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In Defence of Objectivity*[1]

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Having reconciled myself to the fact that *Objectivity in Social Science*, written by me in 1973 (Toronto, University of Toronto Press), has not cast anti-objectivist attitudes from the world of Anglo-American philosophy and social science, and possessed of a now complete file of reviews and the benefit of criticisms produced during on-going debates over objectivity, I would like to return to the book's thesis.

That thesis might informally be put: that the human belief-acquiring mechanism, of which social-scientific thinking is a part, is adequate to the task of arriving at true beliefs about things human. I devoted little space to arguing in *favour* of this thesis, but suppose a *prima facie* argument is: (*a*) that since humans act on beliefs and must co-operate to a high degree with other humans to survive, it is extraordinarily improbable that the human species could have survived even this long were humans unable to acquire true beliefs about themselves, and (*b*) that there is no philosophically relevant difference in principle between social-scientific and non-social-scientific modes of acquiring such beliefs.

Most of the book is devoted to producing counter arguments to positions that objectivity in social science is impossible. My strategy was to show either that the arguments in favour of anti-objectivism are non-sequiturs, or that if they work in the case of the social sciences, then they must work also in the natural sciences and common sense and in fact in the case of any belief whatsoever, hence leading to thorough-going scepticism—a position which I took, and take, to be rationally and practically untenable.

Since its appearance the book has received the most amazing spread of reviews. Stylistically, it has been called 'incomprehensibly convoluted prose' (xiv: 366)[2] and also a 'model of clarity, organization, and incisiveness' (vii: 486). One reviewer reports, 'I find this book useless' (xiii: 535); another says that the book 'should be essential reading' (xi: 26). While two journals solicited review articles, another editor turned back a submitted (and not uncritical) review with a letter, passed on to me by the reviewer, saying that it 'could not be published' since 'the book is such a piece of s . . . , and you tried to make it look reasonable'. (The editor of the U.S.-based journal goes on to speculate that the common Canadian nationality of myself and the reviewer is what prompted reasonable treatment.) Indeed, on reading through the reviews I began to wonder whether objectivity on the subject of objectivity is possible.

Nonetheless, several kinds of arguments have been put forth both in the reviews and in formal and informal discussions of the book, response to which

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1 Thanks are due to Derek Allen and Dan Goldstick for reading and commenting on earlier drafts of this paper.

2 References preceded by Roman numerals in the text are keyed to the bibliography of reviews below, which I believe to be complete. Other page references incorporated in the text are to *Objectivity in Social Science*.

on my part may help to advance the debate. Very few of these arguments move on the terrain defined by *Objectivity in Social Science*. This latter might be described, to adapt a phrase of Robert Merton, as philosophy 'of the middle range'—attempting to avoid deeper epistemological issues, on the one hand, and questions of social-scientific methodology and the details of anti-objectivist positions examined, on the other.

I still think that there is room for philosophical work of this 'range'. My predilections regarding questions of social-scientific methodology include a favourable attitude toward methodological holism, anti-operationalism, and the seeking of causal laws. Epistemologically, I favour realism supplemented with an Hegelian theory of the interpretive nature of perception and an Engelsian theory of relative truth.[3] I believe that all of these views can be shown compatible with objectivity as defined in the book. Perhaps some alternate methodologies and epistemological views can too, in which case their proponents should welcome whatever sound arguments they can find in *Objectivity in Social Science*. Still, the criticisms have shown me that I may have defined the range of the project a bit too narrowly. I shall return to this subject. Of the few arguments that do remain within the context set for itself by the book, I shall discuss some that address 'theory-ladenness' and scepticism, since I think I must have been unclear in my treatments of these topics.

