



**Arms and Insecurity.; Statistics of Deadly Quarrels.**

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some, or even most, of the subjects act in a way which satisfies the theory, there are also some who do not. This leads to the somewhat unsatisfying conclusion that the processes which underlie individual choice behavior are subject to individual differences in a way that makes it necessary to study each individual separately if we indeed wish to predict the behavior of specified individuals.

There is, however, a third type of verification of utility theory, not discussed by Adams, which is weaker, but still of interest. It attempts to predict not individual choice behavior but rather the multivariate distribution of utility for specified alternatives in a defined population of subjects. From this distribution the proportion of subjects choosing given alternatives can be computed and compared with the observed choices of a sample of subjects from the population. This approach is exemplified by the study of Shuford, Jones, and Bock (*Psychometrika*, XXV [1960], 343-56) which shows that the Bernoullian model permits rather accurate predictions of the proportion of subjects choosing among gifts whose receipt is uncertain.

Solomon's survey of mathematical models in factor analysis should interest the sociologist who has encountered centroid factor analysis or principal component analysis in his field, but is not familiar with their historical development in psychology. Although all factor-analytic methods attempt to explain, in some sense, the structure of the correlation matrix of a set of multiple measurements, the method of analysis and the interpretation of the results has been subject to heated controversies. Solomon gives a particularly lucid account of the Spearman-Thomson controversy over the interpretation of the single factor structure of mental tests. Although now largely a matter of history, it remains of interest as a case study of the problems and pitfalls of model-building in behavioral science.

Taken together, the three papers in this book will be valuable for the sociologist or psychologist with quantitative interests who wishes to acquaint himself with a wide range of mathematical applications in social science. It will also be useful as a source of readings for a graduate course in quantitative methods of social research.

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*Arms and Insecurity*. By LEWIS F. RICHARDSON. Edited by NICOLAS RASHEVSKY and ERNESTO TRUCCO. Chicago: Quadrangle Books, 1960. Pp. xxv+307. \$10.00.

*Statistics of Deadly Quarrels*. By LEWIS F. RICHARDSON. Edited by QUINCY WRIGHT and CARL C. LIENAU. Chicago: Quadrangle Books, 1960. Pp. xlvi+373. \$12.60.

The value of mathematical models in sociology is amply proven in the works of Lazarsfeld and Gutman, Simon and his associates, and, more recently, in the work of J. S. Coleman, and others. On the other hand, the extent to which mathematics can be used to develop theoretical and applied knowledge in sociology will have to be determined in the future. If we had to form our judgment on the bases of Richardson's work, we would be quite pessimistic, for his is not only a poor "case," but also suggests that some difficulties might be inherent in the whole approach.

The work of the English physicist and mathematician, the late Lewis F. Richardson (1881-1953), is now being published for the first time, though parts of it have appeared before in journal articles. *Arms and Insecurity* is concerned with providing a mathematical model, *Deadly Quarrels* with the application of statistical analysis to a subject that could not be more relevant: the causes of war, the relationship of national security to armament, the association between an arms race and the eruption of war, etc.

*Arms and Insecurity* develops a mathematical model in which war is viewed as a function of the relationship between the armament budget of one side and that of the other side (or sides). A state of equilibrium exists as long as no side increases its armament expenditure. When one side disrupts the equilibrium, the other (or others) tend to balance it by matched increases in their military outlays. In this way an arms race is generated. When an increase in budget remains unmatched, a power imbalance is created that leads to war unless politicians intervene. The amounts of international trade between countries is viewed as a measure of their "co-operativeness" and hence as offsetting armament expenditure. An "irreversible trend" toward war is created when increases in military expenditure of some countries are large and international trade is small.

In *Deadly Quarrels* Richardson lists three

hundred wars that occurred between 1820 and 1949, as well as other "deadly quarrels" such as banditry, murders, civil riots; the casualties of deadly quarrels range from one to ten million. These quarrels are ordered in subclasses according to the logarithm of the number of their casualties. Richardson then tries to reject the null hypothesis, by building the distribution of war frequencies one would expect, if war were determined by physical opportunities alone; he then compares the actual distribution to the hypothetical one, checking for factors that make for higher or lower predisposition to war. He finds that there is *no* tendency for wars to increase or decrease over time; there are no countries which are responsible for a disproportionate number of wars; and that economic and religious factors hardly account for a significant number of wars. But his report is not limited to negative findings. Randomness is rejected on at least two accounts: war is shown to be "infectious," that is, if country *X* goes to war on country *Y*, the neighboring country *Z* is more likely to engage in war than countries *a*, *b*, and *c*, at some other corner of the earth. Second, groups which are under one government are less likely to fight each other than groups which are not.

While these are the main findings, and the ones the author is most proud of, there are others similar in nature. For instance, there were more casualties from "deadly quarrels" with each more than three thousand dead than from those with less than three thousand dead; nationalism has both prevented and induced wars (that is, prevented within nations, induced among nations), and so on. The following seems to apply to all these statements.

