THE PROCESS OF THEORY-RESEARCH INTERACTION

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Political theorists, in my view, often work in unfortunate isolation and are remiss in their important function of telling political scientists what political science ought to know. What political science ought to know, of course, is a most complicated matter and really designates an ideal of a body of interconnected propositions summarizing political human relationships with a maximum of precision. Few will doubt, but still fewer will observe practically the injunction that the political theorists must present research workers with the existing contradictions of important theoretical propositions. If Jefferson says that the age of reason and democracy has arrived and De Tocqueville says that democracy is an unreasoning leveling that contains the seed of its own destruction, the theorist can help his science by indicating that these are (historically and/or today) important propositions, that they involve, together with the other writings of these gentlemen, a contradiction soluble by empirical study, and that he, the theorist, would be glad to rework the propositions to make them testable. Thus would begin to emerge a greater rapport between theory and research.

Unfortunately, the annals of science contain very little in the way of descriptions of the actual process by which theory interacts with research. It is nice to know that they must support one another, but following the process with one's own eyes or through the eyes of another would be an improved way of appreciating the relationship. Formal logical analysis can go back on a contribution to knowledge and show how it "must have been" constructed by combined theory and fact. However, the logic of "must have been" turns out to be always a synopsis of the finding, relieved of the burden of historical and sociological description. Graham Wallas perhaps stands alone among political scientists as one who essayed to depict the mental and social process of scientific discovery.

Great theorists are themselves puzzled to explain the origins of their contributions. Albert Einstein once confessed that "intuition" inspired some of his work in relativity and "intuition," we must ad-

mit, is an easy synonym for the great unknown. A somewhat cynical young chemist, credited with a new formula and asked to explain its origins, declared that his own theory-research interaction process consisted of studying first under one great teacher, then under another, and then combining the two separate wisdoms into a third wisdom, born of the failure of the two great teachers to communicate with one another.

It would seem that even a faint illumination of the area of the sociology of research would be welcome. Therefore, we may inquire more deeply into the circumstances under which "posing the question," which was referred to above, may assume more than its formal meaning. The problem of posing questions may be abstracted formally in talking of theory, but, sociologically speaking, it is very much dependent upon what incites the question and what happens to the question. The task of the theorist neither begins nor ends with the posing of the question. The failure to appreciate this is an important defect in the scientific temper of many political theorists.

Political theorists must, in greater numbers than hitherto, be participant observers in the process of theory-research interaction. They must add themselves, intellectually and/or physically, to the impressive research teams and research tendencies working in social science today. And the addition must be, if not by total immersion, at least by more than a sprinkling. For the theorist becomes useless if he retires after bringing forth a good — indeed a very good— idea. The hardest part of political theory begins only when the motley crew of research workers, coming from all kinds of political science and all kinds of psychology, economics, sociology and anthropology, ask difficult and enervating questions. If they are not appeased, they set up their own theory shops, getting along as best they can by themselves.

I. AGREEMENT ON STUDY PROBLEMS

In the first place, no sooner will the political theorist advance what he considers an important theory than he will be asked: What is so important about the problem you bring us? Here is where the first crisis comes. Anyone who has spent time, as has this writer, with research teams composed of psychologists, sociologists, and economists will agree, I believe, that each discipline has curious ideas about what is important. Psychologists often tend to be anti-his-

torical; they do not like to talk about trends as do political scientists; economists talk of trends and sociologists do as well, but they have their own peculiar trends to study and their own techniques for studying them. The favorite trend of the political scientist is often an ideological one, congenial primarily to the European sociologists of knowledge. The Lewinian phychologists, a small group, are fine experimenters; their psychological theory is amenable to trend study, but they are quite anti-historical. The cultural psychoanalytic approach is trend-oriented, but its representatives are having almost as much difficulty as the political theorists in getting down to corroborative research.

So, certainly, one cannot find friendly research workers everywhere when he wishes an important problem of political theory to be tested. He had best organize his own team and do the job himself in that case. If he is wise, he will frame his study so that the methods and evidence will please the high standards that have been developing in the newer research fields of social science. An example of this theory-research interaction is the work of Elton Mayo.

