Philosophic Exchange

Volume 4
Number 1 *Volume 4 (1973)*

Article 6

1973

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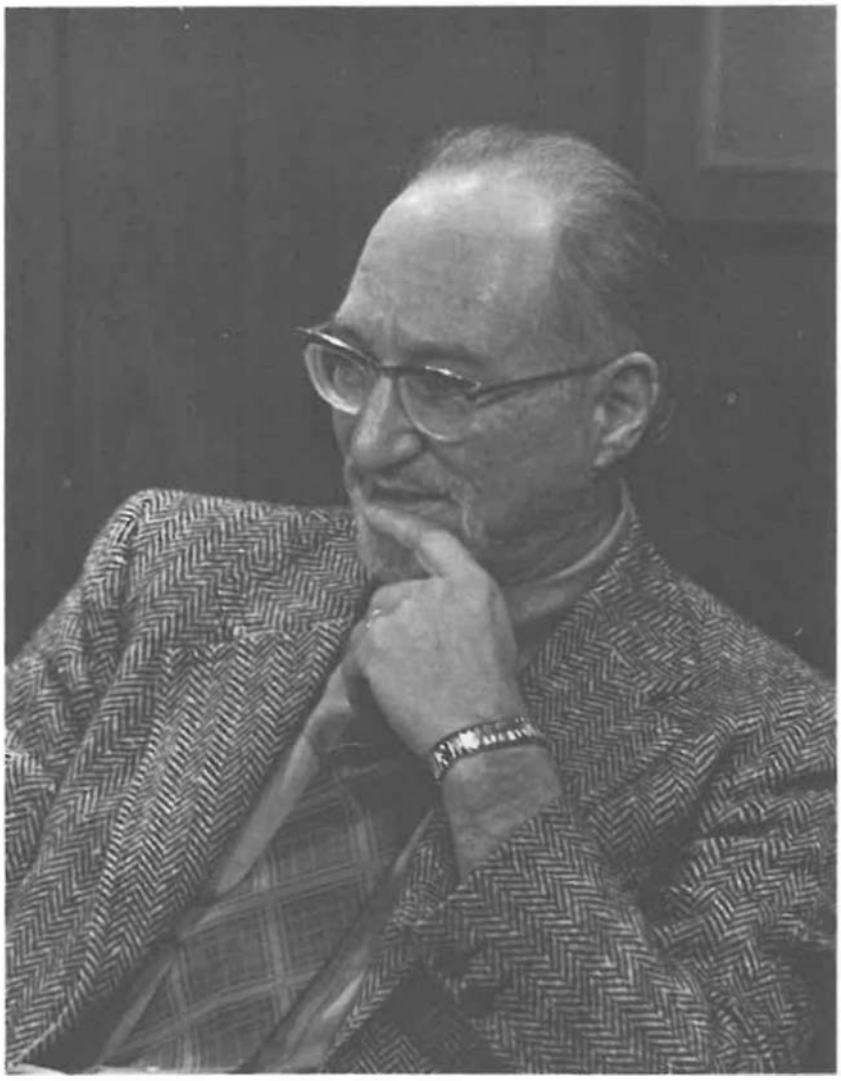

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SOME ESSAYS AT OBJECTIVITY*

by

Richard Rudner

Riddle: What is as big as a lox, is shaped like a box, and has wings?

Answer: A flying lox box.

—Henny Youngman

More years ago than it now makes me comfortable to remember, I participated in an American Philosophical Association symposium on the subjectivity of the Social Sciences. Professor May Brodbeck gave that symposium's opening paper, and both her paper and mine[1] were devoted primarily to attacks on theses which F. A. von Hayek had argued in his book *The Counter-Revolution of Science*.[2] In what now seems a paroxism of smugness I, at any rate, must then have regarded Hayek as an atavistic isolate propounding "old-hat," reactionary doctrines about the Social Sciences—doctrines which, I then obviously thought, would surely be regarded by philosophers of science—indeed by all philosophers in the analytic tradition—merely as curious anachronisms.

Smugness turned out to have its foundation in a quite unwarranted ignorance of new currents of thought then already stirring. The intervening years have not brought any change of mind about the cogency of Hayek's and related views, but they have certainly seen my mind changed about the vitality of such views and about their pervasiveness among philosophers who, I had once innocently thought, would find them unacceptable.

My ignorance at the time was rooted in an unawareness that views like those of Hayek's were apparently being nurtured by the then relatively subterranean streams of Wittgenstein's later thought. When, as in the past few years, those streams gave rise to torrents of articles and books—all with a similar burden concerning the special position of what may be called the language of mentation and, implicitly or explicitly, the special inaccessibility of social and psychological phenomena (including linguistic acts) to scientific investigation—my smugness in construing views like Hayek's as cranky anachronisms was, of course, shattered.

I speak of Wittgenstein as being the apparent or better, the alleged "father" of these "torrents" because, despite the overt and covert claims for his paternity made in many of these writings, I am not wholly convinced by my understanding of some of Wittgenstein's views on language, to credit the claims. Indeed, my impression is that his work does *not* have as consequence the alleged, peculiar imperviousness of social phenomena. But I have no intention of making this a paper about Wittgenstein. The continuation of the particular gambit concerning who is responsible for these views is one that the following discussion will resolutely decline. We forego arguments about whether the baby should be

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deposited on Wittgenstein's doorstep. All we want to do is take a very close look at the baby.

The gist of Hayek's conclusions in *The Counter-Revolution of Science* was that social phenomena (hereafter I shall use this term to embrace what is referred to usually by most uses of either the term 'social' or the term 'psychological') are not amenable to investigation by the method of science—a method which he believed appropriate, at most, to non-social phenomena—and that social phenomena require a special, radically distinct, methodology for their investigation.

Of course, major assaults on the efficacy or objectivity of scientific study of social phenomena have been immensely varied in vitality, interest and plausibility. I am especially concerned with those positions which take a form that may be described as a denial of the unity of scientific method—those which I have categorized elsewhere as separatist positions.[3] Under the rubric of methodological separatism I lump all those positions which have in common their denial that the methodology applicable to the other sciences is applicable to the social sciences. There are, alas, a great many such positions; their denials are variously couched and variously grounded. Historicists, sociologists of knowledge, are perenially with us; radical or activist anti-theoreticians, and hermeneuticists, present views which currently gain interest or favor. Each of these major positions, no doubt, deserves a detailed scrutiny. In the present essay, however, my attention will be focused only obliquely on these positions—and only insofar as they are related to still another major separatist position which has evidenced extraordinary vitality since it was given relatively definitive form in Peter Winch's misnamed book, *The Idea of a Social Science*.

The position Winch elucidates is one which explicitly credits the influence of Wittgenstein, but it obviously shares many tenets with the other separatist positions just mentioned, and it patently, in turn, has influenced subsequent writers. I have found Winch's arguments in his book as powerful and subtle as any put forward by contemporary proponents of like doctrines. But I am especially interested in examining his views since they bear rather heavily upon some even more general problems which are of pivotal importance in current analytic philosophy—problems associated with the notions of adequate translation and translatability.

