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THE ROLE OF THEORY IN SOCIAL RESEARCH

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Some will perhaps consider it presumptuous for one who has come to be known primarily as a theorist to talk about empirical research. I have done some work in the latter field and hope to make some contributions to it before many years. But apart from that I can perhaps but cite a statement of which Max Weber was fond, "In order to understand Caesar it is not necessary to have been Caesar." So perhaps it is possible for one who has not been quite so completely immersed in empirical research as some of you, but who has nevertheless been a good deal concerned with certain of its problems, to help illuminate them.

I should, however, like to make myself clear in advance. The current state of sociological science is not such that anyone is entitled to dogmatize with an air of canonical authority. To my mind the only hope of reaching that fundamental unity of outlook and purpose which I think almost all of us feel should actuate the workers in a field of science, is to attempt seriously, objectively and respectfully to learn from each other's work, thought and experience. It is my very strong conviction that, if the current situation be approached in this spirit, and the trouble taken to get to the bottom of other people's problems, there will turn out to be far more unity on fundamentals than appears at first sight.

What I can do on an occasion like this seems to me to be to present to you with due humility for your consideration a few of the results of my own thinking about and experience in sociological work.

Practically all competent observers are, I think, agreed that there is a basic difference in the situation in such sciences as physics and chemistry, and those in the social group, particularly sociology. I do not mean to deny that the former group have their crucially important unsolved problems and areas of controversy. These, however, occur either on the frontiers of the technical part of the science itself or in the field of the interpretation of the broad significance of its results as a whole. But there is a very substantial core of material on which there is entirely general agreement. What should go into the more elementary courses in these fields is not in controversy. Moreover this common core is not only a body of discrete miscellaneous facts—it is closely integrated with a logically elaborated body of theory, much of which, like the fundamental equations of dynamics in mechanics, is stated in a highly generalized form.

1 An Address delivered before the Annual Institute of the Society for Social Research, at the University of Chicago, summer 1937.
When we turn to the social field we find a very different situation. Most of our controversial problems seem to be not on the periphery but at the very starting points of the field. There is widespread feeling that we must settle the deepest current controversies before we can do anything. This at least seems to be true of those who feel the need to reach high levels of generalization. This feeling of fundamental uncertainty is vividly brought out by the controversial nature of the content of elementary courses.

It is scarcely possible to consider this situation without being struck by what is both one of the most conspicuous and, to me, one of the most disquieting features of the current situation. That is, there is a tendency to the development of a very deep hiatus between the more empirically and the theoretically minded workers in the social science fields.

I do not mean merely that there is a good deal of division of labor, as I am told there is in physics, between men whose work is primarily in the laboratory and those who work only with reports of laboratory results, pencil and complicated mathematical formulae. It is rather the tendency to a complete divorce, a mutual repudiation of the legitimacy of each other’s work and interests, which is disquieting. Certain of the empirically minded are not merely not interested in attempting to contribute to theory themselves, they are actively anti-theoretical. They consider any work in theoretical fields as positively pernicious and contrary to the canons of science. It is speculation, sterile dialectic, metaphysics or even mysticism.

On the other hand, many of the persons best known as theorists have not only not themselves made distinguished empirical contributions, they have often given the impression of not caring very much at least about the kind of empirical work which the empiricists have done, of having rather a sovereign disdain for the arduous difficulties of the empirical fields.

I do not propose to dwell mainly upon the shortcomings of the empirical schools. A very large part of the difficulty seems to me to lie on the theoretical side of the controversy. Many features of our theoretical traditions seem to me seriously to have inhibited the potential usefulness of theory for empirical research. One important reason why the empiricists have tended to be anti-theoretical is that they have, often rightly, seen much to object to in the particular brands of theory they have had held up to them.

But before going further into these questions it is necessary to state certain premises which seem to me fundamental, even though they imply a drastic repudiation of certain forms of empiricist position. Sympathy with a person’s motives and feelings in a situation does not necessarily imply endorsement of the position he takes.