Of course, at this point, one may pause to wish that the social sciences might speak a common language and accept a common currency of findings. But one sees little possibility of this unless some extraordinarily strong intellectual movement can manage to get beyond its starting point, with well-placed allies working hard at converting its symbols and propositions into interpretations that help economists, sociologists, psychologists, and political scientists solve their crucial problems. We have to assert that at the present time a political scientist may increase his own progress by doing this translation as he moves through his materials. He cannot rely on his important problems forcibly impressing many other social scientists.

II. RELATIONS OF GENERALIZATION AND TECHNIQUE

In the second place, the political theorist will undoubtedly be asked to formulate his problem on a minute level. It is not easy or obvious, but rather most difficult to convince rigid fact-research workers that one is not talking nonsense when he proposes a direct study of an important problem. Given to following their unself-conscious impulses wherever they lead, they would tend to waste huge amounts of time and gather all sorts of unnecessary evidence before coming to the point one wished to attack in the first place.

Lacking initial discriminatory powers, they will wander off on circumlocutory trails and, if by chance they strike some highway, they will charge enthusiastically up it until they strike the rear of the theorists, there to find that they have been chasing the equivalents of folk-sayings, free-will versus determinism, individual versus society, leaders versus followers, or some other hackneyed problem. Many times we have turned to "quantitative" studies of voting behavior, of legislative roll calls, of group-interaction, of social class, and of productivity only to find old problem friends, untranslated, unrelated to all that went before, and encrusted with subtleties of technique all out of bounds to the ultimate destination of the finding!

There may be bounds to exactness that we do not understand at the present time. I am inclined to think that knowing something about those bounds would help both theory and research. For example, there are perhaps twenty different warnings that a good political theorist will give before he asks his audience to accept the proposition that a written constitution will increase the amount of "juridical defense" (to use Mosca's term) or "rule of law" (to use Dicey's term) attainable by the members of a society. These twenty variables will break down into many more. One such warning would be: "But this is not so true if the people have no conception of juridical defense." And the subdivisions of this sub-proposition would include propositions dealing with the number of component classes and groups of the public, the extent of their experience with juridical defense, the trends toward expecting juridical defense on the part of various elements of the population, the partial conceptions of juridical defense distributed among the population, etc. Thus already the very important larger statement is subjected to a possible attrition of some magnitude in its exactitude; whatever the precision of the finding on the lowest level of generalization, the finding's contribution to the highest level of generalization — that is, on the level of the proposition as first stated — will not be so inherent in its unique configuration as in the shape it should take to fit into the larger configuration.

Let us suppose we wished to contribute to the stated general proposition by a contributory study on a low level of generalization. We have the means only for a study of our local community. We decide that a study of the conceptions of juridical defense possessed among the local population would be a commendable contribution to

the general proposition. Two of the many possible hypotheses we may proceed to test are: 1. An "x" number of people believe everyone in the community can get a fair trial on any kind of a case, a "y" number of people believe that everyone in the community can get a fair trial on cases involving private property, and a "z" number of people believe that everyone in the community can get a fair trial in cases involving civil liberty. And also: 2. Of "x" above, "a/x" are of the professional class, "b/x" are of the business class, "c/x" are of the clerical class (and so on with other classes, and repeat with "y" and "z").

We note the following about these two hypotheses. They do not cover the full meaning of juridical defense, because it certainly covers much more than court trials. We do not cover more than one community. We cover only what people believe at the moment. Our classes are derived and the best and latest literature on class will not tell us about the constancy of these people as class members or give us the exact attitudinal, physical, or sociological condition of our subjects. And we have not covered all the kinds of trials that are afforded people.

The technical aspects of the study come next. How careful is it to be; must the sample be random and will the heavens fall upon our findings if names are skipped or people have moved away in some numbers? Must we test people on their understanding of class and court trials and ask them many detailed sub-questions when we take their responses on the main questions? Must we run tests of significance on the differences that we find? Must we do other tasks that exact greater time and effort? I should run the risk of offending those who hold as their ideal the natural sciences (they always mean pure physics or mathematical astronomy, it turns out) by saying "No." We are not, as political scientists, in the business of determining exactness to the nth decimal point. We shall have succeeded if we were to come out of our study with statements of this kind: "More members of the professional class than of the laboring class believe that everyone in the community can get a fair trial on any kind of case. The difference seems more marked in cases involving private property and civil liberty, where the laboring class feels that fair trials are much less frequent." This is all the exactitude we can use; more than this would be spurious and misleading in view of the limitations on our data in the first place (as mentioned

above). Nor is it my opinion that this is less than we need, social science being social science, not physics. The general structure of the theory will not take much more precision than the statement just made.