The discussion which follows, then, is divided into three main sections. In the first section I have tried to prepare the way for later making some of the issues sharper by considering the relevance of familiar distinctions between technique and method in Science and between the contexts of discovery and validation. In the second section I consider arguments about the relevance of causal explanation (or causal description) and the relevance of scientifically verifiable theories to the investigation of social phenomena. Finally I examine briefly the relevance of results which have been achieved on problems of translatability by such people as Quine and others for assessing certain of the claims that Winch makes.

Part I

It is important at the outset to see that the sort of claim being made about

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social science by separatists is quite different from certain much less radical claims that have been made about the difference between the ways in which social phenomena and non-social phenomena may be studied. For example, it has often been held (too often considering its banality) that specific *techniques* of observation or experimentation or of experimental control which are applicable to phenomena of Physics are inapplicable to phenomena of, say, social psychology. But it is clear that Winch's kind of thesis about social phenomena amounts by no means to the banal observation that sociologists aren't able to accelerate what they label 'Cambridge Dons' in cyclotrons.

To have become aware that various scientific disciplines employ various and varying techniques of investigation is not to have become aware of anything significant about the methodological character of social science. It is not, for example, at all obvious that the social sciences differ more, in technique, from non-social sciences than do the non-social sciences differ among themselves. What grounds, for instance, could be adduced to support the claim that use of a telescope in astronomy is more different from the use of a one-way mirror in small-group research than it is from, say, the use of a bathysphere? Not only are there differences in technique among non-social science disciplines as vast, or striking, or marked, as those between the non-social sciences and the social sciences, but also there are differences of techniques employed among the sub-disciplines of a single discipline or within even a single sub-discipline in different epochs, which are as great as can easily be thought of.

The point of rehearsing such relatively familiar considerations is not only to emphasize that the separatist thesis about the difference between investigations of social and non-social phenomena, must not be construed as a trivial one concerning differences in *technique*, but also to emphasize that adducing differences of technique will not *per se* provide any evidence for the distinction the separatist *is* claiming.

The distinction Winch, for example, *is* claiming is a distinction of *method* and not of technique. He believes that he establishes by his arguments that " . . . the understanding of society is *logically* different from the understanding of nature . . . "[4], and that an acquisition of the former requires a different methodology from that which is required for an acquisition of the latter—that is to say, it requires not different *techniques* of inquiry but a different *logic* of inquiry.

Winch's claim, as is indeed quickly patent to his readers who are familiar with the literature, belongs to that area of philosophical interest which Reichenbach used to call "the Context of Validation" or "the Context of Justification" in Science. And this brings us to the second of the two distinctions which helps us sharpen the issue; namely, the distinction between the "Context of Validation" and the "Context of Discovery." For while it is important to see not only that what is at issue is a thesis about logics of inquiry or methodologies rather than about techniques, it is equally important to see that problems of the methodology or of the *logic* of scientific inquiry belong to the context of validation. No one, in fact, has demonstrated that there is such a thing as a *logic* of discovery. On the other hand, a logic, or methodology, of validation, of explanation and

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prediction, is precisely what is being referred to when it is asserted that regardless of differences in technique of observation or experiment *the* scientific method is pervasive through all the sciences, or *the* scientific method is applicable in the investigation of social as well as non-social phenomena. It is precisely this logic of validation, explanation, prediction, which is being referred to when Winch claims then, on the contrary, that the *methodology* of the non-social science is *not* applicable to the investigation of social phenomena.

Now, in general, the context of validation is the context of our concern when we raise questions or confront problems connected with validating or justifying the acceptance or rejection of some scientific hypothesis or theory *regardless* of how we have come to discover or entertain that hypothesis or theory. To the context of discovery, on the other hand, belong such questions as how, in fact, *does* one come to "latch on" to good hypotheses?; or, what social, psychological, political, or economic conditions will conduce to the *discovery* or "the thinking up," of fruitful hypotheses by scientists? In short the issues or questions which are appropriate to the context of discovery are, themselves, substantive issues or questions in the social sciences. They are, for example, questions to be answered by the Sociology, or Psychology, or History of Science rather than by the Philosophy of Science. The question of how Harvey came to think of the hypothesis of the circulation of the blood is a substantive question of the History of Science. The question of whether this hypothesis has been sufficiently confirmed by the evidence amassed for it is obviously a quite different question (and one of the easier ones), belonging to the Philosophy of Science.

Again, I emphasize this elementary and familiar point because it helps make clear at least one aspect of what has otherwise been a fairly murky region of controversy. In particular we can see that this claim (that the social and non-social sciences are methodologically different) cannot rest upon or be bolstered by *any* examples or arguments which purport to show either the relative inaccessibility to observation of social phenomena, or the difficulties of constructing social theories that might result from the relative complexity or obscurity of social phenomena.

Thus, consider an argument representative of those given by many people who hold that what they call "the methods of the natural sciences" are not applicable to social phenomena. Suppose, their argument goes, that a Martian were suddenly deposited on earth; suppose, moreover, that the first thing he witnesses is a social act consisting of a man voting Labor in a general election. It is argued by separatists that however well the Martian might be able to describe the purely physical characteristics of this event or explain it *as* a physical event, he could never explain or describe any of its peculiarly social aspects; for, the meaningfulness of the event, the very thing, it is held, which makes it a social phenomenon at all, would be irremedially lost to him.

Now, I quite understand that examples and claims of this kind have important and subtle ramifications for a variety of issues in the philosophy of social science—some of these, but certainly not all, I shall be addressing below. For the moment, however, I am concerned with the example, and the concomitant claims made for it, only as these may be seen to bear upon a somewhat more

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superficial feature of the complex problems that are suggested. I restrict myself here, in fact, to what is the import of the example for the Context of Discovery. In just this sense, then, it should be noted that we are being confronted in the example with what is actually an empirical hypothesis in the Sociology of Science. An empirical hypothesis about the psychology of Martians—or, perhaps, more generally about the limitations of alien imaginations. The example itself hypothesizes that creatures of certain kinds of cultures or backgrounds will be psychologically incapable of thinking up certain kinds of hypotheses.

Of course, there is no good scientific evidence which would scientifically warrant the acceptance of such a hypothesis about Martians. Indeed, there is surely not yet extant any sound body of psychological or sociological theory which makes the concepts in which the hypothesis itself is couched very precise. But suppose one waives all such impediments to the scientific acceptability of this hypothesis about the limitations of alien imaginations. Suppose the hypothesis about Martian psychology to be true. What is important to note is that this would at most establish that the investigation of social phenomena by an alien is technically very difficult to accomplish or, even, empirically impossible--i.e. contrary to psychological law. Nevertheless, to infer from this, supposing it to be established, that the methodology—the logic of validation—of the social sciences must be radically distinct from the methodology of the non-social sciences is simply to be guilty of a *non sequitur*. And just this has been my point in this section. I am tempted to assert *a fortiori* that if Winch's thesis of methodological distinctness is not, logically, capable of being bolstered by even the sort of example just adduced, then no instance from the context of Discovery can bolster it.

