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Andrews: A Social Scientist Looks at Unitarians and Separatists

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A SOCIAL SCIENTIST LOOKS AT UNITARIANS AND SEPARATISTS
by
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As I understand the purpose of this symposium, it brings together a "theorist" on social science methodology, a commentator from another theoretical perspective, and a "practitioner" as a second commentator. Professor Rudner has selected as the object of his theoretical attention the problem of subjectivism in the social sciences and titles his paper, "Some Essays at Objectivity". He focuses his remarks on criticism of a little book by Peter Winch, _The Idea of a Social Science_. He challenges what he calls Professor Winch's "separatist" position from what, in the interests of terminological symmetry, I shall call a "unitarian" position. My response, I understand, should reflect my judgment as a "practitioner" on the practical value of his paper and his position. While he bolsters my theoretical apparatus—as he has surely done—I shall attempt to put his abstractions to concrete test. I shall do so by weighing the relative merits of the two positions in the Rudner-Winch debate.

This delineation of my assignment excludes both a systematic logical analysis of Professor Rudner's paper and the exposition of an alternative approach. Nor will I attempt to consider whether Professor Rudner's critique of the Winch book is fair or accurate or complete. Still less will I critique Winch or, for that matter even read him. So far as these tasks are to be tackled in this symposium, I leave them to my political theory colleague, Professor Kateb. Professor Rudner has defined the struggle between the "unitarians" and the "separatists". I shall stand apart from the contentions themselves and try to appraise the relative utility for the social sciences of the two basic stances. How useful is Professor Rudner's philosophical analysis to social scientists? How useful, by way of comparison, is Professor Winch's approach? Do either or both of them deal with methodological problems important in today's social science? These are the principal questions which are directed to me. County Agent Rudner has come out to Social Science Farms to explain some principles of insect eradication. The Old Dirt Farmer is asked to react. Before I can, however, we need to consider briefly the issue joined by Professors Rudner and Winch as defined by the former.

_The Rudner Argument_. Because a revised version of the Rudner paper is included in this volume I shall present here only enough substance from the original version to enable the reader to understand what I think I am agreeing with. As I read his paper, his main point is that Professor Winch and other "methodological separatists" err in their "denial of the unity of scientific method," especially in their belief that the study of society requires a different "logic of validation" than is required for the study of nature. Professor Rudner reflects the view that social science investigation, unlike the natural sciences, requires "the adoption by the social scientist of the teleology of the observed."
Similarly, he finds unconvincing the Winch argument that the scientific investigator cannot "translate" words (and, by extension, other social phenomena) in any meaningful way. He exhorts us social scientists to push ahead with the Spring plowing and pay no heed to Professor Winch's "flying lox box." That fierce-looking insect, he tells us, is quite harmless. Though we may gaze raptly at the exotic coloration and form and be irritated by its buzzing about our scientifically cultivated crop of social science concepts, we need take no preventive measures. No spraying is required, for any apparent dangers from the flying lox box are purely illusory. In fact, he insists, the flying lox box itself is an illusion, the product of the overactive and misdirected imagination of a sincere but wrongheaded itinerant quack insecticide peddler.

In order for the simple dirt farmer, like myself, to evaluate the Rudner argument, I must be sure I understand some of his key terms. Apparently, Professor Rudner's peers down at the Agricultural Experiment Station know what he means by "the scientific method" and "unity," for I find no definition of those terms in his paper. A poor dirt farmer social scientist, however, does not spin in so closed a terminological circle and needs the help of such definitions.

By picking clues here and there, I believe I can fabricate definitions that will do justice to Rudner's case. He seems to equate "the logic of validation, of explanation and prediction" with "the scientific method" and he provides examples of "paraphernalia of scientific validation (including, e.g., the empirical testing of theories containing lawlike sentences—sentences testable through their uses in explanation and prediction)." Also he infers from Winch's writing that "he appears to concede" that "physical science investigations" consist of "the logical subsumption of statements describing them under general scientific laws—through the confirmation of theories containing such laws and employed to make successful predictions." The framing of a "meaningful empirical hypothesis" is an essential part of the scientific method. So are "verificational canons." These are all the remarks I can find in his paper that shed light directly on his definition of "the scientific method." Any others are so subtle as to escape the eye of at least one dirt farmer.