Perhaps another warning should be appended: if our findings reveal gross percentages of moderate (or less) differences among the classes, we had best say that there are no significant differences in the classes about these matters. One does not need fine statistical devices to justify differences which are quite useless; courage is all that is needed, courage to deny their utility.

I am afraid that I might be tempted out of this position about these points if someone would ask: "Wouldn't you use the above painstaking methods if you had $10,000,000 for studying the whole subject of the rule of law? Then you might study all your sub-propositions with equal pains?" But I would not be so tempted if someone followed with the statement. "We have no possibility of $10,000,000, but if you solve this problem exactly, 200 other political scientists will solve the other 200 problems." This is the vain dream of authors of "pilot studies," that hundreds of research people are directly behind and exactly in their tracks.

Another consideration arises, however, that is worthy of comment. Conceivably, this study that is being made to fill a niche in the theory of juridical defense may also be used to fill a place in the theory of some other concatenation of events, or perhaps in a number of such larger theories. If this is possible, or probable, ought not this study be as perfect as possible; that is, ought it not to maximize all precautions? We all know of cases where studies designed to test one proposition have been used to substantiate other propositions.

This is, indeed, a very important question for social science, having to do with nothing less than the disposition of a major portion of its energies. For concentration upon such studies as these to the neglect of the simpler directed studies will quickly take up the time and resources of most social scientists. Among some social scientists, especially among behavioral psychologists, this approach has achieved the status of a dogma. It may be phrased thusly: "Any fact in the external world is equal to any other fact: Each fact is assumed to have equal possibilities of being incorporated into any one of an unknown number and kind of fruitful larger generalizations." By this assumption, larger theoretical worries are avoided. Since it can-

not be predicted what other larger statements the narrower statement will serve (besides the one being tested directly), it is believed to be useless to attempt to direct a factual study to (1) additional larger statements and, in the extreme form, (2) even the larger statement that first inspired the study. This is theoretical nihilism. It is the implicit theory underlying studies whose authors deny or conceal any concern for larger theory.

The present writer cannot accept theoretical nihilism because, paradoxically, he accepts a phenomenological theory of science that says a fact *can* serve as many purposes as there are sides to reality, and that since these sides are of an unknown but large number, the selection of a "fact" about reality because it is a "fact" is equivalent to taking the unfathomed cogniture structure of the perceiving person as the "free" and firm foundation of science. I think that John Dewey throws much light on this problem when he writes that "fact, physically speaking, is the ultimate residue after human purposes, desires, emotions, ideas, and ideals are systematically excluded. A social 'fact,' on the other hand, is a concretion in external form of precisely these human purposes."

But even accepting theoretical nihilism, as well as the possibility of a first level study contributing to more than its parent theory, we may still apply a standard for conserving resources and maintaining research-theory equilibrium. If the nihilist theory is accepted, that is, the belief that all facts are equal and the number of theories to which a study may contribute is never known, then there can be no objection to adopting our original working principle, to wit, that we should pay attention only to the larger proposition and trust that the nihilist theory being correct, the findings of our cruder study will be useful in some measure to other theories. Furthermore, if political science is characterized by relatively loose propositions generally, rather than tight and exact propositions, then it cannot be believed that the looser statement of this proposition will destroy the possibility of it being useful for broader propositions of political science other than the one on juridical defense.

Beyond these two statements, the case for theory rests in the hands of unknown individuals and unknown social forces working on science. When someone will show us that one way of looking at the facts of political science allows us to build theory upon theory, then we shall study the lowest level of juridical defense (i.e., what-

ever its equivalent will be) with clearer knowledge of what its exact relationship to the higher level generalization about juridical defense must be. And, of course, theoretical nihilism will have to be dismissed as a working principle.

We may turn, briefly, to a related matter. A characteristic defect of theoretical nihilism, implied in the foregoing discussion, deserves explicit formulation. This is the case when, lacking directed hypothetical purpose, the author of a study allows the techniques of study to overwhelm any factual purpose.