Part II

Although I have been concerned to begin by emphasizing the point that Winch's thesis is about methodology, it should not be thought that this by itself tells against Winch or that he is unconscious of the point. On the contrary, the explicit overall structure of Winch's argument involves, first, an attempt to show that Philosophy, or at any rate, Epistemology is activity that is *methododologically* distinct from Science (Chapter 1), and second, an attempt to show that Sociology, as the discipline concerned with "the nature of social phenomena in general," as the discipline which has as its "central problem . . . that of giving an account of social phenomena in general, itself belongs to philosophy" (pp. 42-43). Winch claims indeed that this field of social science "is really misbegotten epistemology . . . . 'misbegotten' because its problems have been largely misconstrued, and therefore mishandled, as a species of scientific problem" (p. 43). To substantiate this conclusion he tries to show that the entire paraphernalia of scientific validation (including, e.g., the empirical testing of theories containing lawlike sentences—sentences testable through their uses in explanation and prediction), is irrelevant to the problem of giving an account of, or coming to an understanding of, the kind of behavior he calls "meaningful behavior." Meaningful behavior is a kind of behavior which he takes to be not merely coextensional with social phenomena but also to constitute its very

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essence. Moreover, since he takes linguistic behavior to exemplify, *par excellence*, meaningful behavior (though by no means to exhaust it), a great deal of his argument is specifically focused on the nature of linguistic behavior and upon showing that application of scientific method to its study is "misbegotten."

What are the arguments which Winch advances to support this extraordinary conclusion? First of all, the term 'meaningful behavior' is defined or, at least, analyzed by Winch in terms of the concept, 'rule governed behavior'. He believes that for an event to constitute a social phenomenon it must be a meaningful act or event and, indeed, conversely. Moreover, for it to be a meaningful act or event—such as, paradigmatically, a bit of linguistic behavior is—it must be rule-governed (p. 52). But what does it mean for any behavior to be rule governed? Well, it does *not* mean that the agent who is manifesting the meaningful or rule-governed behavior need to attributing a "sense" or need have a "subjective intention" or "subjective direction" for his action (p. 47). The agent need have no "conscious motive" nor any conscious "reason" nor any "reason" at all for his action (p. 48). In fact, a meaningful action may well be a merely habitual one (pp. 57ff).

On the other hand when an act *is* rule governed or meaningful or has a sense it is the kind of act which "commits" to other acts—not as cause of these other acts but as symbol of them—it symbolizes them. Winch says, "Action with a sense is symbolic: it goes together with certain other actions in the sense that it *commits* the agent to behaving in one way rather than another in the future." (p. 50) The notion of an act commiting to another act is, Winch claims, "identical in form with the connection between a definition and the subsequent use of the word defined." (p. 50) It follows, he tells us, "that I can only be committed in the future by what I do now if my present act is the *application of a rule*." (p. 50) Moreover, just as one need not have a "conscious motive" nor any "reason" for one's act in order for it to qualify as rule governed, so one also need *not* consciously applying any rule. Winch claims that "the test of whether a man's actions are the application of a rule is not whether he can *formulate* it but whether it makes sense to distinguish between a right and a wrong way of doing things in connection with what he does. Where that makes sense, then it must also make sense to say that he is applying a criterion in what he does even though he does not, and perhaps cannot, formulate that criterion." (p. 58)

The elucidation here, of rule-governed or meaningful behavior in terms ultimately of behavior about which there is some point in saying that it was right or it was wrong, is in apposition with Winch's earlier interpretation of Wittgenstein's analysis of what it is to follow a rule. On this interpretation the notion of *following a rule* is said to be "logically inseparable from the notion of *making a mistake*. If it is possible to say of someone that he is following a rule that means that one can ask whether he is doing what he does correctly or not." (p. 32) And just what is it, on this view to make a mistake? Well, "A mistake is a contravention of what is *established* as correct; as such, it must be *recognizable* as such a contravention. That is, if I make a mistake in, say, my use of a word, other people must be able to point it out to me. If this is not so, I can do what I

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like and there is not external check on what I do; that is, nothing is established." (p. 32)

Throughout the book, Winch bulwarks both this analysis of meaningful behavior as rule-governed or corrigible behavior and of its being the *differentia of* social behavior, by the marshalling of copious illustrations and the refutation of many putative counter-examples. While Winch's discussion by no means clears up difficulties about the meaning of 'rule' I shall take his analysis, adequately illuminating for the present issue; it is not my intention, at any rate in this essay, to challenge it. It is rather with his contention that social phenomena, thus identified, are not amenable to scientific investigation that I am concerned to quarrel.

I have claimed that in maintaining that social phenomena are inaccessible to the scientific method Winch is explicitly aware of offering an argument in the Context of Validation. He makes this quite clear in a number of passages scattered through the book. Thus, he speaks of the case he has been presenting as one which says "that the understanding of society is logically different from the understanding of nature . . . " (p. 119). In another place, he berates Weber for thinking "that the kind of 'law' which the sociologist may formulate to account for the behavior of human beings is logically no different from a 'law' in natural science." (p. 117) In still another passage on the Weberian notion of *Verstehen* and on some of its critics, he shows that he is aware of the precise relevance to his own argument of the distinctions between techniques of inquiry and methods or logics of inquiry, and between the Context of Discovery and the Context of Validation (pp. 111ff); and he accepts, indeed insists on, these distinctions in the arguments which follow. Challenging the tenability of these distinctions, consequently, is scarcely a recourse open to Winch or to those who argue like him that a different methodology is appropriate to social science. Accordingly, I shall take these distinctions, as characterized in the first section of this essay, to have been sufficiently established for the purposes of the present discussion and shall employ them in what follows without any further attempt to defend or substantiate them.

His central thesis that an "understanding" of social or meaningful phenomena requires a logic of inquiry radically distinct from that appropriate to non-social phenomena, involves two main themes or general lines of argument. These two themes constantly reappear in Winch's treatment. Moreover, the two are so tightly intertwined or entangled in some of the examples and discussions that one sometimes gains the strong impression that Winch's failure to disengage them stems from his not seeing that they *are* two different lines of argument. They are, to be sure, closely connected themes insofar as they both issue in an insistence that the social scientist unlike the "natural" scientist must work from *inside* the domain he studies. Nevertheless the two are separable themes—at least insofar as they appear to depend on different sorts of grounds for their support. Again, though Winch presents many different specific arguments for the separatist thesis—usually in connection with attempts at refuting diverse objections to it—all of these specific arguments though appear to have in common one or the other or both of these more general themes or lines of

argument as their chief burden. I think it is fair to say that if it can be shown that these themes or general lines of argument are unsound, or even that there is no good reason for believing them to be sound, we will have shown that Winch's kind of separatism need not be credited.