A cluster of arguments has to do with the import and defensibility of my views on the non-theory-ladenness of inquiry. One reviewer charges that the position of the book should not be called 'objectivism', but 'realism' (this is alright with me) and that the realist cannot accommodate the fact that claims to scientific knowledge 'are not to be had without the existence of disciplines by means of which they are established' (viii: 211). Another reviewer questions whether one can 'know the social life of a people' without grasping 'the meaning which their actions . . . have for these people' and that this grasping requires interpretation (ii: 628). The assumptions of these critics is that for the objectivist there must be some rock bottom base, itself involving no interpretation, on which social-scientific beliefs are built up in order for those beliefs to be objective. The argument of *Objectivity in Social Science*, however, did not suppose this. It held, rather, that *if* there is no such rock bottom base (e.g., sense data or a somehow uninterpreted world of common-sense), then *either* no belief whatsoever can be both true and known to be true, *or else* social-scientific beliefs can be true and known to be true.

Some reviewers saw that this was the general structure of the argument and raised objections to it. Thus one reviewer holds that a theory of perception that makes it interpretive 'allows only as much objectivity as Kant' (xvii: 297); since what would be perceived is not the world but only interpretations of it. My response must be that this is not 'enough' objectivity, leading, as it does, to thorough-going scepticism and that unless we are commited to such scepticism, perception must either be non-interpretive or it must be interpretive without

3 I have defended certain of the methodological preferences in 'Practice and Some Muddles about the Methodology of Historical Materialism', *Canadian Journal of Philosophy*, 3, 1973, 235-48, and 'Inductivism and the Libertarian-Ideographic Tradition', *Journal for the Theory of Social Behaviour*, 8, 1978, 137-47. One defence of the kind of realism I favour may be found in D. W. Armstrong, *Perception and the Physical World*, London 1961. Hegel's theory of perception is incorporated in his *The Phenomenology of Mind*, in the section on 'Sense-Certainty, This, and Meaning', translated by J. B. Baille, London 1931, pp. 149-60. Among other places Frederick Engels defends the theory of absolute and relative truth in *Anti-Dühring*, New York 1966, Part One, Chap. VIII.

locking us into a Kant-like world of appearances. In fact, I believe that there is room for a defensible theory of perception differing alike from one that maintains there is non-interpretive perception and from one holding that sense experience is of a 'world' formed in advance by interpretive preconceptions—namely, one according to which we fallibly but corrigibly perceive the real world *through and in* interpretive perceptions. Be this as it may, I do not think the considerations raised by the reviewer affect the structure of the book's argument. They would need to be supplemented to show that we are indeed commited to thoroughgoing scepticism or to show that there is no alternative to the Kantian theory and this theory can be stopped short of such scepticism, leaving social-scientific views on the sceptical side of the wall.

The book's argument was also appropriately criticized in its treatment of scepticism. One reviewer holds that in practice social scientists are sceptics, since they question the reliability of methods they employ and 'consider views tenable without also believing them to be true' (i: 277). Pertinent to an evaluation of this argument are some considerations I shall shortly advance about the way theories may be held. But suffice it here to note that the question is not whether practicing social scientists are aware of potentially reality-distorting features of techniques that they cannot avoid using, but whether they think it is possible to become aware of and adjust for such features. A sceptic in my sense of the word would respond in the negative. I do not see how such a person could psychologically continue to hold that the beliefs acquired by techniques regarded as irredeemably fallible are still 'tenable' (except, perhaps, in some way that has nothing to do with truth—e.g., as worth lecturing on and writing about so as to have something to do for employment.)

Along the same lines one reviewer notes that sometimes I characterize a view as sceptical if it would have the effect of scepticism in practice (as in my claim about someone who held that no beliefs about the future could be justifiably held). The criticism is that I never discuss or defend how this makes a position sceptical though 'it is clear' that I think 'a sceptic would never do anything or at least anything "political"' (xv: 134). Now nowhere in the book is it said or implied that political inactivity is sufficient for being a sceptic. In fact, it is a main intent of the book to show that unless scepticism about *everything* is justified, then it is not justified about things political. However, I do consider it sufficient to characterize a theory as sceptical if believing it (*per impossibile*) would lead to the inability to take any deliberate actions. Those who disagree with this way of regarding scepticism may wish to substitute some other word in evaluating the various arguments in which the term appears.