In these terms I must categorically disagree with the view that any empirical science can be developed to a high point without reference to generalized conceptual schemes, to theory. The process of the growth of scientific knowledge is not a process of accumulation of discrete discoveries
of "fact." In the first place our study of fact, however little we may be aware of it, is always guided by the logical structure of a theoretical scheme, even if it is entirely implicit. We never investigate "all the facts" which could be known about the phenomena in question, but only those which we think are "important." This involves a selection among the possible facts. Now if we investigate carefully, though few empiricists do, what is the basis of this selection, it will, I think, uniformly be found that among the criteria of importance and the only ones of strictly scientific status is that of their relevance to the logical structure of a theoretical scheme.

Secondly, few if any empiricists, being as they usually are truly imbued with scientific curiosity, are content simply to state bald, discrete facts. They go beyond this to maintain the existence of relations of interdependence, causal relations. It is stated not merely that the steam railroad was developed and certain kinds of industrial developments took place, but that without the invention of the railroad these developments could not have taken place—that the invention of the railroad was a causal factor in industrial development.

Now I wish to assert that such an imputation of causal relationship cannot be proved without reference to generalized theoretical categories. If it is asserted, the assertion is logically dependent on these categories whether they are explicit or implicit.

If this be true, the alternative for the scientist in the social or any other field is not as between theorizing and not theorizing, but as between theorizing explicitly with a clear consciousness of what he is doing with the greater opportunity that gives of avoiding the many subtle pitfalls of fallacy, and following the policy of the ostrich, pretending not to theorize and thus leaving one's theory implicit and uncriticized, thus almost certainly full of errors.

This assertion of the inevitability of theory in science naturally cannot be proved on this occasion. The next best thing is to cite authority. Alfred Marshall was an economist who so far as I know has hardly seriously been accused of tender-minded disregard for fact. In an address at the University of Cambridge he made a striking statement which exactly expresses my feeling: "The most reckless and treacherous of all theorists is he who professes to let facts and figures speak for themselves."² If Marshall stated this point in a most striking form, I think Max Weber may be said definitely to have proved it. It was one of his greatest methodological contributions definitively to have refuted the claims of the German Historical Schools that it is possible to have valid empirical knowledge of causal relationships with no logical implication of reference to generalized theoretical categories.

But if generalized theory is essential to science, it does not follow that anything and everything which goes by that name is of equal value. Quite

the contrary, there is much to object to in a great deal of what has gone by the name of sociological theory. Empiricists are, as I have said, right in repudiating much of current theory though that does not justify them in extending this repudiation to all theory in principle simply because it is theory.

Indeed, that there is something wrong with current social theory seems to me to be clearly indicated by the fact that there is such drastic lack of agreement and that most people who write and talk about it feel impelled to divide theorists up into "schools" which, it goes without saying, are mutually incompatible so that a person who agrees with one school in almost any respect, must by definition oppose all other schools in all respects.

This deplorable situation seems to me in large measure due to a failure to distinguish adequately the various conceptual elements which either go to make up, or have become associated with, what are generally called theoretical structures in science, particularly in social science. I should like to distinguish three classes of such elements and put forward the thesis that much of the difficulty is due to modes of conception of and undue emphasis on two of them, resulting in distortion of the significance and role of the third.

1. No science develops in a vacuum, either intellectual or social. The scientific content of an intellectual tradition is always closely interwoven with elements of a different character. So far as these elements are conceptually formulated, they may be called for present purposes philosophical elements. The problem of the relation of scientific and philosophical ideas intermingled in the same body of thought has been a prolific source of trouble in social as in other science.

With regard to this problem thought seems to have tended strongly to get itself into a dilemma: One horn of the dilemma is the view that the scientific and philosophical components of a body of thought must necessarily be bound rigidly together in a single completely determinate system. The inference is that a body of scientific theory, if it is logically coherent, is simply an aspect of a philosophical system and none of it can be accepted without accepting the system as a whole. Thus the critics of classical and neoclassical economic theory have often held that acceptance of the theory for even the most elementary purposes implied the acceptance of the whole rigid philosophical system, extreme rationalism, psychological hedonism, utilitarian ethics and the rest. Conversely it has often been held that it was impossible to be confident of even the most elementary theoretical proposition, such as that the value of money is an inverse function of its quantity, without first settling definitively all the problems of the complete philosophical system on which it supposedly depends.

There is a very widespread and justified feeling that philosophical theories cannot claim the same order of objectivity and verifiability as the propositions of empirical science. Hence it is not surprising that people who
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disliked these implications should, without questioning the premises on which they rested, attempt to evade them by repudiating theory altogether. After all, to be responsible for a complete philosophical system, once the first innocent step in theoretical reasoning is taken, is a rather terrifying prospect. In this dilemma my sympathies are definitely with the empiricist.