I'm afraid neither of the two can be described in very brief compass. I shall consider each of them in a separate section, and hope that you have a clear enough notion of them at the end of each of the sections. The first theme, then, involves concepts that Winch employs again and again at crucial junctures in his discussion but that are nowhere given any definitive analysis by him; I mean, the related notions of *understanding a social phenomenon* and of *a phenomenon's being intelligible*.

It is, of course, hard to cavil at Winch's unclarified assumption that the aim of any science is the provision of an understanding of its subject matter—or, more specifically, that the aim of any scientist *qua* scientist is to gain an understanding of the phenomena he investigates. This surely seems an innocuous enough admission. But Winch makes it the foundation of the first theme or line of argument. With a basis on this specific foundation taken for granted as a first step, the line of argument employed proffers us a second step that seems equally harmless; namely, that a phenomenon can be said to be understood if and only if it is intelligible. I must admit that the next step now taken, step three, which appears in several guises and in various contexts, *is* a bit dazzling when it is held up nakedly for examination. In, perhaps, its most compelling form, it goes like this: if a phenomenon is a *meaningful* one (i.e. if it is a rule governed or social phenomenon) then it is not *intelligible* unless its *meaning* can be understood. Now, if we remember that understanding the "sense" or the "meaning" of a rule governed act or event consists in "knowing what it is to follow the rule" which in turn is identical with knowing how to construe the act as right or wrong, correct or incorrect, contravening something established or not contravening it, then the conclusion seems almost inevitable.

I say "almost," because one more nail remains to be driven home. To avoid a *reductio* Winch needs to have shown that although understanding the phenomena it investigates is the aim of any field of investigation still *different kinds of understanding* are appropriate to *different kinds of investigation*. The kind of understanding appropriate to a physical-science investigation is not the kind appropriate to a philosophical—in particular, an epistemological—investigation—and specifically an epistemological investigation of conformance to linguistic or, indeed, any other kind of rule. The kind of understanding appropriate to physical science investigations, he appears to concede, may be gained through the acquisition of nomological explanations of such phenomena—that is, through 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. But the understanding thus acquired, he claims, is obviously not the kind of understanding sought in philosophical investigation—an area in which talk of seeking nomological or causal explanations is simply misconceived. In fact, the kind of understanding appropriate to the philosophical investigations relevant here, that is to epistemological investigations of what

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it is to follow some rule, *is constituted precisely of coming to know or understand or learn the rule, i.e., coming to know, or understand, or learn what behavior contravenes it and what behavior does not.* Since this, in turn, is precisely the desideratum in the investigation of all meaningful behavior, i.e. all social phenomena, we are apparently forced to the long signalled conclusion that the aim of social science which is, of course, to understand social phenomena, can only be a philosophical aim, can only be consummated by the achievement of what is *par excellence* a philosophical kind of understanding rather than through a patently inappropriate "natural science" kind of understanding. It is simply a corollary that the methodology appropriate to philosophical investigations is distinct from that appropriate to what we may call causal investigations. At the same time a *reductio*, suggested by uses of 'understanding' and 'intelligibility' in connection with step 2, to the effect that all scientific investigations turn out to be philosophical investigations, is avoided by the establishment (and this is made the first order of business—being attended to in chapter 1) of the fact that there are different kinds of understandings.

Let me recapitulate the steps, in severely telescoped form for ease of recollection:

1. The aim of any investigation is an understanding of the phenomena investigated.
2. A phenomenon is susceptible of being understood if and only if it is intelligible.
3. A meaningful phenomenon is not intelligible unless its meaning can be understood.

∴ α) Understanding social phenomena presupposes understanding their meanings.
∴ β) The methodology appropriate to the social sciences is radically distinct from the methodology of the non-social sciences.

This line of argument or theme occurs, as I indicated, in various guises, more or less truncated, at a number of junctures. There is one particular juncture, indeed, which is pivotal in the book, and examining the argument as it occurs at that point will furnish the opportunity of stalking the beast in its native habitat. The example in question is one in which J. S. Mill is being taken to task for holding that the methodology of investigations of social institutions is the same as that employed in the "natural" sciences. Winch lays the groundwork for his attempted refutation of Mill by first pointing out that when we say a "natural" scientist has come to an understanding of the phenomena he is investigating we are committed to holding that he stands in two sorts of relationship. One is a relationship to his fellow scientists and their's and his mutually rule governed behavior—behavior by virtue of which alone he can be said to have achieved an understanding. For to say, e.g., that he has confirmed a generalization or a statement of uniformity presupposes that he has made "judgments of identity" (p. 83). It presupposes some judgment of identity because "a regularity or uniformity is the constant recurrence of the same kind of event on the same kind of occasion." (p. 83) Now, Winch believes he has already amply demonstrated

(and for the present purposes I shall not question this belief) that judgments of identity in a given context—that is, judgments of what are to count as referents in using terms like 'same' or 'the same'—are themselves particularly important types of rule-governed behavior. In this case the first relationship, being claimed by Winch to hold between or among a "natural" scientist (when he investigates *qua* scientist) and his fellow scientists, is that of being a co-participant in what might be called the language-game of the science. The second relationship is one he stands in to the phenomena being investigated. Such phenomena present themselves to him "as an *object* of study; he observes them and notices certain facts about them." (p. 85)

According to Winch, if Mill is correct in holding the methodology of investigations of social institutions to be the same as that of the natural sciences, then the social scientist's case, respecting these two kinds of relationship will be identical with the case of the "natural scientist." Winch tells us—and I had better quote the relevant passage at length—that

> On Mill's account, understanding a social institution consists in observing regularities in the behaviour of its participants and expressing these regularities in the form of generalizations. Now if the position of the sociological investigator (in a broad sense) can be regarded as comparable, in its main logical outlines, to that of the natural scientist, the following must be the case. The concepts and criteria according to which the sociologist judges that, in two situations, the same thing has happened, or the same action performed, must be understood *in relation to the rules governing sociological investigation*. But here we run against a difficulty; for whereas in the case of the natural scientist we have to deal with only one set of rules, namely those governing the scientist's investigation itself, here *what the sociologist is studying*, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as 'doing the same kind of thing' in relation to that kind of activity.
>
> An example may make this clearer. Consider the parable of the Pharisee and the Publican (*Luke*, 18, 9). Was the Pharisee who said 'God, I thank Thee that I am not as other men are' doing the same kind of thing as the Publican who prayed 'God be merciful unto me a sinner'? To answer this one would have to start by considering what is involved in the idea of prayer; and that is a *religious* question. In other words, the appropriate criteria for deciding whether the actions of these two men were of the same kind or not belong to religion itself. Thus the sociologist of religion will be confronted with an answer to the question: Do these two acts belong to the same kind of activity?; and this answer is given according to criteria which are not taken from sociology, but from religion itself.
>
> But if the sociologist of religion's judgments of identity—and hence his generalizations—rest on criteria taken from religion, then his relation to the