To form the definition needed, then, we must assemble the gist of those remarks in the context of the dirt farmer's general, commonsense notions of the meaning of the term. This may produce a much cruder definition than Professor Rudner would have provided, but it is the most one can expect of the poor dirt farmer and it ought to be adequate for our purpose. Using that approach, I arrive at the understanding that he means "the scientific method" to be a systematic process of intellectual inquiry that includes these steps:

1. The identification of a problem susceptible of such inquiry.
2. The acquisition of enough information about the problem to formulate one or more hypotheses that are designed to solve the problem and are testable.
3. The formulation of the hypothesis or hypotheses.
4. The testing of the hypothesis or hypotheses by application to examples of the problem and determination of whether one of them has the predictive...
capacity necessary for solution.
5. The revision of the hypotheses in the light of that testing in such ways as logic indicates to improve the predictive capacity.
6. Repetition of the process until a hypothesis provides sufficient predictability to qualify as a "scientific law."
7. The application of this process to related problems until enough related laws are produced to form a "theory."
8. The validation of the "theory" by the same process applied more broadly.
9. The use of the laws and theory to make predictions with respect to the problem.

I find even fewer remarks in his paper that define directly Professor Rudner's use of "unity" in the phrase "the unity of the scientific method." Nor, given the context, are the dictionary definitions adequate. However, the matter need not detain us long. His meaning is quite clear, it seems to me. I take "unity" in that phrase to mean that the scientific method has but one character and can be applied to both social and physical phenomena. The steps listed in the paragraph above, say Professor Rudner and his friends, can be followed in social, as in physical, science inquiry.

Assuming that I have defined Professor Rudner's terms correctly—or have come close enough—I accept the Rudner argument. To the extent that Professor Winch disagrees, may he burn in Hell forever. Certainly, "the scientific method" seems to provide a rigorous, systematic process for intellectual inquiry in the social sciences. I understand that the same can be said of the physical sciences. But sometimes the method is too rigorous. The material of the social sciences is too intractable for full application of the scientific method. Society is not always as cooperative as nature in submitting to the kind of tests required by the scientific method. However, that should not be construed as invalidating the Rudner thesis. The fact that the scientific method cannot always be applied fully does not mean that it should not be applied as fully as possible. In any case, I understand Professor Rudner to be arguing that it can be applied, not that it can be applied fully or that it should be used in all cases. My experience and observation lead me to agree that we in the social sciences need not leave that fertile territory exclusively to the physical sciences. The tool is too useful to go by default to only one branch of science.

Value of the Rudner Thesis. To say that the Rudner thesis is right is not to say that it is worth stating. Before we social scientists buy the Rudner brand of insecticide, we need to know, not only whether it works, but also whether it does the job that we need to have done. Are his views of value to us? Or was his earlier belief correct in holding that the unity of the scientific method is beyond dispute? In that case, further dispute is a waste of effort. Is Professor Winch a "curious anachronism"? If he is, he ill deserves the attention he gets from Professor Rudner.

If the value of the Rudner position depended on its originality, it would fare poorly. Admittedly, he dismantles the Winch case with great skill and imagination. But when he is finished all he has left is a case that was won long ago and
many times over. Social scientists may dispute the practicality of using the scientific method in this case or that, but its validity and utility when practical is not challenged seriously by working social scientists.

Originality is not the only valid measure of value in such a discussion. Well-stated reminders are valuable too. Though we know we have as much right as the physical sciences to the scientific method, we often forget it. The scientific method requires discipline and work. Often, it proves impractical. We get lazy, discouraged, inattentive. The method is used much less by social scientists than by physical scientists. Professor Rudner does well to remind us that this valuable tool is available to us, also.