Of the more blanket criticisms of the book, I shall address three: that it inadequately or wrongly represents the positions of theorists criticized (Kuhn, Whorf, Mannheim, etc.); that it should address deeper philosophical issues; and that it is exclusively negative.

A disadvantage of treating arguments as they are embedded in specific authors' texts, especially more or less popular ones, as opposed to treating them in the abstract, is that one is almost certain to fail to do justice to the full contributions of those authors. Thus, one is bound to step on the toes of those who appreciate such contributions, and this is apparently what has happened in the case of several of the book's reviewers (e.g., v, vii, x, xiii, xvi). In some cases I am charged with out-and-out misrepresentation. Sometimes I think this has resulted from basic philosophical differences over the way to approach the question of objectivity. Other times the charge finds a certain basis in the fact that many of the authors I treat are inconsistent anti-objectivists, or they provide

arguments used by many social-scientific anti-objectivists while not wishing to espouse anti-objectivism themselves. Whether, allowing for these sorts of considerations, I still am guilty of misrepresentation is something readers of the book can judge for themselves. I, myself, have not changed my views about the authors criticized, though were I to re-write the book, I would make it clear that I did not intend to reject *everything* someone wrote in criticizing *some* things.

Several critics have held that the book ought to have concerned itself with such epistemological questions as the nature of truth and evidence (as in iv: 13 and xiv: 366). However, I think it is justified to meet arguments of anti-objectivists without producing such things as a theory of evidence. What is attempted is a demonstration that those who think that no objective marshalling of evidence (no matter how conceived) is possible are wrong. As to truth, I explicitly defend the possibility of objectively ascertaining truths, where this means correspondence of beliefs with what is the case. Those social-scientists who do not aim at such correspondence could find the book irrelevant to them, but I do not think I should have devoted space to trying to convince them that this is the sort of concept of truth that one ought to have.

Similarly, those social scientists who accept a correspondence theory about what 'true' means, but think it is impossible to have any justified beliefs except about what is *absolutely* true might find much of the argument of the book useless, since I defend the possibility of objectively sustaining a belief that a theory is largely or substantially true (as opposed to being true in every detail) and that it is true within (possibly unknown) limitations (as opposed to being true in all possible domains, as, for instance, early Newtonians thought their theory was). One reviewer maintains that while I have successfully defended the possibility of objectivity in the pursuit of truths considered as correspondence with facts, I have not considered the possibility that in the social sciences truths considered in some other ways ought *also* to be pursued (vi: 415). I am satisfied if I have indeed successfully defended an objectivity that supposes the correspondence theory of truth and confess to doubt that some alternate kind of objectivity can be elaborated and defended that is not subject to the criticisms of what I called the 'new objectivities' in Chapter Five of the book (pp. 111-18).

Some reviewers found the definition of 'objectivity' troublesome, and I believe this concept should be expanded upon. In the book it is said that an inquiry can be objective if and only if:

> (*a*) it is possible for its descriptions and explanations of a subject-matter to reveal the actual nature of that subject-matter . . . and (*b*) it is not possible for two inquirers holding rival theories about some subject-matter and having complete knowledge of each other's theories (including the grounds for holding them) both to be justified in adhering to their theories. [P. 4.]

One reviewer thought that by 'possible' in this definition I meant 'logically possible', where this means 'describable without self-contradiction' (xi: 26). In fact, I meant more than this; since I wanted to meet those who try to show that social-scientific knowledge is impossible by some argument other than induction from (alleged) failures at attaining that knowledge on the part of social scientists, and I do not think that all such arguments reduce to linguistic ones. Other reviewers have found condition (*b*) unnecessarily restrictive (vii and xii), but I think it is essential.