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> performers of religious activity cannot be just that of observer to observed. It must rather be analogous to the participation of the natural scientist with his fellow-workers in the activities of scientific investigation. Putting the point generally, even if it is legitimate to speak of one's understanding of a mode of social activity as consisting in a knowledge of regularities, the nature of this knowledge must be very different from the nature of knowledge of physical regularities. . .
>
> This point is reflected in such common-sense considerations as the following: that a historian or sociologist of religion must himself have some religious feeling if he is to make sense of the religious movement he is studying and understand the considerations which govern the lives of its participants. A historian of art must have some aesthetic sense if he is to understand the problems confronting the artists of his period; and without this he will have left out of his account precisely what would have made it a history of *art*, as opposed to a rather puzzling external account of certain motions which certain people have been perceived to go through.
>
> I do not wish to maintain that we must stop at the unreflective kind of understanding of which I gave as an instance the engineer's understanding of the activities of his colleagues. But I do want to say that any more reflective understanding must necessarily presuppose, if it is to count as genuine understanding at all, the participant's unreflective understanding. And this in itself makes it misleading to compare it with the natural scientist's understanding of his scientific data. (pp. 86-89)

The portentious character of the thesis Winch purports to be demonstrating or illustrating in this passage should not be underestimated. He believes that the argument given here, and similar ones, show that Mill is wrong in regarding "all explanations as fundamentally of the same logical structure" (p. 71) and that Mill is wrong in believing "that there can be no fundamental logical difference between the principles according to which we explain natural changes and those according to which we explain social changes."

What has now been outlined above is, perhaps, sufficient to convey both the import of what I've called Winch's first line or argument as well as something of the flavor of his style of argument. It is now time to turn to a more direct evaluation of his argument.

To those acquainted with Michael Oakeshott's essays on politics and political behavior collected in *Rationalism in Politics and Other Essays*,[5] Winch's uses of terms like "unreflective understanding" will very likely have struck a familiar chord. Winch's employment of such terms is no accident. Although he has a relatively minor disagreement with Oakeshott about whether habitual behavior is rule-governed behavior (and thus requires the possibility of the kind of reflectiveness appropriate to understanding rule-governed behavior) he is for the most part self-consciously in agreement with Oakeshott's general thesis about what might be called the primacy, or the fundamental character, of *unreflective*

knowledge in coming to the sort of understanding appropriate to investigations of social phenomena.

Whatever the profundity or complexity of the views which Oakeshott and Winch build on the distinction between reflective and unreflective understanding (and independently of the finer shades of difference between them) the distinction itself seems simple-mindedly straightforward. It resembles closely both that venerable friend of ours, the distinction between *knowledge by acquaintance* and *knowledge about*, and also a somewhat newer friend, the distinction between *knowing that* and *knowing how*. Consider how it is that we may come to have knowledge of, or an understanding of, driving a motor car on English roads, to use one of Oakeshott's examples (*op. cit.*, p. 7). No amount of studying the rules, or laws, or descriptions of the relevant activities can substitute for actual driving. More precisely (though I am not sure that Oakeshott sees this), these cannot substitute for the actual experience-of-driving. The reason for this, in the final analysis, if I may abort a rather tedious recital, is that when in certain contexts we *refer* to a knowledge, or an understanding, of driving on English roads, we are in such contexts making an essential reference to such experiences—we would in such contexts withhold application of the predicate 'knows how to drive on English roads' from any individual who had not had such experiences. The difference between reflective and unreflective knowledge of how to stop a motor car is precisely the straightforward one that the latter necessarily includes experience of handling a car, for example, experience of treading on a brake pedal.

Now, I do not wish to deny that there are varying shades of difference among all the many dichotomies I have been mentioning. Nor do I want even to deny that the differences may come to be significant in seeing the differences among the overall views of, say, Russell, Ryle, Winch, and Oakeshott. But whatever their significance in such a more comprehensive connection, these differences are not important here. I shall be concerned only with a characteristic which all the dichotomies have in common. What they all have in common is the delineation of an opposition between what we may call direct experiences of objects or events on the one hand, and more indirect relationships to such events, such as formulating or reading descriptions of such events, on the other.

With respect to the "natural sciences" there is surely now general agreement that a *scientific* understanding of things or events of a given kind does not necessarily presuppose direct experience of such things or events. Indeed, as Winch himself points out, we have acquired the understanding appropriate in "natural science" when we have, say, a nomological explanation of the event being investigated.

Still, there have always been philosophers (Bergson, and Whitehead if I correctly understand him, are notable examples) who, while agreeing that attainment of scientific explanation of, say, physical phenomena, is as much as can be attained by physical science in the way of understanding such phenomena, have nevertheless taken this to be symptomatic of the deficiency or limitations of the scientific method in general—even as employed in physics. A typical claim in this vein is one to the effect that science distorts through

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abstraction from physical reality. It is held, for example, that a scientific description of a tornado conveys in only a feeble, truncated, wretchedly poor manner what is conveyed with overpowering richness and fullness by the direct experience of a tornado.

Nowadays, few of us are so analytically naive as to be taken in by this kind of view about deficiencies or limitations alleged to characterize science. We, most of us at any rate, comprehend the point of Einstein's remark that "it is not the function of Science to give the taste of the soup." It is the function of science to describe the world, not to reproduce it. Of course a description of a tornado does not provide us with a tornado; it is not the same thing as a tornado! And, incidentally, the description does not *fail* to be a tornado on account of being incomplete, or truncated, or generalized. Even if it were a "complete" description of a tornado—whatever that might be—it would still be a *description* of a tornado and not a tornado. Moreover, a description of a tornado no more fails to be a tornado than does a tornado fail to be a description. No, it should be clear enough that to allege deficiencies or limitations to science in the above fashion is simply to be mistaken or confused in a particularly egregious way about the nature of science. To be sure, if we are good scientists and if we are lucky, we can use our scientific descriptions to help lead us to the experiences we desire of the extra-linguistic world—but, to expect those descriptions to be the extra-linguistic world or even to be very much like it, obviously misconstrues the enterprise. And really, to make such a mistake is to commit a kind of solecism very much like "accusing" my eyeglasses of failing, or of being defective, because they do not have enough thrust to propel an Apollo space capsule to the moon.

I have not been able to resist dubbing the fallacious view that the function of science is to reproduce reality the *reproductive fallacy*.

This is all very well; but how exactly, you are doubtless wondering, does it bear upon Winch's line of argument about social phenomena? The answer is, I believe, that Winch's argument commits a somewhat subtle form of the reproductive fallacy; and having elucidated the latter in the neutral context of the physical sciences enables me to deal much more succinctly with Winch's argument.