But I cannot accept the dilemma. In my opinion the whole thing rests on a serious misconception of the relation of scientific theory to philosophy. I do not believe either that scientific theory has no philosophical implications, or that it involves no philosophical preconceptions. They cannot, in that sense, be radically divorced. But at the same time it does not follow that they are rigidly bound together in the sense this dilemma implies. On the contrary, though they are interdependent in many subtle ways, they are also independent. Above all it is perfectly possible for a scientist, even a theorist, to get ahead with his work without worrying about a philosophical system in general, but only considering philosophical questions one by one when and as they directly impinge on his own scientific problems. Indeed, this false dilemma is the principal source of the charge that theorizing is necessarily "metaphysical" and has no place in science.

Please note, I do not say that scientific theory should never concern itself with philosophical problems. But I do say that its burden can be enormously lightened if it divests itself of unnecessary philosophical concerns; and it can do this to a far greater extent than is generally believed, especially by empiricists.

2. The second type of conceptual element involved in bodies of theory which I wish to discuss is one which falls within the competence of science strictly construed, is hence not philosophical, but has, I think, received quite undue prominence especially in sociology. This is the element of what may be called "broad empirical generalization." Examples are such propositions as "the course of social development as a whole follows a linear evolutionary course," or "social processes are in the last analysis determined by economic (or geographical, or racial, etc., etc.) factors." Such "theories" embody a generalized judgment about the behavior of, or causes in, a hugely complicated class of empirical phenomena. They are analogous to such judgments as "the physical universe as a whole is running down."

Indeed it is in terms of such views, if not their philosophical positions, that sociological theories are usually classified. We have evolutionary vs. cyclical theories, economic, biological, religious interpretations.

Here again we find a dilemma. For we may well ask, how are propositions such as these to be proved? Where are the specific observations, the patently rigorous reasoning? If the proof is as cogent as their proponents claim, why the warring schools? Why cannot people be brought to agreement? The empiricist quite understandably begins to suspect it is because there isn't any such evidence, or it is woefully inadequate to the conclusions. Hence so far as theory in general is identified with this kind of thing, it is
held to be "speculative," only for people who have not absorbed the discipline of scientific caution, of asserting only what they can demonstrate. Here again my sympathies are with the empiricist. I do not think the great majority of propositions of this order have been or are capable of being rigorously demonstrated. Critical examination of them will reveal scientific defects of one kind or another.

3. So, if scientific theory in the social field consisted only of these two classes of elements, there would be much reason to follow the empiricist's advice and eschew it altogether. But I am confident that this is not the case, there are other elements as well which the usual empiricist indictment is prone to overlook, what I should like to call generalized analytical theory. This it is which seems to me to be the most important kind of conceptualization in the physical sciences.

Empiricists are often fond of maintaining that they emulate the physical sciences. It is my suspicion that they are able to make this claim partly because analytical theory has in such fields become so completely integrated with empirical research that it is completely taken for granted—no one feels it necessary to talk about its role because it seems obvious. After all, mathematics in its application to physics is theory.

Analytical theory in the sense in which I mean the term here, is a body of logically interrelated generalized concepts (logical universals) the specific facts corresponding to which (particulars) constitute statements describing empirical phenomena. Use of this concept in empirical research inherently tends to establish logical relations between them and their particular content (values) such that they come to constitute logically interdependent systems. Correspondingly the phenomena to which they apply come to be viewed as empirical systems, the elements of which are in a state of mutual interdependence.

Much the most highly developed analytical system in this sense in the social field is economic theory. Indeed, economists alone have among social scientists been steeped in an analytical system. But precisely because of the difficulty of clarifying the relation of this analytical system both to empirical reality itself and to the other types of conceptualization just discussed, even economics has not been spared an empiricist revolt, the institutionalist movement, which, though probably now passing, has in this context done a great deal of damage and threatened to do more.

Indeed, one important reason for the apparent backwardness of analytical theory in the social sciences is the greater formidability of these difficulties here as compared with the physical sciences. Those concerned with ordinary biases and with the often difficult distinctions between scientific and philosophical considerations I shall leave aside. But two others are of such great importance that I should like to say a few words about them: (1) Even in mathematical terms it is difficult to handle a system involving more than a very small number of variables. Where for various reasons
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mathematical treatment is excluded, as it is in most of the social field, or severely limited as, I think, it is in all, there is a very strong impetus to simplification of problems by dealing with only a few variables in a system.

