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In the Tradition*

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I

This volume is a potpourri of papers from the Boston Colloquium for the Philosophy of Science. First presented between 1973 and 1980, the essays cover a wide range of topics, including the question of what makes an historical reconstruction acceptable, the nature of social reciprocity, the shortcomings of atomistic empiricism, the problem of the diversity of sociological theorizing, Jewish responses to Copernicus' work and the general character of technology. Moreover, like many collections of this sort, the articles are uneven in quality: while some are well thought out, others appear to have been thrown together for the occasion.

In their introduction, Robert Cohen and Marx Wartofsky present the collection as a response to the collapse of positivist philosophy of science. We should, they suggest, see the papers as addressing questions prompted by the shift that occurred in the nineteen sixties when philosophers turned from comparing the social sciences with the natural sciences to examining them in terms of 'social-scientific categories'. Perhaps, but nobody should expect to find a detailed treatment of the epistemology and methodology of the social sciences in this volume. Few of the essays make more than superficial reference to the social sciences, let alone attempt to clarify their epistemology and methodology. Better, then, that we consider each essay on its own terms and set aside the question of their contribution to 'the ongoing renewal of the philosophy of all the sciences' (p. viii).

II

The first paper in the volume, Paul Diesing's 'Ideology and Objectivity', concerns the way in which ideology influenced a number of political scientists engaged in research on bargaining in international crises. In the project that Diesing describes, ideology entered 'at every step' in the investigators' deliberations (p. 2). While none of the investigators consciously ignored or falsified the historical evidence, they nevertheless urged accounts that reflected their differing ideological biases. What happened, according to Diesing, was that they used a 'double standard' and accepted facts that fitted their narratives while rejecting those that did not (p. 3). Diesing's conclusion: 'there is no royal road to objective truth' and we should instead make do with a plurality of theories (p. 16).

One difficulty here is that Diesing and his colleagues accept without question that all political views deserve equal attention. If Diesing is right, academic political science is less of a science than is often supposed. But this hardly establishes that objectivity is an unattainable ideal. What Diesing's argument

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shows is not that objectivity should be jettisoned, but only that we should be as sceptical about the views of political scientists as we are about those of politicians. Which of their views, we need to ask, are more ideological? Which of their political prejudices are worth taking seriously?

To be fair to Diesing, he is quite willing to castigate some accounts as being 'ludicrously one-sided' and he is not at all adverse to judging some as being 'more balanced and objective' than others (pp. 13, 14). But if it is possible to make such discriminations, why think that ideology must always thwart objectivity? Given the twin methods of empirical testing (p. 6) and ideology analysis (p. 16), we should be able to reduce, if not entirely eliminate, the ideological accretions that Diesing isolates. Of course, this is not to say that political scientists should never disagree. We must recognize, as we do in the case of the natural sciences, that the issues can be exceedingly complex and that reasonable people can come to quite different conclusions. (Moreover, how clear is it that 'a recognition of one's own biases . . . in the short run . . . certainly leads to modesty' [p. 16]? Might it not also lead to better ways of justifying the boldest of theories?)

III

In 'Towards a Logic of Historical Constitution', Leon Goldstein restates his view that historians 'constitute' the past rather than describe or reconstruct it, and he argues that historical constructions are acceptable just in the event that they explain the available evidence. For Goldstein, it is a mistake to suppose, as do many philosophers of history, that the relation between present evidence and the 'constituted past' is deductive or inductive. We should, he says, see it as being 'abductive', 'there [being] no direct way to bridge a starting point and a conclusion' (p. 36). Goldstein recognizes that it is 'extremely difficult to determine just how it might be argued that the logical function of the constituted event is to explain the evidence', yet he nonetheless declares himself as continuing 'to believe that that is precisely what its logical function is' (p. 46).

Perhaps the most striking fact about Goldstein's argument is the degree to which it is shaped by traditional philosophical assumptions. He focuses primarily on the 'logic' of historical construction and assumes that the historical, social and valuational components of particular constructions are philosophically unimportant. More crucially, Goldstein's final position differs little from the view urged by logical empiricists like Carl Hempel. For Goldstein, as for Hempel, the relation between historical construction and evidence is one of explanation, and he too seems to be committed to the hypothetico-deductive model. True, Goldstein criticizes the deductive model of explanation, but his argument is inconclusive. It may well be that the historical past cannot be constituted by logical inference from the historical evidence, but this hardly establishes that it cannot be explained by subsuming it under appropriate covering laws. (See also p. 46.)