The commission of the fallacy does not, I think, lie in Winch's contention that social phenomena are rule governed phenomena, nor, of course, in his seeing that an investigation of rule governed phenomena does, itself, constitute a rule governed phenomenon. Nor, does it lie in his seeing, quite correctly, that the sorts of rule governed phenomena constituted of, say, religious activity or religious institutions are broadly of the same kind as the rule governed phenomena constituted by an attempt to verify some causal theory through an explanatory or predictive use of it. But we do get a commission of a species of the reproductive fallacy when, granting all this, it is inferred that the only methodology appropriate to the investigation of a rule governed phenomenon is one that issues in a *reproduction* of the condition it investigates.

Suppose, for want of a better, we employ the term 'teleology of the observer' to refer to the relevant characteristics of the scientists' "game"—including the

verificational canons, instances of conformance to them, the conceptual apparatus of nomological or causal theories, explanatory uses of these, predictive uses of these, etc. And, correspondingly, suppose we use the term 'teleology of the observed' to refer to relevant characteristics of some social phenomena (e.g., religious behavior) which we wish to investigate. What Winch, and others who hold similar views, are claiming then is that the only way in which such a social science investigation can achieve understanding is via the adoption by the social scientist of the teleology of the observed.

But this surely is an idea of *no* social science. It is, logically, the same as claiming that the only understanding appropriate to investigation of tornados is that which is gained in the experience of tornados; and anyone who put forward the idea that the only kind of understanding at which we should, or could, aim in connection with the investigation of tornados be confined to whatever understanding comes to us in the experiencing of tornados, would be having an idea of no Meteorology. Notice that in rejecting Winch's thesis, it is not necessary to deny that some sort of knowledge or understanding of, say religion, is gained in "playing" the religious "game" any more than it is necessary to deny that some sort of knowledge or understanding is gained in experiencing tornados or in driving on English roads. The point is that nothing whatever in such a concession implies that these "direct" understandings are the only possible ones; nor would such a concession imply that the scientist's "game"—the adoption of the teleology of the *observer*—is at all inappropriate for achieving the sort of understanding, a quite different one to be sure, which it is the function of successful scientific investigation to provide.

Again, notice that the mere fact that both the observed and the observer in social science investigations are rule governed, the fact that the teleology of the observed and the teleology of the observer are both teleologies, does not *per se* demonstrate that the teleologies must be identical. No more than does the fact that both verbal inscriptions describing tornados, and also tornados, have mass, demonstrate that inscriptions are tornados.

In sum, Winch's argument here exemplifies what might as well be termed the 'fat cow-herd' sub-species of the reproductive fallacy. This conclusion is also supported by a close look at the third step in the main line of argument which was presented above. The third step you will remember was given in the claim that

3. A meaningful phenomenon is not intelligible unless its meaning is understandable.

Now, the truistic second step presumably sanctions our replacement of 'intelligible' in 3 by 'understandable' thereby yielding

3'. A meaningful phenomenon is not understandable unless its meaning is understandable.

But the transformation of 3 into 3' makes quite patent, in the light of the con-

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siderations we have already adduced, the equivocation on the notion of *understandable* which is embodied in the step.

I have not denied that one kind of understanding of a social, or meaningful, or rule-governed, phenomenon consists in understanding its meaning in the sense of playing the "game" relative to which it has that meaning. In this sense, indeed, understanding the meaning is identical with playing the "game" which in turn, of course, is identical in this context with following the rules of the game. But there is no reason whatever for believing that this sort of understanding is the only sort of understanding of meaningful phenomena possible, nor any reason whatever for believing that it is the sort of understanding appropriate to scientific investigation of social phenomena. In short, for Winch's 3rd step to be acceptable, its two occurrences of 'understandable' must be univocal, and must carry the sense of *direct understanding* just outlined. But if 'understandable' carries this sense in its occurrences in step 3, then it must be equivocal relative to its occurrences in step 2 or step 1 which depended on a sense of the term not thus restricted for their plausibility or acceptability as truistic. On the other hand, if the two occurrences of 'understandable' in step 3 are equivocal relative to each other then 3 is not acceptable as true. In neither case, therefore, does the line of argument commit to the truth of the conclusions Winch draws. What, if anything, he shows on this line of argument simply doesn't establish his case.

Part III

Yet, having shown the defectiveness of this one line of argument, having shown that the existence of a teleology of the observed does not *per se* presuppose its adoption by the observer who seeks a scientific understanding of the observed's behavior, does not by any means wholly dispose of Winch's general thesis. There is, as I have mentioned, a second main theme, or line of argument, which is threaded through the work and which must also be considered. If we construe the line of argument examined in the preceding part as an attempt to establish a positive thesis, we may profitably, I think, treat this second line as an attempt to establish a corresponding negative thesis. Thus, the argument already considered may be thought of as one which is for the adoption of a *philosophical* method as candidate for the office of methodology of the social sciences. Correspondingly, his second theme may be thought of as an argument against what I shall from now on simply call the scientific method as such a candidate. The two lines of argument are closely related and, as I've indicated, Winch himself shows no particular interest in disentangling them. But they do not at all come to the same thing and they require independent refutation as well as independent establishment.

It will shorten our treatment of this second theme if attention is confined primarily to just one kind of social phenomena: linguistic behavior or activity. It is clear that the case of linguistic behavior, of language as a social institution, figures with surpassing prominence in Winch's own treatment. It has, of course, figured centrally in many discussions ranging over the literature on similar controversies; and, in any case, I should not be inclined to challenge the general-

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izability, of what might be established in the present context for linguistic behavior, to other social phenomena. If we confine our attention to language and linguistic behavior, the argument may be seen to be one construable in terms of what Quine calls *radical translateability*.[6] If we were to generalize from linguistic to non-linguistic social behavior, we should have to find some suitable term other than 'translateability' to apply to the general problem or else widen our notion of 'translation' or of 'language' so as to render them applicable to what are now thought of as non-linguistic social or meaningful phenomena. But this is a problem of no overriding severity; for present purposes, we may assume that it is solvable, and not allow it to detain us.

To begin with, let us also take as unproblematic Quine's characterization of the nature of radical translation. He remarks of that intrepid social scientist, the linguist, that his task is "the recovery" of a person's language "from his currently observed responses" (*op. cit.*, p. 28). The linguist "unaided by an interpreter is out to penetrate and translate a language hitherto unknown. All the objective data he has to go on are the forces that he sees impinging on the natives' surfaces and the observable behavior, vocal and otherwise, of the native" (*ibid.*). Still, not all types of translation are germane. Translations between kindred languages may be helped by the occurrences of verbal cognates and between unrelated languages, aid might be afforded—if there has been previous contact—by "traditional equations" based on other shared items of culture. No, as Quine puts it, "What is relevant rather to our purposes is *radical* translation, i.e., translation of the language of a hitherto untouched people" (*ibid.*).

Having given this characterization, Quine devotes the next 50 pages of his book to the vicissitudes of radical translation—tirelessly drawing morals from and brilliantly illuminating an amazing number and variety of, quite general, epistemological issues, but nevertheless showing that the linguist proceeds by employing the scientific method: that is, he frames empirical hypotheses about the meanings of the native's vocalizations under conditions which he (the linguist) sets, and he (the linguist) checks these hypotheses by arranging a variety of test situations and circumstances whose outcomes he (the linguist) observes.