However, another point in Professor Rudner’s paper is much more valuable in current social science methodological debate. I refer to the support it brings to those social scientists who reject the argument that only the internal perspective is valid for the study of some social phenomena. “If you aren’t part of the problem,” says that argument, “you can’t study it.” Only Africans can study Africa, for instance. It takes one to know one. An especially colorful example of this approach might be an American woman anthropologist becoming one of the eleven or so wives of a chieftain in the course of her study of the sexual behavior of his tribe, believing that she must become one with them—literally—in order to study them. This seems to be the “teleology of the observed” for which Professor Rudner berates Professor Winch. His attack on the Winchian view that “the only way in which . . . a social science investigation can achieve understanding is via the adoption by the social scientist of the teleology of the observed” hits that approach squarely between the eyes. The internal-monopoly view in the social sciences seems to have crested and to be retreating in the face of reality and commonsense. Still, the additional nail or two that Professor Rudner has driven into the coffin is well worth the effort.

Value of the Winch Thesis. In order to decide rationally whether to buy the Rudner brand of insecticide, however, we must do more than merely consider its merits. Also, we must consider the alternative, the Winch brand. As a semiliterate dirt farmer, I have not read the Winch book which Professor Rudner discusses, nor do I think it necessary for my purposes here. I am more interested in and concerned with the disagreement between Professor Rudner and Professor Rudner’s interpretation of the Winch thesis than I am with the Winch thesis itself. In fact, even taking Winch on Rudner’s terms, the value of the Winch alternative is quite apparent.

In the first place, the Winchian emphasis on the “internal perspective” calls attention to an important, special asset of the social sciences. Although that perspective should not be construed as providing exclusive access to the truth, it remains a valuable tool for the social scientist. An eloquent reminder of its availability is as useful as Professor Rudner’s reminder that the scientific method is available to social scientists. To say that the French (or even French politicians) have no monopoly on the study of French politics is not to say that insights derived from their perspective cannot be valuable to other students of French politics. Our opportunity to benefit from that perspective puts us at least one step ahead of the physical scientist. We should exploit that advantage
more. Take Professor Rudner's meteorologist, for instance. He can study tornadoes from books, charts, articles, radar readings, etc., or he can observe them visually from a distance if he has the good fortune to be nearby when one strikes, or, if his luck is really outstanding and he is picked up by a tornado and taken for a whirl, he can observe the object of his inquiry from within.

Compare that meteorologist with our matrimonially-inclined anthropologist. Like the meteorologist, she can use appropriate literature in her study, she can observe from outside (peeping through the brush, I suppose), she can observe from within (residing in the village). But, also, she can go further. She can participate in the practice she is studying and she can interview native participants to elicit information on their activities and attitudes. Indeed, she can go still further by recruiting a member of the tribe into the anthropological profession, trained to study his own group.

The physical sciences, unless one includes medicine, lack this opportunity to exploit the internal perspective. Not even Nils Bohr could contrive to go splitting with an atom in his smasher nor does Linus Pauling claim that he has ever interviewed a molecule. And, however much our lucky meteorologist might run around with tornadoes, he will never be able to hang American Meteorological Association memberships on them. Note that I reject the claim that the internal perspective is exclusively valid or necessarily superior, but I reject also, any suggestion that it is valueless or that it does not create special opportunities for social scientists. It gives social scientists access to a very valuable asset that is totally inaccessible to physical scientists.

It seems to me that the "unity of the scientific method" argument implies that no significant differences exist between the use of that method in the two areas of science. I realize that this is not the direct thrust of Professor Rudner's thesis, but we are responsible for our implications also. An implication of his paper obscures a very important difference of which social scientists should be reminded at every opportunity. Without such reminding we lapse too easily into the role of library-bound scholars which should be anathema to most social scientists. Too many students of the British House of Commons, for instance, have never interviewed M.P.s. A classical example of that attitude was Woodrow Wilson's pioneering study, *Congressional Government*, which he wrote a few miles from Washington without ever visiting Congress or talking to a Congressman. Such lapses are much less common now but still frequent enough to require reminders, like Winch's, that unity is not necessarily identity and that some of the differences can be exploited to the benefit of the social sciences.