Examples in my experience would be studies that were made on the most intensive level, with the most elaborate precautions and artificial controls, of a unique work-operation — something like cutting concocted patterns in cloth, let us say. It would be, I believe, the conviction of most political theorists that so many are the assumptions that must be made, so many the different kinds of work introduced into any larger generalization about work-productivity, that nothing of larger utility was gained from the painstaking effort that went into this diminutive study. By the time it arrived at its final resting place as evidence, its fine edges would be completely destroyed, indeed, would *have* to be destroyed. And, it often happens that the very design of such studies is so exacting as to eliminate even the direct, minor purpose of the study to supply aid to a larger generalization. The controls establish such artificial conditions that all relationship to real events is lost.

If political theorists would become more active, I feel there might be more "middle-level research," perhaps with a lesser degree of control and precision, but on a much higher degree of utility. As Aristotle declared, "It is the mark of the educated man to look for precision in each class of things just so far as the nature of the subject admits."

III. RESEARCH MEMORANDA

A third consideration arises here. Theorists are very often good readers. They are expert in extracting meaning from past works. Their research companions often are not. Too much research occurs without reference to the existing literature. To use psychological language, non-reading is part of the anti-historical syndrome. The question can properly be asked of the theorist: What have others done with your problem? Political research and social science re-

search need many more commentaries on research problems in crucial areas. Such research memoranda as the Social Science Research Council has been sponsoring for some time are valuable adjuncts to research.[1] The summary of leadership materials in the various social sciences that was prepared by Professor Stogdill under different auspices is another example.[2] What is desirable to research workers is a memorandum that gives semantic clarity to the concepts and facts with which they are dealing by passing them through the various meanings they have been given by others. What exists in the way of logically interconnected propositions can be brought out, and many stumbling blocks to research can be averted.

The Laboratory for Research in Social Relations at the University of Minnesota has been engaged for two years in studies within the general area of social responsibility. Many meetings have been given over to obtaining the perspective just described. It early became obvious in these meetings that almost every problem of every social science related in some way to the idea of social responsibility. A great job of clearing the forest began, and still continues. Social responsibility had to be reviewed for the values ordinarily contained in the phrase, for the facts and findings on the matters mentioned in these values, for the various formulations of and techniques for investigating these values and findings that each discipline customarily employs, and finally a part of the area had to be cut out for particular analysis. I am sure that this clearing was undertaken with no less clamor and sweat than attends the clearing of a forest of redwoods. To say now how theory and research ideas interacted during this long period would be almost impossible. Reams of stenographic reports were gathered, but they convey nothing of the dark and silent processes of personal thought. A great part of theory-research interaction takes place in the individual mind and will never thenceforth be recognizable as composed of two parts — one theory, the other data-gathering and technique-devising. Only in a symposium like this must we force a distinction and urge its abolition by means of a clumsy formula of (1) theory, (2) research, (3) better theory.

[1]As instances, see Robin M. Williams, Jr., *The Reduction of Intergroup Tensions* (New York: Social Science Research Council, 1947), and Otto Klineberg, *Tensions Affecting International Understanding* (New York: Social Science Research Council, 1950).

[2]Ralph M. Stogdill, "Personal Factors Associated with Leadership," *The Journal of Psychology,* 25 (January, 1948), 35-71.

IV. INVOLVEMENT IN THE RESEARCH PROCESS

Finally, the political theorist, when he involves himself in the process which this symposium discusses, will be called on to help in the development of techniques for observing and analyzing the materials to be covered. He may rightly confine his interests to other spheres, of course. But some experience here will be of value, for it is perhaps the most enlightening part of the whole process for a political scientist who is trained in another tradition. It is here that he realizes for once that theories are not political slogans that have one form, to be preserved and venerated for ages, but that they are changeable things, dependent — much as we hate to admit it — on the techniques available to solve the problems under scrutiny.