Now, the relevance of the work of Quine, and others similarly disposed, to Winch's thesis is clear. Winch's thesis may be construed simply as the view that radical translation is impossible—not merely terribly difficult, notice—but logically impossible; to attempt it would be, to use his word, to engage in a "misbegotten" enterprise necessarily doomed to failure.

It is likewise clear from the organization of his own book, that Winch would not linger to quarrel over the truth or falsity of the early putatively *empirical* hypotheses of the linguist. He would, in fact, attack the enterprise at a more profound level by challenging the very sense of a putative hypothesis like,

H_1: " 'Gavagai' means the same as 'Rabbit'."

For, Winch claims, the sense of the occurrences in such hypotheses of terms like

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'means' and particularly, 'the same', is precisely what is fundamentally at issue. To assume that one can use such terms in conformance with the teleology of the observer, i.e., with meanings governed by the rules of the *observer's* language, is, he would say, obviously to beg the question. The way to get to understand the native's language is by learning it in approximately the same way the native did; that is, by adopting the teleology, not of the observer, but of the observed—by engaging in behavior *rule-governed by the native's linguistic rules.* Just this was the burden of the argument examined in the preceding part of this essay. But, in fact, Winch is also urging something beyond this: He is claiming that there is no other method which can yield an understanding of the native's language; *in particular, that no application of the scientific method, as in the procedure indicated above of radical translation, can possibly do so.*

The outline of this second major theme or line of argument of Winch's, then, may be presented fairly briefly. The first step consists in pointing out that certain fundamental processes such as *judgments of identity* (which, on the level of linguistic behavior, are represented by the occurrence in statements of terms like 'the same') are rule governed processes. That is to say, what will count as the same in any context is decidable only relative to some set of rules. Again, on the linguistic level, this would amount to the assertion that the meaning of terms like 'the same' is established only by the rules of the language to which they belong. Step two, of this "free" reconstruction of Winch's argument consists in pointing out that the rules governing, say, judgments of identity—and their linguistic counterparts—are not likely to be the same for both the language of the observer and that of the observed. Even more tellingly, in this step of the argument, it is being held that there is no way, in principle, for the radical translator to tell whether the rules are the same rules or are not. While for him to assume that they are the same is to beg the question.

Step three is now a thrust of the other horn: if the radical translator does *not* assume that they are the same, then he can frame no meaningful empirical hypothesis at all. For what is true of terms like 'means' and 'the same', is equally true of such key terms as 'name', 'predicate', 'sentence', and 'term' and, most crucially, '*rule*' itself. Accordingly, if, for example, the radical translator does *not* assume that 'Gavagai' is, in the native's language, a name or a predicate or a rule-governed vocalization, in some sense of these categories sufficiently like their sense in the native's language, then no meaningful hypothesis of translation can be framed at all. We can perhaps clarify this important step of Winch's argument a little more by following through on an example. Consider the putative hypothesis,

H_2: " 'Gavagai' means Rabbit."

But whether 'Gavagai' does mean Rabbit is obviously dependent in some measure on what 'means' means—indeed, whether or not H_2 is a nonsensical locution depends upon what 'means' means. But (Winch would presumably continue) 'Gavagai' can't mean at all in the English sense of this latter term, for

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'means', has *its* meaning elucidated or determined relative only to the rules of English—rules which have no applicability in the native language; while, on the other hand, the term 'Gavagai' has its meaning elucidated or determined relative only to the rules of the native language—in particular, relative only to rules determining the meaning of whatever native counterpart there may be for the English word 'meaning.' Thus if the translator does not assume that the rules of the native language are sufficiently like the rules of English, he cannot even frame an intelligible hypothesis about the meaning of native vocalizations.

Winch, of course, does not couch the premises of his argument in just this fashion; nevertheless, I think this is a fair (perhaps even more than fair) presentation of their import. In any case, the conclusion of his argument is, as indicated above, that the scientific method is inapplicable in coming to an understanding of alien language behavior—that is, in the present context, his conclusion amounts to denying the possibility of radical translation.

How good is this argument? It is wrong-headed though deep. It is a profound argument in the sense of having implications not only for current controversies in the Philosophy of Social Science and the Philosophy of History, for current controversies about Ethics and about intentional phenomena generally, but also implications for the problem of the nature of philosophical analysis itself—and in particular for the problem of what kind of pursuit Ordinary Language Philosophy turns out to be. However, I must here restrict myself to delineating the wrong-headedness of Winch's argument rather than elucidate its profundity in this respect. I did, though, want to indicate, even if scantily, the broad grounds I have for thinking that his arguments (and, indeed, the Winch-like arguments of others which have been flourishing in a recent spate of articles and books) do merit very close attention.

Still, the arguments *are* wrong-headed. To begin with, it should be noted that the task of translating a language is not the same as the task of learning a language. Patently, the criteria of adequacy in translation are different from the criteria of adequacy in learning a language. What is involved in seeing this is very much like, if not identical with, what was involved in noting the equivocal senses of the term 'understanding' which were encountered in the preceding section. No doubt, we have "learned a language" only if we have gained the kind of understanding of it which is identical with directly experiencing behaving in conformance with its rules. On the other hand, if coming to *understand* a language is among the criteria of adequate translation at all it is surely not in the sense of understanding just mentioned, but rather, one of the other senses of that equivocal term.

Consider again the hypothesis of translation

H_1: " 'Gavagai' means the same as 'Rabbit'."

In the first place it should be noted that the native word, 'Gavagai' does *not* occur in H_1. And this, in itself, is enough to make us look more doubtfully at the 2nd and 3rd steps of the line of argument above—for both of these steps appear to depend strongly on the assumption *that native words occur in* (i.e.,

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are used in) the linguist's hypotheses. In the second place, any obscurity about the term 'means' *as such* which occurs in H_1 is considerably lessened as soon as it is recognized that H_1 *in toto* is appropriately analyzable for the linguist into

H'_1: " 'Gavagai' in Language J may be translated as 'Rabbit' in Language E."

For, this is after all what the translator *is* hypothesizing. To be sure, such an analysis immediately reveals the burden which is placed by the linguist on the notion of translation itself and the linguist must address himself to making that notion clear. Nonetheless, even in advance of having done so we are free of at least one muddle: H'_1 *is a meta-linguistic* assertion relative both to the native's language and to the target language. Accordingly, the locution 'may be translated by' need be a term of neither of these languages and may be, indeed ordinarily will be, a *technical term of the science of linguistics* instead. This possibility, I believe, pulls most of the remaining teeth of the 2nd and 3rd premises.

