a technical journal and the other summarizing the findings for quick and compact oral presentation.

Actually, many of the reports to any large scientific meeting—say the Christmas meetings of the American Association for the Advancement of Science—never see permanent record in print.

Space available in technical journals to-day is much too precious to record, for all posterity, every obscure and minor advance of science. These minor papers, which make up the bulk of almost any meeting, can serve their function best if they are presented most attractively to the listening audience, for that is about as far as they will go; that and perhaps the title, and a short abstract in the program of the meeting.

In preparing an effective oral presentation of their subject-matter it might be wise for scientists to adopt something of the technique of the professional science writers who place the question, "What does it mean?" at the top of their list of requirements.

Every one, layman and scientist alike, is interested in what any new discovery in science means to the broad path of progress. That is why the newspaper reporting of science for the layman emphasizes the significance of a discovery in the very first paragraph. If the significance were buried at the end of the paper, in the fashion of a scientific report, no one would ever bother to read it.

Scientists may not like to hear it put so bluntly but, seriously, no one in the world cares what they are doing, or how they did it or even who did it. All people are interested in about any scientific discovery is, "What does it mean?"

The technical details, occupying so great a part of technical scientific reports, interest only other specialists in that particular field. The rare exceptions are those reports important enough to have wide applications in other fields of science.

This situation is true even within a given branch of science and in physics—as an example—the boredom among the electrical experts when the band spectroscopists are talking is equalled only by the boredom among the latter, when the former speak. The same thing might well be said of the various specialists in the medical sciences when they listen to each other's papers.

All this means that a scientist, in reporting his work to a scientific society, might come to the meeting armed with two manuscripts. One would be his technical paper that he hopes may some day appear in print; the kind of report which he now reads to a bored audience. This report he would keep out of sight in his innermost pocket. Its sole use would be for those very few interested individuals who come up to him after the talk for more technical details.

The other manuscript would be his oral presentation. It would tell the story of his work in a simple, summary fashion with the emphasis on what has been accomplished, rather than on how it was done. It would be intended to tell the story for *intelligent* but *ignorant* laymen, for scientists—outside their own specialized niche in science—are just that. As such they are interested in what a new discovery means rather than the specific details of how it was done.

Mr. Lucke, in his comments in SCIENCE for April 10, rightly takes scientists to task for "reading" their reports in a fumbling, halting fashion.

However, most scientists, if they will prepare their material simply for oral presentation and use a minimum of scientific terms, will find that they will be able to "read" their papers with effectiveness and with interest to their audience.

Scientists are obviously too busy—or should be too busy—with their research to have time to take a course in public speaking (although if their reputation rises to such a degree that they are in demand for many addresses they ought to consider this possibility).

However, mere reading of a statement written in oral English, in contrast to written and scientific English, can be very effective.

The best spokesman in the world to-day is President Roosevelt. He *reads* his fireside chats. Not every one can be a Roosevelt in radio or speaking style, but every one can say things simply. Every one can avoid the laziness, for that is what it is, of trying to kill two birds with one stone by trying to read a complex scientific article whose ultimate end is to appear in print in a scientific publication. Even the President couldn't do that and make any one enjoy it.

ROBERT D. POTTER,
Science Editor, The American Weekly

SCIENTIFIC BOOKS

ROTATIONS IN PSYCHOLOGY AND THE STATISTICAL REVOLUTION

Factor Analysis. A Synthesis of Factorial Methods. By KARL J. HOLZINGER and HARRY H. HARMAN. Chicago: The University of Chicago Press. 1941. $5.00.