I take it that two theories are rivals if, in attributing some properties or relations to the same subject-matter, they entail contradictory propositions, and I use 'justified' in such a way that for a belief about something to be justified it not be an error, insofar as arriving at and sustaining a true belief about that thing is

the end in view, to hold it. That is, that in the light of available evidence the belief is apt to be true. Now consider a single inquirer entertaining two hypothesized rival theories. Surely the inquirer could not be fully justified in holding both theories to be *true*, since this would mean that it would be in the interest of arriving at and sustaining true beliefs about the subject-matter in question that he or she hold a pair of beliefs of which it is necessarily the case that at least one is false. If the available evidence is such as to justify believing that p, then it surely will not also be such as to justify disbelieving that p. If, then, the objectivist cannot allow a single inquirer to be justified in believing rival theories, how can this be allowed in the case of two or more inquirers (having complete knowledge of each other's theories and the grounds for holding them)? The *number* of inquirers is irrelevant to the question of justification.

Introducing considerations about the *degree* of evidence does not change this situation. If two theories, A and B, are rival theories, then the objectivist cannot allow that one and the same body of evidence could justify theorist 1 in considering A probable (even if just barely) and hence B improbable (even if just barely) and also justify theorist 2 in considering B probable and A improbable any more than one could allow for the same body of evidence to justify a single theorist in simultaneously considering A probable and B probable.

Why is it thought by many would-be objectivists that people can be justified in holding rival theories? I believe the view rests in the first place on a confusion between being justified in 'holding' a theory, that is, regarding it as true (though not necessarily as absolutely so), and being justified in 'entertaining' a theory, or pursuing it with the aim of discovering whether it is true. Where available evidence is so scant that neither of two rival theories is apt to be true in the light of that evidence, it might well be that an inquirer would be justified in continuing to entertain a theory, that is, justified in attempting to find evidence which would justify holding it, even if the inquirer was aware of the other hypothesized theory which someone else, and indeed the first inquirer as well, would also be justified in entertaining in this way. Further, an inquirer might be justified in *holding* one theory and also be justified in simultaneously *entertaining* a rival to it, provided there was some non-negligable empirical possibility of the latter's being true.[4]

Another source of the would-be objectivist's views might be a confusion between the justification of a theory and its truth. It might be thought that unless a theory is true, then either it is not justified to hold it at all, or else it is no more justified to hold it than to hold any rival. This is manifestly not the case. A false hypothesis may be apt to be true, and unless it is known to be false, it would not be an error to hold it insofar as having true beliefs is what is desired.

In the background of both the confusions I impute to would-be objectivists is something else, I believe, namely anti-objectivism. If anti-objectivism were correct, then it would not be possible to ascertain progress in science. If this were the case, then one could not say that holding one theory as opposed to

4 Imagine two rival theories such that at some time each is supported by 'enough evidence' (somehow construed) to justify at least entertaining it, but neither is supported by enough evidence to justify holding it over the other (as some think was once the case regarding the wave and the particle theories of light). Ought the objectivist to allow that existing evidence could ever justify *holding* both of these theories rather than just entertaining them? A circumstance in which something approaching this could be sanctioned consistently with the characterization of objectivity in the book would be one where each theory is held to be true *in part*, while it is hypothesized that the two theories are in some way compatible. What is unknown is exactly *how much* of each theory is true or just *how* the two could be compatible. (Rather, it is supposed that these are matters for continuing inquiry.)

another is more or less in the interests of discovering and sustaining true beliefs about something, since nobody could ever know what the truth is and hence could not learn to distinguish between erring and not erring in the pursuit of true beliefs. Similarly, on the anti-objectivist view, the distinction between entertaining a theory and holding it breaks down. On the one hand, nobody could be justified in holding a theory for the reasons just given. On the other hand, it would never be unjustified to entertain a theory for the anti-objectivist, since it could never be objectively established that available evidence was such as to virtually rule out the empirical possibility of some belief's being true. Whereas the objectivist wishes to say that at some point it can be ascertained of some hypothesized theories that they are so unlikely to be true that it is not even justified to continue entertaining them in the way described earlier. One example is the flat earth theory. Another—to take an example illustrating that justification even in entertaining theories is not a matter of there being consensus—is the theory that some races are genetically disposed to poverty and violence.