The task of clearing up the technical notion of translation of course still remains. And though it is happily not within the province of this essay to do anything definitive about that, I can indicate that there appear to be ways of clearing it up which would give no sustenance whatever to Winch-like positions. Thus, criteria of adequate translation might be so couched that H'_1 is held to be confirmed when under certain stimulus conditions, say the visual presentation of a rabbit, a representative native, or a majority of the natives, respond by uttering 'Gavagai'. While under those same conditions (note, that 'same' here is relative to the scientist's teleology), a representative speaker—or the majority of speakers—of the target language respond with the utterance 'Rabbit'. If this, obviously too simple—but, for the present purposes, serviceable clarification of the import of H'_1 is accepted, then the empirically testable character of the hypothesis is abundantly manifest.

In any case there is no reason for restricting the linguist to just one notion of translation. The science might well utilize a family of such notions each appropriate to some different aim of the scientist and each differing from the others in, say, the degree of stringency set by the conditions of adequacy for translations of that type. In this light, then, Winch's 2nd or negative line of argument seems wholly uncompelling too. Radical translation *is* possible.

Are there, then, any recourses which are available to Winch for meeting these objections to his argument when that argument is construed as one about radical translation? His book suggests that he might take either of the following two:

1. He might say that the notion of radical translation construed, as above, in terms of a linguist's empirical hypotheses of translation, does achieve some cogency; but only at the cost of relinquishing its claim to constitute an inquiry into what are peculiarly social phenomena. For, as now construed, such an inquiry could only give us an understanding of language behavior from the "outside." But for Winch to take this recourse would be tanta-

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mount to a retreat to the argument assessed and found wanting in part II of this essay.

2. He might on the other hand attack the cogency of this notion of translateability by pointing out two, connected, ostensible weaknesses. Both appear to draw support from Quine's thesis of the indeterminacy of translation. One of them is that even if our criteria for adequate translation were to be a condition as weak as the one which Quine calls 'stimulus synonymy', one could never, in principle, be sure that the translational hypothesis being accepted was true. The other weakness Winch might impute is the closely related one that for every translational hypothesis which the linguist might formulate, it is demonstrable that it would be possible to formulate a second hypothesis, incompatible with the first, which was yet not only confirmed by all the evidence now confirming the first, but also, in principle, such that every confirming or disconfirming test we might ever make of the first would also be, respectively, a confirming or disconfirming test of the second.

Yet, I think that these possible Winchian gambits for meeting the objections I have raised in part II also come to naught. The first gambit is replied to by pointing out that our never being sure of the truth of translational hypothesis beyond the possibility of error, is simply a condition of every empirical hypothesis. It could only be construed as a weakness by begging the very question of whether our knowledge of social phenomena must be of a kind other than that vouchsafed through applications of the scientific method.

The second gambit is considerably more complex and requires, perhaps, a much more complicated reply. Still this reply has, I think, *already* been given both by Quine toward the end of his discussion of radical translation (*op. cit.*, sections 15 and 16, especially pp. 71-79), and by N. Goodman in his article "The Way the World Is;"[7] I can remind you of something their (fuller) answers have in common by quoting the apothegem which occurs near the close of Goodman's essay—although he is addressing himself to mysticism there, his remark is obviously relevant here as well: "The answer to the question 'what is the way the world is? What are the ways the world is?' is not a shush, but a chatter." (p. 55)

In any case, one point about the discussion of radical translation must, perhaps, be noted here in a little greater detail. It is this: although I believe the account of translational hypotheses given above to be compatible with Quine's treatment of such hypotheses (and, indeed, the rebuttal of a Winchian argument this account contains, likewise to be compatible with Quine's views of such hypotheses) I am not, of course, in that account attempting any gloss of the recondite problem of the *indeterminacy of translation.* There already exists a long and complex literature on this latter problem—a literature to which Quine himself makes notable additions from time to time. But, both the original treatment of the concept of Indeterminacy of Translation (and some of its companion concepts, such as that of the inscrutability of terms) makes it abundantly clear that Quine's views do not bulwark separatist positions in

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social science. One supposes that the separatist point is that the meaning of a social act (such as a speech act) is not accessible from the "outside" of the "language community" in which it takes place (i.e., it is not accessible to scientific investigation). But this separatist view is surely not the burden of Quine's contentions on translational indeterminacy. The main thrust of his position here—as elsewhere, faithfully, in his philosophical essays—is to deny that there is anything which is *the meaning* of the native's speech act (utterance) to be conveyed in a translation. Whatever the reasons may be for calling a translation, on Quine's view, indeterminate, they are not those of failure to carry the meaning of the native's original utterance—failure to convey, or be able to unequivocally pick out what the native did *really mean*—for there is, on Quine's view, no such meaning. The question of what the real meaning of native utterance is, is precisely the kind of question "whose very significance" Quine "would put in doubt."[8] As he says, the dubiousness of the significance of just such questions is "what I am getting at in arguing the indeterminacy of translation." (*ibid.*) In short, indeterminacy of translation is a function of the empirical, or evidential, *underdetermination* of what I have been delineating as translational hypotheses—a characteristic which such hypotheses share with hypotheses of the non-social sciences—indeed, with all scientific hypotheses. It is not a function of our inability, except in a Pickwickian sense, to determine scientifically the meaning of an utterance.

All of the above considerations have brought us, finally, "by a commodious vicus of recirculation" back to the riddle and answer with which this essay began. There is a point which I hope the riddle makes: The history of philosophy, indeed, intellectual history in general, has taught us that to define, or to characterize, the determining conditions of anything, does not, fortunately, or perhaps unfortunately, entail the existence of a thing which meets those conditions. In the final analysis, it seems to me that Winch has, perhaps clearly enough, specified the conditions under which the correct answer to the question "How shall we investigate social phenomena?" would be the one *he* gives. I think though we are fortunate that those conditions are not fulfilled—and we can, if we feel so inclined regard his idea of "no social science" as being a mildly humorous one. It is reassuring to be able to recognize calmly that there are some conundrums whose clearly correct answer is "A flying lox box." We can agree to this and still feel serene in the knowledge that there are no flying lox boxes.

FOOTNOTES

[1]M. Brodbeck, "On the Philosophy of the Social Sciences," and R. S. Rudner, "Philosophy and Social Science," *Philosophy of Science*, Vol. 21, 1954.
[2]F. A. von Hayek, *The Counter-Revolution of Science*, (New York: Free Press, 1964).
[3]E.g., see *Philosophy of Social Science*, Chs. 1 and 4.
[4]All page references, unless otherwise indicated, are Winch's *The Idea of a Social Science*, (London: Routeledge and Kegan Paul, 1958). (p. 119, my italics).
[5]Michael Oakeshott, *Rationalism in Politics and Other Essays*, (London: Methuen & Co., 1962).
[6]W. V. O. Quine, *Word and Object*, (New York: John Wiley & Sons, Inc., 1960).
[7]N. Goodman, "The Way the World Is", *Review of Metaphysics*, Vol. XIV, No. 1, 1960.
[8]Quine, "On the Reasons for Indeterminacy of Translation," *Journal of Philosophy*, Vol. LXVII, No. 6, March 26, 1970, p. 181.