Sampson, S.
1969 Crisis in a Cloister. Ann Arbor: University Microfilms. Number 69-5775.

Shepard, R. and P. Arabie
1979 "Additive clustering: representation of similarities as combinations of discrete overlapping properties." Psychological Review 86:87-123.

Snyder, D. and E. Kick
1979 "Structural position in the world system and economic growth, 1955-1970: a multiple network analysis of transnational interactions." American Journal Sociology 84:1096-1126.

Steiber, S.
1980a "Quantitative models and methods for world-system analysis." The Sociological Quarterly 20:forthcoming.
1980b "International trade and domestic development: a cross-national assessment of dependent relations in the world-system." mimeo.

Stolzenberg, R.
1979 "The measurement and decomposition of causal effects in nonlinear and nonadditive models." Pp. 459-488 in K. Schessler (ed.), Sociological Methodology 1980. San Francisco: Jossey-Bass.

U.N.E.S.C.O
1968 Statistical Yearbook 1966. Paris: United Nations Educational, Scientific and Cultural Organization.

White, H., S. Boorman, and R. Breiger
1976 "Social structure from multiple networks I." American Journal of Sociology 81:730-780.

IN RELATION TO SCIENCE AND SOCIAL NONSCIENCE: A CRITIQUE OF PEARSON AND FISHER

Robert J. Wazienski
University of Kansas

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The objective set before us in this work is to explore the direction which social science has taken in this century. We do not intend to focus our attention on the nature and occurrence of phenomena, for these empirical notations are important only if one wants them to be. Furthermore, they are not relevant to science. The goal here is to simply distinguish, using a few isolated examples, between science and empiricism.

There seems to be remarkable confusion as to the distinction between science and empiricism. We must note that this confusion is not new.

> In ancient times there was no social science. What we call social science was then called social philosophy or political philosophy or ethics. Aristotle's political and ethical philosophy, for example, was the same as his social science. It was much latter, with the success of Newtonian science, that social thinkers felt that it was possible to create a modern social science similar to the modern natural sciences. This modern or positivistic social science would have one basic characteristic, it was thought: it would be scientific (Stern, p. 1).

It was thought, and believed, that this "scientific" approach along with the statements derived thereof could introduce the capability of interpersonal agreement. The worth of this notion is sound, in that it is consistent with the function of science and reflects an adequate goal of the social sciences. However, along the path from social inquiry to social science, a reversal of the stated intention has occurred. It will suffice to say that the discipline of sociology has advanced in a circular fashion, and that modern empiricists are nowhere closer to the scientific approach than they were three hundred years ago. Quite simply, they have increased their efforts only to lose sight of their goal. In essence, science has been put at bay for the sake of empiricism.

The task before us is to set down this scientific approach in the Galilean tradition, and to critically analyze the essential stance of two important authors in the social sciences; Karl Pearson and Sir Ronald Fisher. Although this task is fragmented and isolates only a few examples, it provides an intellectual impetus for detailed analysis.

The point of reference for this paper is the conception of exact science implicit in the work of Galileo Galilei's *Dialog Concerning Two New Sciences*. The work here cited is more than a mere discourse of Mechanica (statics) and Movimenti Locali (dynamics) but rather is a prescription for theory and methods in the scientific enterprise.

Three issues are isolated in this paper, for they seem to mark (account for) much of the divergence of Pearson and Fisher from exact science. They are abstraction, modeling, and rigidity. Each of these concepts have been misused or ignored in the sociological endeavor (theory and research) while they have been fundamental to the exact sciences.

Galileo knew what abstraction meant. Furthermore, he understood the difference between the abstract and the concrete. The distinction here is actually quite elementary if we consider the following:

> . . . I . . . call to your attention the fact that these forces, resist every movements, figures, etc., may be considered either in the abstract, dissociated from matter, or in the concrete, associated with matter. Hence the properties which belong to figures that are merely geometrical and non-material must be modified when we fill these figures with matter. . ." (Galileo, p. 112).

Notice the bent to Galileo's perspective. It implies that phenomena (physical and social) has, or can have, an abstract representation for studying it in the purest sense. It also indicates that modifications take place *after* abstraction.

Only with a conception of abstraction, can a modeling procedure (with laws and principles) be presented. It should be emphasized that this modeling procedure, as an interpreted geometry, must be a wholly and totally abstract representation. To this extent, a modeling procedure indicates the order of phenomena and not the opposite. ". . . the phenomena to be investigated is modeled and subsummed by the calculus . . . such that a dynamic picture is generated. This formulation then guides the whole of the experimental process. . ." (Willer, p. 3).

This statement, as a theoretical and methodological guide, is well grounded in the work of Galileo. As one example, " . . . we are confirmed mainly by the consideration that experimental results are seen to agree with and exactly correspond with those properties which have been . . . demonstrated by us" (Galileo, p. 160). That is, we have demonstrated it first!

By this we can see that abstraction is the necessary process from which the scientific enterprise proceeds. It is central to the work of Galileo and it is absent (for the most part) in social science.