AMONG 24 variates the number of correlation coefficients is $24 \times 23/2 = 276$, and one might perhaps imagine that about half of these would be positive and half negative. Actually, when Holzinger and Swineford worked out the correlations among 24 mental tests given to a large group of school children they found 275 positive correlations, mostly of substantial magnitude, and only one negative correlation, which had the insignificant value $-.075$. Eight bodily measurements made by Dr. F. Mullen on 305 fifteen-year-old girls yielded 28 correlations, all of which were positive and of significant size. These facts, which could be paralleled in many other cases, point to the existence of underlying factors or causes common to the several tests or measurements. The girls who were big around the chest were also, on the average, bigger around the waist and had longer arms and legs than those smaller than the average in chest girth. In other words, some girls were big in general and some were small in general, though what was directly measured was in each case not general size but one of its many manifestations. The students who were good at word meanings were also good at addition and at counting groups of dots; in fact they were, on the average, better on everything on which they were tested than were their classmates who were less good at word meanings or in other individual tests. This and many similar observations constitute clear proof of the existence of something in the nature of general mental ability, varying from person to person, and affecting the degree of success attained by the person in anything he tries within a very extended category of mental activities.

When it is once recognized that an aggregate of such measurements is to a considerable extent a set of different measures of the same thing, the problem arises of choosing suitable weights for combining the several measures in order to arrive at the fundamental variate underlying all of them as nearly as possible. At this point there is a good deal of indeterminateness for lack of a universally acceptable criterion as to what constitutes the best possible set of weights. A semblance of agreement is reached because of the fact that any set of *positive* weights applied to the positively correlated measures gives a weighted mean that might plausibly be taken to represent general intelligence or whatever the underlying common factor may be. But when deviations from such a weighted mean are correlated there is also evidence in the residual correlations of further common factors, quite independent of the first one considered. In tests of school children, for example, there seems to be a common quality, independent of general intelligence and secondary to it in effect on total variance, that represents a contrast between verbal ability on the one hand and arithmetical ability or speed on the other. Indeed, there is evidence of several sorts of ability or differences of contrasting abilities, which so to speak must be measured in different dimensions, since they are statistically independent of one another, though they all contribute in varying measure to success in the tests. But to identify such fundamental "factors," to specify them as definite functions of the test scores, and, above all, to give them names, requires something on which different workers have not been able to agree.

A large number of schemes for the statistical treatment of such problems has been proposed. The volume now at hand is a compendium of most of them, with a good account of their bases and mutual relations, including the rotations of axes involved in getting from one to another. The introductory chapter includes an excellent discussion of certain relevant phases of scientific method in general. Matrix algebra and n-dimensional geometry are essential in this subject, and the reader is provided with introductions to these and some other mathematical topics in special sections. An extraordinarily clear elementary treatment is given of rotations in n dimensions. Correlated as well as uncorrelated sets of "factors" are discussed, there is a chapter on the estimation of the values of the factors for individuals in terms of their test scores, and some essential indeterminacies in the problem are duly indicated. There is an appropriate emphasis on the criterion of "parsimony," that is, on the desirability of using the smallest possible number of variates to describe a phenomenon. However, this criterion is apparently overlooked in the preference shown for types of solution involving not only general factors common to several tests or all of them, but also individual factors specific to each test and not entering into any other. Thus the whole number of factors used is actually greater than the number of tests, though the original object was to represent a large number of tests by a small number of factors.

This subject of "factor analysis" has received much attention from psychologists in recent years, and is altogether fascinating. Its potential applications go far beyond the domain of psychology. It is in fact a branch of the theory of multivariate statistical analysis, which is in turn a branch of the theory of statistics, and has applications in the most diverse fields in both natural and social sciences. Unfortunately, this logical position has frequently been overlooked by workers in factor analysis, who have treated it as a branch of psychology, and have therefore not thought it necessary or worth while to keep in touch with the rapid advances that have been taking place in multivariate analysis and in general statistical theory. The result is that factor analysis, after a brilliant start years ago, has come to be surrounded by an aura of obsolescent statistical methods, concepts and terminology. Its isolation from the main body of statistical theory has left factor analysis in a state which can not exactly be described as stagnation, since there is so much activity, but also can not be regarded as wholly healthy.

