B's life a contract whose value is calculable, not on mechanistic but on statistical grounds. You might be interested in contrasting the views of life insurance under the statistical view with that under the mechanistic view. Under the mechanistic view, we should hold that except for our present ignorance of the physics, chemistry and mechanics of life, we could calculate and perhaps announce the date of each man's funeral at the time of his birth or earlier. All our life annuities would become annuities certain. This seems fanciful and somewhat ridiculous, but it would seem to be the logical consequence of making the mechanistic view the only view in science.

It seems to be a fact that the actual scientific knowledge of phenomena touching life most directly is, in the main, statistical knowledge. It is the sort of knowledge exemplified by mortality tables. This applies to what we know about heredity, birth, growth, behavior and death. It applies to economic processes and social transformations. Even if life be a mechanism, its phenomena are best known in terms of statistical averages and probabilities. Again the deeper knowledge of our most exact sciences seems to be pointing more and more to the conclusion that many fundamental problems of physics are statistical in nature. Thus, problems of atomic structure are regarded as statistical with the results stated in terms of probabilities. The individual electron does not seem to follow strictly determinate laws. We are told that either its location or its velocity remains in doubt. Its future motion can not be exactly determined, nor can its past motion be exactly given. This does not seem to be due to imperfect devices of observation and measurement, but to an inherent characteristic of matter. The principle of uncertainty in relation to either the position or the velocity of an electron is a statistical statement. It is fairly obvious that the influence of this principle of uncertainty is being felt in the philosophy of science. Perhaps some of you will recall in this connection a paper by Professor Warren Weaver in *The Scientific Monthly* for November, 1930, on "The Reign of Probability." His concluding sentence is that "the first part of the twentieth century (and we know not how much more) should be known as the reign of probability."

In considering the place of the statistical views in science, the question naturally arises: Is the statistical method in science simply a substitute for the mechanistic method or a last resort when the situation becomes so complicated that we give up making predictions about each individual item by any calculable process? It may seem so from a first impression of what has been said in this paper. Moreover, I think the statistical method is frequently in the nature of a last resort with regrets that we can not adopt the mechanistic view. However, I do not believe this description covers the ground reasonably if we should review the rationale of the whole situation. The statistical method has a rationale of its own. In this connection, it may be well to recall that the validity of a proposition about physical phenomena under the mechanistic view at its best depends in the last analysis on verifications that usually involve errors of observations and approximate numerical computations. On account of such errors, we are unable to assert that the very exact proposition of the mechanistic view is literally true. On the other hand, we may be able to verify and assert in certain cases that the proposition in the statistical view is literally true, although it is a proposition about the probable and approximate. It thus seems to be a tenable position that the statistical view should not be regarded in all cases as a last resort.

In conclusion, the plausible inference is that along with our reverence for the mechanistic view and its achievements, it seems appropriate to recognize its limitations, and to develop also an appreciation for the rationale as well as for the convenience of the statistical view. However, it is not my intention to exaggerate the importance of the statistical view. In practice, it is usually the joining of statistical and mechanistic considerations that makes it possible to get workable results out of what appear on the surface to be weak and often meaningless relative frequencies and averages. The extensive analysis of data by improved statistical methods is a great step in advance, but such analysis will remain relatively sterile unless it is supplemented by the formulation of useful or interesting theories.

OBITUARY

STEPHEN MOULTON BABCOCK

WISCONSIN'S Grand Old Man is dead! The news has been flashed around the world that Stephen Moulton Babcock has closed his earthly labors. Working away daily with that patient, purposeful persistence that has characterized his assiduous labors for decades in his efforts to wrest from nature the mystery of the interrelations of energy and matter, to-night the busy mind is stilled. The laboratory that has afforded him the material vehicle through which his imagination played is silent. The pendulum that is so delicately adjusted that the doctor hoped he would be able to measure its variation in temperature as the weight swings to and fro will continue to vibrate until the mechanism runs down, but the hand of the master will no longer record its beat. The book was closed as he would have had it. The chapter was not finished, but a few days ago he added here and there a line, working away with undimmed enthusiasm that has been the marvel of his friends these many years. Rich in years that have been filled to the brim with new ideas that have kept his mind young and elastic, he has labored on and on. The joy of life to him was always the unsated quest.

Science is an exacting goddess. She brooks no rivals. He who would woo her and win must forego many of the allurements that detract the mind that generates new ideas. But to Babcock nothing could swerve him from his steadfact devotion to her cause.

The University of Wisconsin has had its share of really great men. Some have been great in the teaching field; some for their power of lucid statement through the spoken word that burned its way into the minds and hearts of men. Babcock was the scientist—the explorer who loved to push back the boundaries of the unknown. He knew no fatigue if any unsolved problem arose in his pathway. The joy of conquest appealed to him as it does to the finder of some undiscovered bourne, yet he would have hated to have been forced to organize his discoveries and reduce them to formal treatment. One thing he often said he never would do and that was to write a book.

The Spartan spirit of the pioneer marked his own method of research. He would not tolerate an assistant. Time after time Dean Henry, in the early days of the experiment station, would try to help him multiply his fingers through additional help, but it was of no avail. He would rather whittle out a piece of apparatus with his jack-knife (and the writer has seen many a piece so constructed) than to have a finely calibrated mechanically perfect device made for him in the machine shops. He used to say he could think better if he was using his own hands in fashioning the tools he needed.

Babcock lived in the right age to bring out the best that was in him. His pioneer spirit would fain spend but little time in poring over the writings of others to classify knowledge that already existed. He had but little regard for self-constituted authority. If a statement occurred in a book this was almost *prima facie* evidence that it had been borrowed from some other source; far too frequently, books masquerade in borrowed plumage. The laboratory, not the library, was where Babcock sought truth. He knew that nature would not lie, but he was never quite sure that man might not have erred in making the record.

Fortunately for Babcock, he had no graduate school to tie him down to Procrustean limits. I doubt whether he would ever have submitted to the exactions of a seminar. But those of us who have been fortunate enough to work where we caught now and then a glimpse of the movement of his scientific mind have indeed had a rare privilege. It was as if