We must also consider the most misunderstood term in social science today; that of rigidity (or rigor). When Galileo states "I propose to demonstrate in a rigid manner," he means precisely that he will define what is included in properties and procedures and what will be ignored. In conjunction with abstraction and modeling this rigor is practiced in the exact sciences.

The work of Galileo has set the premise for scientific achievement, yet this direction has been misunderstood or ignored in the application of theory and methods in the social science. Abstraction does not dominate as the preliminary step in social theory, as empirical observation has come to the fore. Modeling procedures do not order anything, but rather are set to the cast of empirical data. Rigor has been systematized to the point where uncertainty can be "rigorously" calculated. These attempts lead away from, and not toward, exact science.

We need only to turn to Karl Pearson to see how the aim of science is lost, to a degree, in regard to the science of society. He had provided a type of systematic empiricism that was not solely based upon observables but also based upon the prediction of events based on observables. The observables with which Pearson is concerned, is human society.

At the very beginning of his work, *The Grammar of Science*, Pearson states that "... a change has taken place in our appreciation of the essential facts in the growth of human society," (Pearson, p. 1) thus entrenching the need for scientific exploration of the human entity. This, in itself, is a noble task (although not an original one) and is deserving of some commendation. But, however clear the task may be, the "methods" lack severely. Consider the following statement (in a Weberian sense if you choose) with respect to its honesty and intent. "The scientific man has above all things to strive at self-elimination in his judgements, to provide an argument which is as true for each individual mind as for his own" (Pearson, p. 11). This statement is in reference to the role of science in society (citizenship) and taken by itself (the "self elimination of judgements" concept) is in agreement with a scientific endeavor. But consider this "method" now in relation to how unscientific the perspective truly is. "The classification of facts, the recognition of their sequence and relative significance is the function of science" (Pearson, p. 11). The observant reader of Karl Pearson would realize that these "facts" of which he writes so reverently are observables, or at best they are based on observables. Systematic empiricism thus operates under the guise of science for Pearson.

If the reader interprets Pearson to be thoroughly misled (wrong) in his conception of science, it is just as easy to believe that he (Pearson) is thoroughly confused instead; for Pearson does display that he might know what science is. "The discovery of Law (scientific law) is the peculiar function of the creative imagination"(Pearson, p. 32). This statement does not estrange Pearson from science in an inportant sense, but subsequent commentary does. The initial reaction is that Pearson has, after all, an understanding of abstraction, as this misconception is further supported: "The statement of this formula was not so much the discovery as the creation of the law of gravitation" (Pearson, p. 76). So where does Pearson's confusion of abstraction come in? The answer to this question is in his conception of scientific laws or "resumes." These "brief expressions" are nothing more than generalizations. As such they need to be separated from the abstraction of law in the exact science. Pearson failed to do so. He states that "The laws of science should be associated with the creative imagination in man" but contradicts himself by saying that "(S)cience endeavors to provide a mental 'resume' of the universe" (Pearson, p. 36). In essence he has associated the "creative imagination" with empirical generalizations.

In *The Grammar of Science* Pearson asserts the claim that our knowledge of the future is based on the past. "That the future will be like our experience of the past is the sole condition under which we can predict what is about to happen and so guide our conduct" (Pearson, 1957, p. 36). This statement shows a distinct misunderstanding of the meaning of science, for empirical suppositions cannot predict, regardless of how well they may guide. Furthermore, it bases the universe on an assumed (or regular) irregularity. In this case, the failure to recognize the use of abstraction has led to the formulation of a regularly inexact "science" where independence and equivalency are excluded.

We should turn back to Galileo to see that Pearson missed two essential points: namely abstraction and modeling. Pearson does not separate abstraction from generalizations. To Pearson these generalizations are seen as "laws" which represent a degree of accuracy in empirical relationships yet to occur. In science a "degree of accuracy" could not lead us to any law or prediction. The issue is further confused in that Pearson believed that experience does not show an exactness in any relationship, and therefore, equivalence between any relationship is not possible. It is clear that an understanding of the purpose of abstraction is missing. Secondly, his "modeling" procedure is a simple accumulation of the "facts." A model is not derived from "facts." Its' predictive ability lies in its level of abstraction. Empirical generalizations (and the probabilities thereof) produce a "science" of uncertainty which cannot predict, define or even justify itself. In this regard (and for other implied reasons) his discourse on "Cause and Effect—Probability" is a moot point.

By the time Sir Ronald Aylmer Fisher had completed *The Design of Experiments,* systematic empiricism had become thoroughly confused with (and mislabeled) science. "The history . . . of systematic empiricism in sociology is concerned with the development of systematic methods of association, relation, and induction and was completed when Fisher provided a method of association by significance tests" (Willer, p. 16).

To most empiricists, the procedures and practicality of designing research has come to the foreground. The concern is with setting up and evaluating research designs rather than with the logical process of going from "association to relation to induction to produce an empirical generalization" (Willer, p. 61). What has evolved is not the "rigid" process that Galileo speaks of, but rather a "rigid" process whereby uncertainty is calculated rigorously. "We may at once admit that any inference from the particular to the general must be attended with some degree of uncertainty, but this is not the same as to admit that such an inference cannot be absolutely rigorous" (Fisher, p. 4). This statement prescribes nothing more than a rigorous calculation of uncertainty. Furthermore, any induction at this point has no promise (mathematical or theoretical) from which to proceed.