One recurrent criticism has been that the book concerns itself just to refute anti-objectivist arguments without discussing, as one review puts it, 'the substance of objectivity' (i: 276). This viewpoint has caused me a certain amount of puzzlement. Most social-scientists, it seems to me, go about their work trying to be objective. Meanwhile, theorists who both express and fuel a widespread popular sentiment argue that the attempt is doomed to failure in principle. My aim was to counter as many of these arguments as I could find. What more substantial or positive task could have been done? Assuming that it is not expected that I should have explained what the (objective) truth about human society as a matter of fact is, I can think of three possibilities.

The most interesting request would be for an historiographic account of the origins and sustaining causes of anti-ojectivist attitudes. Such an account would have to take into consideration, in my view, the *ideological* function of anti-objectivism and *differences* in historical causes and effects of anti-objectivist attitudes from time to time, class to class, and from culture to culture. It would also take up questions of the social-psychology of anti-objectivism and would require disentangling and acknowledging some historically humanistic or progressive dimensions of the attitude as against what I see as its primarily anti-progressive dimension. However, this was not the task of the book, nor is it a task that I felt or feel myself competent to address. Historicist claims to the contrary, I do not think that success in such a task is a necessary condition for resolving the sorts of philosophical issues the book did address. What is more, such a much-needed historical analysis would itself be a social-scientific exercise which should be facilitated by dispelling the false belief that objectivity in such exercises is impossible.

Another thing that could be demanded is the elaboration of an epistemological theory, successful defence of which would show not that this or that argument for anti-objectivism fails, but that all such arguments must in principle fail. Of course, this elaboration and defence would be most welcomed by objectivists, but, again, I do not see myself as the one to produce it. Also, I remain convinced that there is value in surveying and countering specific anti-objectivist arguments. I have been on the lookout for new arguments, not treated in their essence by *Objectivity in Social Science*, so far without success.

A third possibility is that I should have addressed the question of whether 'non-objectivity is a permanent but only a contingent fact' (iii: 365 and perhaps xi: 26-27). Whether objectivity is always possible, but never actually attained is not a question that is appropriately settled, it seems to me, in a philosophical treatise. If objectivity is in principle attainable, and if, as I argue, it is desirable to

pursue social science objectively, then social scientists ought to strive for objectivity, and whether it is in fact attainable will be illustrated in their success. The proof of the pudding is in the eating.

Nonetheless, perhaps I could make some observations about how objective decisions can in fact be made in evaluating rival social-scientific views. The difficult problems do not enter at what might roughly be called the 'observational' level. Though they may involve some difficult technical problems in statistical analysis and testing, and, of course, raise certain questions about the justification of inductive projections, the question of whether people in some society usually marry within their income groups and similar lower level questions seem capable of objective solution. However, many philosophers and some social scientists are not prepared to admit this in the case of evaluating general theories.

I believe, however, that general social theories can be objectively evaluated, though with more difficulty. The view that they cannot seems to me to derive in part from attempting to apply to the evaluation of theories the same criteria that are used in evaluating observational hypothesses. There are, however, some significant differences. In the first place, an observational hypothesis can, arguably, sometimes be at least falsified by a single observation or experiment. I believe that this is never the case with general theories. A sufficiently sophisticated theory in natural or social science will have within it the conceptual machinery to explain any one apparent false prediction made on its basis, for instance, in such a way as not to falsify the theory. Testing in the case of theories is *cumulative*. As the theory makes more and more false predictions and more and more *ad hoc* adjustments must be made, a point is reached where it is no longer justified to consider the theory in the running as a candidate for revealing the actual nature of things.

