THE NECESSITY OF PREDICTION IN THE SOCIAL SCIENCES

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ONE OF the commonest criticisms of the social sciences is that the basic abstractions (terms) from which they proceed are unrealistic. It is objected (1) that nothing in the real world corresponds to the basic terms of the social sciences, and (2) that factors are so abstracted from their contexts that essential details are left out. To meet these criticisms two remedies are usually suggested: (1) that only those terms be used which correspond to events or entities in the real world, and (2) that situations be studied in their totalities.

For example it is argued that, since there are no "economic men" and no "pure competition" (or "pure monopoly" or pure anything else), the formulations of economics (and other social sciences) will lead nowhere and should be modified to take account of the facts of a much more complicated world. This kind of suggestion is probably made more often by social scientists themselves than anyone else. Moreover, workers in anthropology, sociology, economics, and psychology find that there are no real boundary lines between their fields. It is easy therefore to feel that the splitting up of social science into discrete areas of study must of necessity result in a fundamental elementalism which may well render social science sterile, and that the only cure is interdepartmental cooperation. If, in studying such phenomena as "power," "personality," and "culture," each department abstracts from the "whole," the way to arrive at better results would appear to be to combine the departments in the hope that this will bring about a combination of their abstractions that will leave out nothing of importance.

I SHOULD LIKE to take a position with regard to this sort of criticism (and the anxieties it produces among many social scientists) which at first may seem a little odd for a student of general semantics but which is, I feel, entirely in keeping with the general semanticist's desire to achieve greater predictability. The gist of this position is that criticism of the social sciences of the kind outlined above is usually *out of order.* When we criticize on such grounds *before*

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determining the predictive adequacy of the theories we build out of our abstractions, we are criticizing prematurely. The case in defense of this notion is quite simple: how can we tell whether false-to-fact maps are being produced unless we test them? What is the use of "correcting" theories which have never been tested? If no one knows, for example, just how much can or cannot be predicted from demand curves because no one has ever seriously tried to find out, what can be the sense of condemning propositions involving the abstraction "demand curve"?

We should be able to agree that, in general, very few predictions are made by social scientists and that they do very little testing. The fact that theories come and go in social science does not always mean that the theories going out have been tested and found wanting while the theories on the way in have been tested and found superior. There has been, for example, in psychology a long historical procession of schools of thought. But this procession of schools is to a notorious degree more of a fashion parade than the gradual displacement of old theories by newer ones with greater predictive value.

I believe it is not too much of an oversimplification to say that many social scientists have been so busy trying to make their basic abstractions correspond to the world that they haven't taken enough time to make predictions. That is to say, one of the fundamental reasons for the relatively slow pace of development in the social sciences is the improperly timed concern with the kind of criticism I have mentioned. Let me illustrate my point with a fictitious example.

Let us suppose that there has been published in one of the journals a paper which proposes a theory dealing with falling bodies. The theory describes bodies as "physical objects" and relates their time of fall to distance and some constant of acceleration. Let us suppose that this theory is greeted in the way theories are usually greeted in the social sciences.

After the usual lapse of time a rash of papers will appear which raise objections, including, *inter alia,* the charge that the description of bodies as "physical objects" is a gross oversimplification, leaving out all sorts of essential details such as weight, size, shape, composition, and temperature; the further charge that the time and place of fall, the orientation of the time-keepers, the accuracy of the instruments and the reliability of the personnel, etc., all introduce complicated variables which need to be considered.

So these scientists set to work to develop definitions of "bodies" which will correspond to "real objects"; they seek ways in which to discuss falling bodies as "contextual wholes." All this painstaking activity will seem very reasonable since the assumptions of the original theory are clearly false-to-fact, for certainly there are no "physical objects" except in contexts; obviously every "physical object" is different; it is only natural to expect that the observer will influence the observed and correlations of interpretation must be established.

A great deal of ingenuity is then put into constructing a scheme which will rank bodies in terms of their size, shape, weight, composition, etc., in order to find the different combinations of each which would be equivalent for purposes of theory-construction. It is soon discovered that no absolute scale can be created; hence a system is developed which only gives rank ordering where it is meaningless to inquire about the absolute differences between rankings. At about this point the problem becomes too difficult for all except a few persons who have the patience and the mathematics to carry on. The problem of falling bodies therefore drops out of fashion, while the opponents of science get another chance to reiterate their cry that the world is *much* too complicated to be understood by scientists and that what we need is a return to Faith.

NOW EVERYBODY knows that the way to stop all this nonsense is to take a couple of cannon balls of different size and drop them from the top of the Leaning Tower of Pisa. What happens to the details which were left out: weight, size, shape, composition, temperature, time and place of fall, orientation of timekeepers? The answer is that in this case these details were unimportant. No one could know for sure in advance that they would not be important, but also *no one could know for sure in advance that they would be important.*

I believe we are forced to the conclusion that there is not much use in questioning the basic abstractions of a theory which can be tested until we have tested it. If "completeness of detail" refers to the inclusion of all the details which are important (i.e. which will affect the result), then again, no one can say whether such completeness has been obtained in any theory until it has been tested. "Taking situations as contextual wholes" can become a monumental task of including more and more details until we have such a complicated theory that it predicts nothing correctly. Is such a theory likely to yield better predictions if we add still more details?