The work of R. A. Fisher on the theory of estimation, and that of J. Neyman and E. S. Pearson on testing hypotheses, have rendered obsolete the criteria for selection of a statistic used in most textbooks on statistics and by most factor analysts. The sampling distribution appears instead as the fundamental basis from which must be derived the choice of a statistic and the method of testing a hypothesis, governing the whole investigation from start to finish. Factor analysis persists in the old habit of regarding sampling distributions as luxuries to be considered after everything else is done, if at all. Modern statistical theory aims at getting exact sampling distributions, sometimes succeeds and, in case of failure, examines a variety of approximations with a clear consciousness of the distribution that is to be approximated. In this whole comprehensive treatise on factor analysis there is not one exact sampling distribution, nor any more modern basis for the sampling theory than the Pearson-Filon paper of 1898. Criteria such as those of maximum likelihood, minimum variance and uniformly most powerful tests lead modern statistical theory to recommend as the bases of calculation functions found by applying the methods of the calculus to the determination of maxima and minima of well-defined functions of the observations. Something of this concern with maxima and minima appears in factor analysis, but is much obscured by the injection of hypothetical "communalities" and the like into the data in such a way as to vitiate accurate statistical discrimination between rival hypotheses.

One bit of fallacious reasoning seems in the past to have affected a good deal of thinking about factor analysis and multivariate statistical theory in general, and to have given the subject a directive impulse that still persists. If p measurements are made on each of N individuals drawn from a normally distributed population at random, and are plotted as coordinates of N points in p dimensions, the swarm of points tends to have a sort of ellipsoidal shape. If $p=3$, for example, the typical ellipsoid along which the density of probability for these points is a constant may have any shape from that of a sphere to that of a pancake or a needle. In the last cases the distribution may be thought of as approximating a distribution in two dimensions or one. The problem has therefore been propounded more than once of finding a sampling distribution for testing by means of a sample the hypothesis that the distribution is really of fewer dimensions than the number p of variates measured. Such efforts are completely futile. If all the points of a population on which three variates are measured lie in a plane, then every sample from this population lies in the same plane. Conversely, if a sample of more than three points is coplanar, we may conclude that the population is coplanar because in the contrary case the probability of coplanarity of the sample would be zero. Hence there is no problem of a sampling distribution here. But factor analysts have written as though they thought that the deviations of actual observations from the values they compute on hypotheses of smaller dimensionalities could be ascribed to sampling errors. A dubious passage on pages 21–22 of the current volume is perhaps in this category, though another interpretation may also be possible. The reasoning in this passage is, on either interpretation, unsatisfactory.

When it is realized that no deviations from coplanarity, however small, can be explained as sampling fluctuations, it is necessary for the factor analyst determined to represent a number of variates by a smaller number of factors to find some other yardstick for these deviations. One such yardstick, which I suggested in 1933, is provided by the errors of measurement estimated by the reliability coefficients of the tests. Many psychologists, however, have felt that to judge hypotheses by a comparison of the main effect considered with mere errors of measurement is too much like letting the tail wag the dog, and have not used this standard. In order to explain the manifest fact that the dimensionality of the set of observations is actually the number of variates, and no less, they have resorted to highly unsatisfactory "communalities" associated with "specific factors" whose existence does not seem particularly clear on *a priori* grounds, and which is not tested by the statistical methods used.

Another possible yardstick which I also suggested in 1933 is in a theory of the variates themselves as a random sample of a hypothetical aggregate of variates endowed with a probability distribution. This theory has not been developed since then, though it must be essential to the vital problem of finding factors that shall in some sense be invariant when the set of tests used is replaced by a new set. This last question is mentioned, far too briefly, on pages 107–8 of the present volume.