In his calculation of uncertainty Fisher has adopted the normality assumption and in effect uses the equation for the normal curve along with measures of central tendency and dispersion. The resulting error terms are applied statistically to associations.

> . . . estimates may be made, which in themselves contain the whole of the information available for finite samples. These especially valuable and comprehensive estimates are called sufficient statistics, and the great simplicity of problems, which fall under the head of theory of errors, is due to the fact that with the normal distribution both the quantities requiring estimation, the mean, and the variance, posess sufficient estimates. It is for this reason that in so much experimental work we need only be concerned with the precision of the total, or mean, of the values observed, and with the estimation of this precision from the sum of squares of the residual deviations (Fisher, p. 248).

The implications of this are surely complex in mathematical form. But this is where complex mathematical formulation is muddled with the elements of science. The above statement does not involve a theoretic calculus based on an abstract model. It is based on observed error terms (deviation) or probablistic error terms resting on the binomial expansion. It (itself) is precise, but only to the extent that it is a precise measure of non-determination.

Furthermore, Fisher's method assumes the interpretations of classical probability theory (based on a-priori assumptions) while employing a relative frequency model (an a-posteriori approach). The effects of this, contradiction not withstanding, are misleading to the extent that the sum of squares principle cannot be attended to in any predictive sense. Yet Fisher's approach rests on it and is commonly called "scientific."

A further comment must be made in light of the foregoing discussion. Although the analysis of variance technique is a test of difference in means, it is based upon the comparison of variances. This further aborts scientific modeling procedure by leaving certain values "free to vary."

To Fisher, testing a null hypothesis was simply a matter of distribution and randomness. His purpose was with the determination of randomly distributed differences and had very little to do with association. In effect, a test of a random distribution is a test of the data inclusive of that distribution. It is not truly a test of association. Fisher extends the application beyond a simple affirmation of distribution and randomness to that of association. What he has come up with, in reality, is a false faith in randomness.

> Having decided that . . . our estimates of the error of the average difference must be based upon the discrepancies between the differences actually observed, we must next inquire what precautions are needed in the practical conduct of the experiment to guarantee that such an estimate shall be a valid one. . . . Randomization properly carried out . . . ensures that the estimates of error will take proper care of all [differences], and relieves the experimenter from the anxiety of considering and

estimating the magnitude of the innumerable causes by which his data may be disturbed (see Fisher, p. 46-50).

Carter, Naboodiri and Blalock state an interesting corrolary to this.

> Sir Ronald Fisher . . . was once asked: What would you do if, drawing a Latin square at random for an experiment, you happaned to draw a Knut Vit square? Fisher remarked that he would discard the selection and draw again. . ." (Carter et al., p. 13).

The commentary to this is that analysts should avoid random arrangements which are clearly patterned. If this is so, then the concepts of randomization is valid only to the extent that it reflects a-posteriori probability assumptions. Beyond this point, randomness is discarded. This is not science—it does not even resemble scientific procedures. It surely is systematic, but such a badge is not "holy" enough for persons of this discipline to wear.

On the basis of what has been written here it seems clear (even in so few examples) that Fisher and his method are not scientific. Fisher does not mention abstraction, and his "models" (experimental designs) are applied to mathematics as if they and their mathematical formulations had some meaning scientifically. They do not.

We must make it clear that this paper is not intended to simply point out the weaknesses of Pearson and Fisher. Nor is it an attack on systematic empiricism. It is an explanation, illustrated through Pearson and Fisher, that systematic empiricism is not science, and that the scientific tradition in sociology needs redirection. An understanding of Galileo and Einstein would provide an excellent starting point. Furthermore, this paper is not intended to be "anti-empiricism." It is intended to show only that there is a difference between science and empiricism, and that this difference should be understood and given proper recognition.

REFERENCES

Carter, Lewis, N. Krishnan Namboodiri, and Hubert Blalock
1975 Applied Multivariate Analysis and Experimental Design. McGraw Hill. New York.

Fisher, Sir Ronald A.
1935 The Design of Experiments. Oliver and Boyd. London.

Galilei, Galileo
1954 Dialogs Concerning Two New Sciences. Trans. Henry Crew and Alfonso de Salvio. Doves Publishing, Inc. New York.

Pearson, Karl
1937 The Grammar of Science. Dent. Ltd. London.

Stern, Herold S.
1973 Social Nonscience: A Critique of Social Science. Stipes Publishing Co. Champaigne.

Toulman, Stephen
1960 The Philosophy of Science. Harper and Row. New York.

Willer, David and Judith Willer
1973 Systematic Empiricism: Critique of a Pseudoscience. Prentice Hall. Englewood Cliffs.

Willer, David
1976 "What is Exact Theory?" Research paper presented to the National Science Foundation.