A theory can be useless because it includes too many factors just as easily as it can be useless because it includes too few. There would be no sympathy for the student of mechanics who contested the professor's equations on the grounds that they ignored friction and he "could not imagine a world without friction."

THIS PROBLEM of where to stop trying to include all relevant factors is not just a problem for the theoretician. The ("hard-headed, practical") business world runs afoul of it too. Consider the following example from my own work, terminal and transportation planning.

One of the cardinal needs in air transportation is an adequate prediction of the air traffic which will be developed between any two cities given different classes of service. If one could know how many passengers would move between different pairs of cities with one or another type of service, even within rather wide limits, the task of developing air route service patterns, improving utilization of equipment, certificating new stops, and a host of other practical problems could be much more easily solved.

Now there are many different approaches to this problem. We can take the classic formula, population times population over distance; we can modify it for unusual advantages over existing ground transportation, the type of city being studied, the density of cities in the surrounding area; we can make allowances for the distances of airports from cities, the number of schedules, the hours of arrival and departure, the aggressiveness of the airlines and airport personnel; we can put in corrections for the historic community of interest between any two cities by measuring the mail flow and telephone calls; we can study hotel registrations and convention attendance; we can investigate branch business establishments between pairs of cities. With a good imagination and a better budget, there is almost no limit to the number of factors which can be considered.

Indeed, there are more factors which can be introduced into this problem (from among the "infinite" characteristics of events at the process level) than there are pairs of cities to be studied. Anyone who has approached the problem of predicting air traffic between pairs of cities knows the hopeless feeling of dealing with "the situation as a whole." But most analysts confronted with the problem continue to try to consider "all relevant factors." Hence, a standardized technique for estimating passengers between pair of cities has *not* been developed by the industry, by the cities, or by federal agencies. Each new problem is approached *ad hoc*.

BUT LET US stop a moment. Taken alone, almost any of the ways of predicting air traffic tells *something*. The information we are seeking need only be approximate, and a number of mistakes can be taken in stride. Would it not be wise to take several of the simpler techniques for predicting air traffic and find out *how* reliable they are before complicating the problem? What use is it to develop complicated methods when even the simplest have not yet been tested?

Korzybski taught that the advance of science depends on a cycle consisting of the development of abstractions, the making of predictions, and the testing of these predictions. We cannot get far if we spend all our time on the first stage. We can establish ever more refined procedures for the development of the *initial* abstractions, but the final arbiter must remain the testing of the predictions which can be drawn from them. Where 100 per cent correct predictions are sought and no attention is paid to tests, there is little opportunity for advancement. It would seem much wiser, then, to take the least involved theories first, run through the cycle, and find out to what extent the predictions are correct. Then, when we find that we are making 50 per cent correct predictions instead of 40 per cent, we shall know that we are at least making progress.

In this way, by knowing *how* correct our theory is (instead of knowing merely that it is "wrong"), we can move ahead without worrying about the fact that the accuracy of our predictions still falls short of that of the physical sciences. We should pay less attention to the question, "Is social science as good as physical science?" and more attention to the question, "Is social science today better than social science yesterday?" The latter question can be answered by a comparison of the correctness of yesterday's predictions with today's—a comparison almost impossible to make at this date in view of the scarcity of recorded predictions and tests.

We are off on the wrong foot when we criticize our basic terms in the social sciences as being "oversimplified" or as being "mere abstractions." If no predictions have been made, tested, and found wanting, it is certainly not yet time to revise our abstractions. And, of course, if no predictions are ever to be made, no amount of inclusion of detail and no refinement of concept can ever demonstrate anything.

As we observed in the Introduction, cultural unity in America consists in the fact that most Americans have most valuations in common, though they are differently arranged and bear different intensity coefficients for different individuals and groups. This makes discussion possible and secures an understanding of, and a response to, criticism.

In this process of moral criticism which men make upon each other, the valuations on the higher and more general planes — referring to *all* human beings and *not* to specific small groups — are regularly invoked by one party or the other, simply because they are held in common among all groups in society, and also because of the supreme prestige they are traditionally awarded. By this democratic process of open discussion there is started a tendency which constantly forces a larger and larger part of the valuation sphere into conscious attention. More is made conscious than any single person or group would on his own initiative find it advantageous to bring forward at the particular moment. In passing, we might be allowed to remark that this effect — and in addition our common trust that the more general valuations actually represent a 'higher' morality — is the principal reason why we, who are convinced democrats, hold that public discussion is purifying and that democracy itself provides a moral education of the people.

GUNNAR MYRDAL, *The American Dilemma.*