This inevitably implies that analytical theory in the social field is highly abstract. For the values of the variables of such a system state only a very limited number of facts about the concrete phenomena to which it applies. It is very seldom that other elements are sufficiently constant within any very wide range of variation of these variables so that trustworthy interpretation and prediction can be based on the laws of this analytical system alone. It needs to be supplemented by considerations involving the others as well. This is one of the most important reasons for the unsatisfactoriness of proceeding directly to broad empirical generalization. The case of some of the deductions from economic theory is an extremely vivid one. The facts relevant to any system of analytical theory are never all the facts knowable about the phenomenon in question, and only part of these are the values of variables. (2) A variable is a logical universal or combination of them. Its "values" are the particular facts which correspond to this universal. These facts are or can be obtained in one and only one way—by empirical observation. But it is the essence of the ordering function of theory that any old facts, however true, will not do, but only those which "fit" the categories of the system. What facts it is important to know are relative to the logical structure of the theory. This is not to be understood to mean that theory should dictate factual findings, but only the definition of the categories into which the findings are to be fitted.

Precisely here is one of the crucial problems of the relation of theory to empirical research. For theory to be fruitful it is essential that we have research techniques which provide the right kind of facts. There is, indeed, evidence that this is one of the most serious difficulties, that a great deal of current research is producing facts in a form which cannot be utilized by any current generalized analytical scheme. This is a very complex problem. I can comment on only one phase of it.

One important group of social empiricists is particularly partial to measurement. They point out the extreme importance of measurement in physics and conclude that only so far as its facts are the results of measurements can sociology claim the status of a science. I do not wish to depreciate the value of measurement wherever it is possible, but I do wish to point out two things: First, the importance of facts is relative to the way in which they can be fitted into analytical schemes: measurements are fundamental to physics because many of its variables are such that the only facts which make sense as their values are numerical data. But numerical data are far less scientifically important until they can be so fitted into analytical categories. I venture to say this is true of the vast majority of such data in the social fields.

Second, measurement as such is not logically essential to science, how-
ever desirable. Measurement is a special case of a broader category, classification. It is logically essential that the values of a variable should be reducible to a determinate classification. But the classification they admit of may be far more complex than the single order of magnitude which measurement requires. Where nonmetrical, even nonquantitative data can, with the help of such classification, be made to fit directly the logical structure of an analytical scheme it may be possible to establish relations of crucial importance which any amount of numerical data lacking such analytical relevance could not bring out.

In conclusion I may state schematically what seem to me to be the principal functions of analytical theory in research.

1. In the vast welter of miscellaneous facts we face it provides us with selective criteria as to which are important and which can safely be neglected.

2. It provides a basis for coherent organization of the factual material thus selected without which a study is unintelligible.

3. It provides a basis not only of selection and organization of known facts, but in a way which cannot be done otherwise reveals the gaps in our existing knowledge and their importance. It thus constitutes a crucially important guide to the direction of fruitful reasearch.

4. Through the mutual logical implications of different analytical systems for each other it provides a source of cross fertilization of related fields of the utmost importance. This often leads to very important developments within a field which would not have taken place had it remained theoretically isolated.

Finally, it may be asked, have the social sciences outside of economics any analytical theory at all to use? Must we not remain empiricists through sheer lack of anything else to turn to? I do not think so. I believe there is far more analytical theory in use than many of us realize. We have been, like Molière's hero, speaking prose all our lives without knowing it. Moreover, in a work recently published I have traced a process of development of analytical theory of the first magnitude including, I believe, a demonstration of its fruitfulness in empirical research. I am convinced that investigation would show that the ramifications of this development reach far beyond the limited group of workers with whom I have explicitly dealt.

My closing plea is then: Let us take what we already have and both use it to the utmost and develop it as rapidly as we can. Let us not either through failure to understand what it is that we have or through disillusionment with its very real shortcomings, throw it overboard to the tragic detriment of the interests of our science. If it is used and developed through the intimate co-operation of empirical and theoretical work, I am very hopeful for the future of sociological science.

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