Words simply cannot remain the same. Social responsibility cannot be studied as "Social Responsibility." To do so would invite hopeless confusion. This phrase at the present time can embrace intelligibly such matters as the duty of rulers, the free market of economics, obedience to custom, or the proper toilet training of infants. Adam Smith talked of one kind of responsible individual who "by pursuing his own interest . . . frequently promotes that of the society more effectually than he really intends to promote it."[3] J. A. Hobson wrote that "only in so far as a reasonable interpretation of economic processes in the light of social co-operation can be got into normal effective thinking, will it be possible to displace the passions of personal, class, and national rights by that larger conception of human co-operation needed for the free extension of the sense of responsibility."[4] And T. H. Green declared that "there can be no right without a consciousness of common interest on the part of members of a society" and added, in another place, that the mere conformer is not moral, because to be morally responsible one must make a conscious choice of good or evil.[5]

Obviously, there are here several important contradictions or, at least, problems. One might add that the attempt to bring in new theories results rather quickly in greater perplexity, for one finds

[3]Adam Smith, *Wealth of Nations* (1776), (Cannan ed., New York: Modern Library, 1937), p. 423.

[4]J. A. Hobson, Herman Finer, and Hanna Meuter, *Le Sens de la Responsabilité dans la Vie Sociale* (Bruxelles: Institut de Sociologie Solvay, 1939), p. 80.

[5]Thomas Hill Green, *Lectures on the Principles of Moral Obligation* (London: Longmans, Green and Co., 1904), pp. 48, 218.

himself shortly concerned with the Malinowski theory that primitive man conforms and does not conform, much as other men, and the Freudian theory of libidinal displacement to explain why one chooses certain things rather than others as objects of responsibility. As was said above, a research memorandum is a profitable guide and inducement to research, but it is no substitute for it.

So from a world of confused statements, the laboratory had to move to problems. One's favorite scattered thoughts, occurring as social responsibility was studied as a unitary concept, had to be put aside (perhaps to be slipped into questionnaires later on, when one's colleagues were not watching). The problem is now the thing, and we find here a means of focusing the research, say, on what people of different socio-economic classes think about their political responsibilities. This shift is typical of non-systematic sciences when they go into research. A non-systematic theory resorts finally to problem-directed research if it rejects aimless "fact" research. The problem on which all agree provides a temporary framework.

At this point political theorists must, of course, revise their vocabulary, their interests, and the generality of the propositions to be studied. The original theory, so eloquently but vaguely stated in the literature, is now scarcely recognizable.

The final blow is struck when the techniques of studying a particular problem are adopted or devised. General statistical studies of classes, intensive interviews on behavior and attitudes, extensive interviews, questionnaires of 1000 items or of 10 items, scaling by item responses or scaling of responses by a panel of judges, and numerous other proposals are made for the gathering and analysis of the data. By this time not even an expert logician and semanticist can tell what new approach, emphasis, observations, and coverage of the target problem will emerge in the end. As one questionnaire develops from another, as the interview techniques change, as the method of scoring and rating people on this limited sector of social responsibility that is really not Social Responsibility is revised, the theoretical infant grows more and more strange to its parents. In the reaction to this change lies a partial explanation of the cloister-phenomenon; no wonder that the parents wish to prevent their children from being exposed to such experiences! But the theorists' presence is all the more necessary here in order to hold the structure of the problem together so that it does not disintegrate into *only* those bits of things

that can be asked easily, *only* those partial things that can readily be seen, or *only* those incomplete aspects of things that can be manipulated numerically.

That the events described thus far actually happen to political theorists who connect with research and do not perish in the process may be documented by the experiences of men like Charles E. Merriam, Harold D. Lasswell, Roberto Michels, and Charles A. Beard. Similar experiences befell briefly a number of political scientists when they encountered head-on the concepts and methods of other social scientists at the Conference on Political Behavior at Ann Arbor in 1949.[6] They occur frequently at places like the University of Chicago where students scarcely know that a "disciplinary" problem exists until they go on to graduate work; even there, they often are bigamous, scholastically speaking. Finally, the process transpires wherever interdisciplinary encounters take place. By joint conferences, joint research, and informal associations freed from the jealous tutelage of disciplinary stereotypes, interdisciplinary experience increase the self-consciousness of theory-guided research. That this is a great or minor good is perhaps ultimately a matter of faith. This writer would venture on his own account to say that the history of science and culture evidence the existence of positive relationships among self-consciousness, fact-objectivity, and political theory of a high order.

[6] Alexander Heard, "Research on Political Behavior: Report of a Conference," *Items*, 3, No. 4 (December, 1949), 41-3.