On page 112 a solution is obtained for the coefficients of a general factor in terms of the observed correlations. This appears to be only one of infinitely many solutions of this problem, all satisfying what Fisher calls the criterion of consistency. It provides for the coefficient a statistic that is a function of all the observed correlation coefficients, and in this sense uses all the information in the sample; but it is not clear that it uses all the information in the sample in Fisher's sense of having minimum standard error. Once this problem of obtaining a consistent statistic with minimum standard error is understood, its solution should not be outside the range of reasonably possible straightforward mathematical research; and it is just possible that the solution might turn out to be that given. A related point is that the "least square" criterion on page 115 might well be replaced by minimizing another quadratic form whose matrix is the inverse of the covariance matrix of the triads.

The achievements of factor analysis so far seem to be in the nature of promise, and of challenge to further research in mathematical statistics. Complete methods have not yet been achieved which can be recommended to workers in the empirical sciences without reservation, or with the expectation that they will permanently be regarded as the best possible. Empirical problems, especially in psychology and neighboring fields, will certainly play a part in shaping the direction of the research necessary to put the required methods into final form. But the research will necessarily be a matter of mathematical statistics, for which no combination of psychology and matrix algebra, even with the addition of the other subjects conventionally taught in university mathematics departments, can be adequate unless the progress already made in mathematical statistics itself is utilized.

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SOCIETIES AND MEETINGS

BIOLOGY AND MEDICINE IN THE WAR[1]

In conjunction with the meetings of the Federation of American Societies for Experimental Biology, the Boston and Cambridge Branch of the American Association of Scientific Workers presented a symposium entitled "Biology and Medicine in the War."

Dr. Maurice B. Visscher, of the department of physiology of the University of Minnesota, discussed the problem of organizing biological research in wartime. Despite the general belief that the present war is the province of the physicists alone, biologists have the opportunity, he said, of making important contributions to the national effort. Not only may biological research aid in lessening the effects of war, but on the offensive side it may assist in solving the problems of combat in desert, tropical or arctic conditions, as well as in determining the factors necessary to elicit maximal performance of fighting men. Dr. Visscher pointed out that while biologists might be inducted into service and assembled in huge central laboratories, such action would delay research while laboratories were being constructed and would seriously deplete the teaching staffs of the universities. He described as short-sighted the view that practical problems should not be attacked until projects have been assigned and contracts drawn up. He emphasized that devotees of pure science should not insist on "research as usual"; on the contrary, scientists as enlightened individuals have a greater responsibility for the national welfare than have other members of the community.

Dr. Carl W. Walter, of the Harvard Medical School, defined six major concerns of war medicine: the control of epidemic disease, the care of the wounded, the organization of civilian preparedness, the acceleration of medical education, the maintenance of adequate nutrition and the coordination of research. The problems of the epidemiologist have been increased by the possibility of rapid transport of the vectors carrying communicable disease from one geographic point to another. War of movement has made necessary a new concept of military medical service, since the surgeon must now follow the mobile forces instead of remaining behind the lines in a base hospital. Other changes in military surgical practice include the insistence upon expert treatment of minor injuries and the emphasis now placed upon methods of treatment which yield ambulatory convalescents. The accelerated program of medical education presents serious problems because it not only requires a return to the strictly didactic type of instruction, but also deprives the student of time in which to mature.

Dr. Lucien Brouha, of the Harvard Fatigue Laboratory and formerly of the University of Liège, discussed the lessons which must be drawn from the attempts to organize biological and medical research in France during the first months of the war. There "secrecy" was imposed to such an extent that no scientist knew exactly what problem he had to solve, nor what progress was being made by other workers engaged in similar research. Lack of liaison between the various laboratories and between scientists and the armed forces was a fatal mistake, repetition of which must be avoided in this country at all costs. Dr. Brouha stressed the necessity of utilizing the scientific power of the nation to the utmost. Quality of the war material and of the soldiers who use it is as important, if not more important, than quantity. While the quality of the material depends upon technical achievements, the quality of the men depends upon the wise use of biological and medical knowledge in their selection, nutrition and care.

[1] Symposium at a meeting of the Boston and Cambridge Branch of the American Association of Scientific Workers, April 3, 1942.