Obscuring the differences between the sciences with respect to the internal perspective creates another, more serious danger, it seems to me. Here, too, I see the "separatist" argument as more valuable to the social sciences than the "unitarian" view. I refer to the danger of subjectivism to which the title of the Rudner paper alludes. In my opinion, subjectivism is the most serious methodological problem affecting the social sciences today. We need all the help we can get to cope with it. I am disappointed that Professor Rudner did not see fit to exploit the opportunity offered by his title to tackle it instead of deflecting our gaze from it. He reassures us that Professor Winch's "flying lox
The "unitarian" notion obscures the great differences between the physical and social sciences in the relationships of the investigator and the investigated. Because of those differences, the social scientist is under much greater psychological and social pressure and temptation to use the scientific method corruptly than is the physical scientist. Furthermore, most social phenomena worthy of scientific attention are more complex and much less easily susceptible of experimental control than are most phenomena of interest to physical scientists. This complexity makes the temptations and pressures very difficult to resist and the lapses from grace very difficult to discover and expose. It seems to me that the "unitarian" argument, however correct it may be technically, distracts attention from the problems arising from those differences and renders a grave disservice to those social scientists who are striving to work scientifically.

The first of those critical differences is central to the objectivity-subjectivity debate of which, I understand, this symposium is intended to be a part. In the social sciences, Man studies Man, especially Man in his relations with other Men. In most of the physical sciences, Man studies Nature. The social scientist is, in effect, both investigator and investigated. Because of this identity, grave problems of objectivity arise. The conflict of interest between the social scientist's two identities makes scientific detachment extremely difficult. He must detach himself from himself.

Sometimes, this conflict of interest is direct and palpable. The patriotic American political scientist may be sorely tempted to present in the most favorable light his country's role in an international dispute. The economist-educator may have to exercise restraint to avoid biased selection of indicators to show the economic value of education to best advantage. The Black sociologist may be under pressure to sacrifice scientific validity to the interests of racial equality.

Sometimes, however, the conflict may be subtle and indirect. It may be highly personal and psychological. For instance, I am convinced that many specialists in one of my fields, comparative government, are attracted to that study because they are dissatisfied with our own society and nourish a subliminal hope that a search among other systems and institutions will confirm their implicit assumption that other countries have done better than we have. An anthropologist friend tells me that many of his colleagues have similar motivations. For one reason or another, they fit poorly into their own culture. They enter anthropology because they yearn subconsciously to find a more compatible culture as a way to prove that society, not themselves, is out of step.

Conversely, some social scientists who identify themselves with specific groups or institutions tend to exalt them in their study. Like passengers in a hand-cranked elevator, they rise with the object they raise. A striking example of that phenomenon is recent study of the American Presidency. The reputation of that institution among students of American government has
depended far more on their personal experiences with it and their partisan biases than on scientific observation and theory.

For a period of time around 1960, the Presidency was regarded with wildly-exaggerated admiration. A consensus that formed among historians and political scientists portrayed the Presidency as the Great Repository of Virtue and Wisdom in our political system, the only institution with the vision, the impetus, and the ability to act in the national interest. However, they argued, the Presidency has too little power to put its virtue to best service for the country, especially in the areas of foreign and defense affairs. On the other hand, they saw Congress as a collection of bickering, selfish, little men out of step with the times and locked in an institution that was incapable of real action or leadership. They provided eloquent reasons and many examples to support their argument.1

Ten years later, another consensus—diametrically opposite to its predecessor—dominated the social science view of the Presidency. Now, the office was seen as a “Frankenstein monster” spouting evil and spewing destruction. Virtue had become Vice; Wisdom had become Folly. Now, Presidential power was viewed as being grossly excessive, especially in the foreign and defense affairs areas where it had been seen as most insufficient a decade earlier. On the other hand, Congress has acquired shining armor and a halo. Now, the consensus holds that Congress is more “democratic” than the Presidency and that Presidents are “too isolated” from the people to exercise wise judgment. Only Congress, we hear now, can act in the national interest. The 1970 writers provide reasons for the new consensus that are equal in eloquence, but opposite in import to those of the 1960 consensus.