Where exactly is that point? Perhaps a general answer to this question—one that lays down some rule in advance of inquiry and valid for theories about any subject-matter—cannot be given. Rather, such rules may always be subject-matter-specific and evolve in the process of inquiry. But this does not mean that therefore any decision that a theory has been finally disconfirmed is arbitrary. In this respect the falsification of theories is like the verification of observational hypotheses. The fact that so far nobody has been able to lay down a rule in advance of inquiry telling a scientist that at a certain number of confirmatory observations, for example, a hypothesis is confirmed, does not mean that in practice inductive confirmation has been arbitrary.

Another consideration which bears on the evaluation of theories is *scope*, a consideration which, along with simplicity, I do not think is 'pragmatic' (where this means non-objective), as one reviewer suggested (vii: 485-86), since I think empirical, if not also *a priori*, arguments can be produced to show that superiority in a theory's scope (or simplicity) is, other things being equal, an index of its being more likely to be true. It is relevant in evaluating two social theories to take account of the scope of their relative applications. And it is relevant to see what *other* theories they entail. Related to this is that a theory can be evaluated by looking at the kinds of assumptions it makes regarding background conditions.

To illustrate what I mean, perhaps I could take the example of political-scientific Pluralism, or power-political theories of the sort advanced by Robert Dahl, David Truman, et al. Pluralist theory predicts that check and balance systems of government will be sufficient, barring special circumstances, to prevent totalitarian or 'permanent power imbalance' situations from developing. When governments incorporating such systems, such as the Philippines and South Korea (both countries whose constitutions were written with Pluralist

advice after the war, incidently) became fascist, this might be plausibly explained by appeal to special circumstances. But the Pluralist theory begins to wear thin in this regard as other check and balance systems of government fail to check totalitarianism, such as in Brazil and Chile. Also, if the evidence brought forth by Domhoff and others about the United States of America is correct then a permanent power imbalance seems to be possible *concurrently* with a check and balance system of government.

In the case of the scope of empirical application, it has been charged that in limiting itself to governmental decision making and the power politics immediately preceding it, Pluralists leave unexplained the more important questions as to just why groups have the power they do and why the range of issues over which governmental power politicking takes place is as it is. Again, Pluralism seems to entail a general theory of human nature, namely that human co-operation is a function of enlightened selfishness, which theory, it is argued, is anthropologically and psychologically incorrect. Similarly, Pluralists seem to assume as a background condition in formulating laws and making longer range predictions that people's interests do not typically change in the course of conflict, and this view has also been challenged. Now these criticisms of Pluralism, if sound (and though it does not matter for the present discussion I think they are[5]), would count heavily against the truth of Pluralism considered as a general theory of political society, though they are not the sorts of criticisms that can be made of an observational hypothesis. (Or at least if they can be made this is only because some such hypothesis can be shown to somehow involve a general theory.)

What about *verifying* a theory? This is a more difficult and complicated matter especially in the social sciences. I disagree with the Popperian view that in principle it is possible to falsify but not to verify. For one thing some theories in the natural sciences, such as Newtonian mechanical theory—once qualified as applying in a certain domain, and allowing for progress in making more precise the exact boundaries of that domain—have surely been verified. For another thing, I think it can be shown that if Popper's specific arguments in favour of the impossibility of verification were sound, then it would be equally impossible to falsify.

Nonetheless, at least two factors seem to me to combine to make verification most difficult in the social sciences. One of these is that debates in the social sciences are closely tied up with political ideology. Thus, were I to turn now to a general defence of Marxism, which, to the apparent bewilderment of several reviewers (ix, xii, xv, xvii), I believe *is* by and large verified,[6] the stance of readers, both pro- and anti-Marxist, would straight away take a different turn than one which remains in the comfortable realm of philosophical and methodological abstraction. People's political feathers would be up, and it would be more difficult to control for bias. Ideological overtones are obvious in the case of Marxism, but also enter, sometimes in less obvious ways, in discussing any social-scientific theory, since views about whether or how things social ought to be changed are usually called to mind in evaluating theories about why society is presently constituted as it is.