Starting with intrinsic criticism, Richardson just is not as potent a mathematician and statistician as one would like him to be. His equations are made to fit the numbers he deals with by a set of arbitrary coefficients. But he does not make it sufficiently clear that other sets of coefficients could have been provided, thus other equations formulated that would have fitted the data just as well (or better), and that his coefficients—unlike those of the physicist—are merely subjective estimates of the characteristics represented. Moreover, the reservation that his statements are valid, "unless politicians interfere," renders the model useless to the student of purposive behavior. In my mind, it makes their empirical refuta-

tion impossible. Nor is it clear where the deterministic nature of armament expenditure and volume of international trade stops and the ability of politicians to control them starts.

On the statistical side, the data—which Richardson himself views as being reliable only on a take-or-give 20 per cent basis—is not accurate enough to validate the claim that an arch is more appropriate than a straight line, if it allows one to engage in the curve-fitting effort at all.

Shifting to extrinsic criticism, it must be pointed out that some of the difficulties seem to be inherent in the effort in which Richardson is engaged, rather than in his shortcomings.

What happens is both expected and familiar. In order to formalize the substantive issues he (*a*) eliminates those concepts which have no quantitative expressions or on which data are not available, which make him leave out most relevant variables; (*b*) to measure the remaining concepts he invents indicators which are so partial that the results of his mathematical operations cannot but be irrelevant to the initial problem.

One illustration should suffice. The author wishes to know if armament increases or decreases national security. But since he has no comparable information on armament, he compares *expenditure* on arms. (By this standard a nation armed with one hundred missiles, each carrying an atomic bomb [which is comparatively inexpensive], is fifty times less armed than a nation which has a fifty times more expensive but conventional army.) Since he has no direct measure of national security, he sees high security as expressed in a low rate of casualties in the following war. He ends up by finding out how much it costs to kill a soldier, while he presumes that his formula deals with armament and national security.

Like some assumptions used by other model-builders, those introduced by Richardson, for example, concerning the co-operative nature of international trade, are completely artificial. But unlike other model-builders, the economists who first introduce but then remove the assumption of perfect competition, Richardson sticks to his assumptions. This makes his statements—for example, war is unlikely when international trade is high—quite naïve if not simply wrong.

In his statistical endeavor Richardson selects as his central variable the number of casualties per war, a variable we would feel intuitively is of limited relevance to most significant issues. It is certainly necessary and desirable to check on our intuition *if* some other criteria would justify the use of the variable, if, for example, there were a theory on why the number of casualties should be related to other variables studied, but ease of quantification and availability of data are not such criteria. One is, therefore, not surprised that the meaningful questions put to the data by Richardson are answered by "no correlation," and that the associations found are either utterly trivial (neighbor countries are more likely than non-neighbors to be drawn into a war), or tautological (groups sharing a government—e.g., an order-maintaining authority—fight each other less often than those which do not share one). It has been suggested that trivial statements might be wrong, hence have to be checked. We quite agree. But a profession must worry when too much of its resources and manpower are involved in checking the trivial, while the non-trivial—just as much in need of testing—is left unverified. Moreover, methods of research and analysis should be compared in order to find out which ones are more prone to triviality. We may sometimes want to check trivial statements, but our instruments should not force us to do so.

This brings out what is probably the most annoying aspect of this work. Like some other proponents of mathematical models and statistical analysis, Richardson is so sure of the scientific quality and theoretical superiority of his work that he is constantly sneering at intuitive and more qualitative approaches. Unlike Rashevsky, who is aware of the limitations on the usefulness of mathematical and statistical models so far and views their applications as illustrations of a method rather than as "hard" findings, Richardson feels that his models should direct the work of statesmen and political leaders. He actually tried to prevent World War II by sending copies of his essays to several diplomats. Mathematical models may one day be the only form of theorizing in the social sciences as well as the base of all political decisions, but Richardson's

work certainly does not encourage the belief that this is likely or that the day is very close.

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*Tradition, Values and Socio-Economic Developments.* Edited by RALPH BRAIBANTI and JOSEPH J. SPENGLER. Durham, N.C.: Duke University Press, 1961. Pp. viii+305. \$6.00.

When Victorian interest in social development was pushed aside by functionalism from its central position in social science, many fundamental problems were left unresolved and have remained so even within the less ambitious field of inquiry which has survived the attack on evolutionism and historicism. We return to these problems nowadays under radically different conditions both in the social sciences and in the world they study: and the interest of this publication from the Duke University Commonwealth Studies Center lies not so much in any major new contribution to the subject as in its reflection of the current state of the discussion. It enables us to ask whether the contemporary theory of social change is more advanced than its predecessors.

We inherit two main legacies from the labors of the past. One is the concern of the eighteenth and nineteenth centuries with the idea of progress and with the identification of stages and processes of evolution. The other is the nineteenth-century effort to understand the genesis and dynamics of industrial society. The first inheritance was tied to the colonial and imperial concerns of Europeans in contact with primitive and other technologically inferior peoples. The second was no less restricted by the particular conditions, and especially the class conditions, of indigenous Western industrialization.

Twentieth-century events have joined together and transformed these two sets of conditions, and the earlier restrictions have been replaced by possibilities for the study of social and economic development in a wider historical and comparative context. Moreover the impulse to this type of study is now sustained by what Wilbert Moore refers to as an evolving universal goal—the world-wide spread of