This dramatic reversal of consensus seems to result from two factors. First, the 1960 writers preferred the policy positions of the Presidents to those of Congress during the preceding quarter-century. The 1970 writers, on the other hand, much preferred Congressional policies of the preceding half-decade to the Presidential positions—especially on Vietnam. In both cases, they elevated their policy preferences into constitutional theories.

Secondly, most of the 1960 writers had had some substantial personal association with the Roosevelt-Truman-Stevenson organizations in the executive branch of the national government. None had any significant connection with Congress. The 1970 writers either had had no affiliation with the Kennedy-Johnson-Nixon Presidencies or had seen such affiliations turn sour.

The record of one member of the 1960 group—the great constitutional law scholar, Edward S. Corwin—illustrates the effect of career on theory. While Corwin was Woodrow Wilson’s protege at Princeton and, later, while he supported the New Deal actively and publicly and assisted Franklin Roosevelt in the “court packing” case, he was a “strong Presidency” theorist. When he broke with Roosevelt politically—apparently disappointed at being passed over for a Supreme Court seat—he turned against the strong Presidency also. After Roosevelt died and Corwin became a leader in the campaign against the proposed “Bricker amendment”, he reversed his theoretical position on the question of Presidential power again. One cannot escape the harsh conclusion that the constitutional theory of one of the leading social scientists of his day followed...
his personal career interests and political views.

The extravagant claims of the Presidential Glorifiers of 1960 seemed very quaint by 1970. They had been fabricated of very perishable materials. Those materials were the circumstances of the time viewed from the writer’s personal interests. They would not have been tempted to indulge that bias if the relationship between investigator and the investigated had not produced the kind of conflict of interest that is typical of the social sciences but not of the physical sciences. In stressing the unity of scientific method, it seems to me, Professor Rudner overlooks this very grave methodological problem.

That problem is compounded by the complexity that is typical of social science problems. Generally speaking, phenomena of interest to social scientists involve material more varied, uneven, and uncontrollable than is the case with most objects studied by physical scientists. This disorder renders the task of verification much more difficult in the social sciences than in the physical sciences, for predictions usually cannot be stated with the parsimony or tested with the precision of the physical sciences. They cannot be stated with comparable parsimony, for they must take so many factors into consideration. They cannot be tested with the same precision because the materials of the social scientist tend to be so much less susceptible to experimental control. The difficulties of verification and prediction create golden opportunities for inattentive social scientists to yield to the temptations, biases, and pressures that beset them on all sides and to corrupt Professor Rudner’s “scientific method”.

No doubt, some analogous situations exist in the physical sciences, but they are not typical—as they are in the social sciences. If the difference between the two branches of science is one of degree, the degree is so great as to have the significance of kind. Once again, by stressing the similarities of the two branches, Professor Rudner distracts us from what should be our major concerns, whereas the Winch school calls attention to it.

Conclusion. In reviewing the relative value of the Winch and Rudner contributions for the improvement of social science methodology, no clear balance can be struck. We need Professor Rudner prodding us to make fuller use of the scientific method and to regard scornfully claims that the internal perspective has some monopoly on truth. On the other hand, we need Professor Winch’s reminder of the special values of that perspective and of the dangers of subjectivism. The flying lox box may not, in fact, exist, but we are beset by plagues of other pests that exist abundantly. We have plenty of use for both brands of insecticide—and more besides. We need the Winches and the Rudners and, above all, we need the kind of stimulus provided by symposia like this to keep us aware of the many dangers surrounding us and to keep us from lapsing into methodological sloppiness from lack of sensitivity.

FOOTNOTES
1For a fuller treatment of this point, see my contribution to a book of readings on American government being edited by Professor Norman Thomas for publication by Dodd, Mead in 1974.