5 I have defended this conclusion in 'Pluralism and Class Struggle', *Science and Society*, **39**, 1975-76, 385-416.

6 I have advanced some views in an elementary way about the relation of objectivity to Marxism in 'Marxism, Social Science, and Objectivity', in William Shea (ed.), *Basic Issues in the Philosophy of Science*, New York 1976, pp. 127-35, and in my *Understanding Marxism*, Toronto 1977, Chap. eight.

A second complicating factor in the social sciences (though not entirely restricted to them) is the 'hodge-podge' nature of social-scientific theorizing. I mean by this more than one thing. First, there are a multitude of social-scientific middle range theories that may not be in conflict, unbeknownst to their adherents, but address different aspects of social reality. The situation I have in mind is something like the six blind wizards in the ancient Hindu tale each of whom touched a different part of an elephant and formed a judgment failing to recognize that he knew only part of a single animal. Social theorists do not like to view their theories as limited in application and are sometimes overly hasty in seeing theories other than their own as incompatible rivals. Such is the case, I believe, with utility theory and classical Ricardian theory in economics or with certain versions of holistically and individualistically oriented theories in sociology and history.

Another way that social-scientific theorizing is a hodge-podge is that it is not always clear what level of abstraction theories are moving at. Let me give two examples. Behaviourists like to propose their theories as narrowly empirical, and they like to restrict themselves as much as possible to discovering and recording observational laws. Yet I think it can be shown that this is an illusion and that behaviourist explanations are, at least in the interesting cases for social science, shot through with general theoretical speculation.[7] If such general speculation is not brought to light then both strengths and weaknesses of behaviouristic accounts are often overlooked in evaluating them.

A second example can be seen in the history of Marxist theorizing. Here something like the opposite misconception has prevailed on the part of many Marxists. Regarding Marxism as a very general socio-historical theory, they have concluded that it somehow includes within it all in the way of theory that is necessary to generate explanations of what goes on in society. Hence, Marxists rejected middle range theories of social psychology, political science, and sociology, and sometimes even the disciplines themselves as bourgeois. In fact, as Marxists concerned about phenomena like class consciousness, the specific mechanisms of political structures and behaviour, and the dynamics of small groups like the family are discovering, there are rather large holes in these areas of Marxist speculation which can only be plugged by special theoretical work at their own, relatively speaking, micro-levels.

Add to both these factors the hodge-podge created by what seems to me a near mania on the part of many social theorists for almost self-indulgent, abstract methodological speculation and debate to the total or near exclusion of attempts at empirical application. This has created more heat than light and still has not answered such questions as what a 'structure' is, whether functionalism is a theory, a method, an approach or something else, whether an ideal type is a truncated theory, a heuristic device, a model (whatever a model is thought to be), and so on. Of course, nobody whose stock in trade is the philosophy of the social sciences could deny that it is important to pose and answer such questions. But it seems to me that in more than one social-scientific discipline confusion has sometimes resulted from failing to distinguish between this sort of work and the formulation and empirical testing of substantive social theories.

My conclusion from considering the hodge-podge nature of social-scientific theorizing is not that therefore it is impossible to verify theories in social science, but that, among several other things, a job of conceptual disentanglement is needed—a job of sorting out putative claims of theorists and actual achieve-

7 This point is elaborated on in 'Inductivism and the Libertarian-Ideographic Tradition', op. cit.

ments, of locating an hypothesized theory's focus, of sorting genuine and merely apparent conflicts among theories, of distinguishing methodological and substantive claims, and so on. Doing this job is hard work, as, of course, is the job of actually building and testing theories. But, as any practicing social scientist can tell us, striving for objectivity in social science is in general very hard work indeed.

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