But despite these reservations, Goldstein's essay remains one of the most interesting in the volume. His criticisms of standard positions are helpful, and his description of how history is written in the light of common sense and scientific theory is especially instructive. Goldstein is right to remind us that historians constitute the past by making 'a motley of odds and ends . . . fit together so that a reasonably coherent picture emerges' (p. 33), and to insist that historical accounts are deeply affected by their authors' 'intellectual commitments as to what is socially and humanly possible' (p. 42). Indeed, his descriptions of historical practice (especially his brief account of H. R. Holland's explanation of the origin

of the so-called Kensington stone) are so well crafted that one is left wondering what a general philosophical theory or explication of the concept of historical plausibility could add or why it is needed.

IV

Carol Gould's argument in 'Beyond Causality in the Social Sciences' is that 'the category of causality . . . is inapplicable to the relation of subject to subject' and should be replaced by the 'entirely different category . . . of reciprocity' (p. 53). In the first part of the essay, she criticizes both the causalist's and the anti-causalist's accounts of action, the former because individuals are agents, actions have meaning and the act of offering a causal explanation of actions itself presupposes intentionality, and the latter because agents can and do transform objects causally in accordance with their purposes. Then, in the second and longer part of the essay, she sets about clarifying the notion of reciprocity and argues with considerable ingenuity that it plays a central role in social interaction.

Gould's account of reciprocal relations as being 'constituted by the understanding and actions of the agents' is a considerable improvement on Kant's and Hegel's rather obscure treatments of them, which she takes as her starting point (p. 67). Nonetheless, it is difficult to picture clearly what she has in mind when she speaks of agents acting on the basis of 'shared understanding' and 'free agreement' so that their respective actions are 'equivalent'. (Her 'more concrete interpretation', which is cast in terms of a general discussion of Hegel's account of the 'master-slave dialectic', only further complicates an already complicated matter.) More importantly, it is difficult to see how reciprocity as Gould defines it can function, as she contends, both as a norm and an explanatory principle. It may well be that full reciprocity—which involves both the recognition of others as free individuals and the active concern with the agency of others—establishes a normative model for the development of new forms of social relations (p. 79). But when explaining action, what counts is rather the nature of the reciprocity actually in place. It is one thing to say that full reciprocity functions as a standard against which non-reciprocal or less than fully reciprocal relationships can be criticized, quite another to hold that it has the special feature of being both explanatory and normative.

These points aside, there remains the question of whether Gould has in fact managed to forge an alternative to standard causal theories of action. Had Gould argued that causal categories are always inappropriate for explaining the behaviour of agents, her position would have been clear and familiar. But since she is willing to grant the propriety of causal accounts of the transformation of objects by agents in accordance with their desires and interests, she can hardly reject causal accounts of interactions between agents on the grounds that they preclude intentionality. In other words, if 'subject to object relations' can be causal, why not 'subject to subject' relations as well (provided the 'objects' involved are recognized as persons rather than as things)? (On this view, reciprocity becomes an 'external relation', but this is less of a problem than it may seem, since Gould herself treats the internality of reciprocity as being simply a matter of agents freely arriving at understanding and agreement [p. 67].)

V

Marjorie Grene accurately describes her contribution, 'Empiricism and the Philosophy of Science or *n* Dogmas of Empiricism', as 'rather a jeremiad than an argument' (p. 89). In her view, the psychological atomism of the British

empiricists was 'misguided' and 'bizarre', and 'the notion of perception as judgmental has become so commonplace that we fail to notice the absurdities into which it leads us' (p. 95). We should, she argues, reject empiricism in favour of 'comprehensive realism', according to which 'being-in [is] a fundamental category' (p. 94) and 'the visible world is constituted by a complicated network of organized objects and events, carried at the same time by the observer's bodily sense of being the one who sees' (p. 99).

Much of what Grene has to say about these matters is sound and important. We do indeed 'start, and finish, as real beings in a real natural and historical world, . . . a world that in our case includes scientific institutions, scientific beliefs, scientific languages, scientific methodologies' (p. 94). And there can be little doubt that important presuppositions of standard philosophy of mind are, as she points out, undermined by J. J. Gibson's 'perceptual psychology' (cf. p. 98). But these observations are marred by the tone of the essay. Locke, Berkeley and Hume may have been wrong but they were not fools. One can only wonder what prompted a self-proclaimed historicist like Grene to treat her long dead opponents with so little sympathy.

VI

'Realism and the Supposed Poverty of Sociological Theories' by Ian Jarvie may be read as an answer to Diesing's paper on ideology and objectivity. Whereas Diesing laments disagreement in political science, Jarvie regards the diversity of contemporary social theory as 'healthy, fruitful and exciting' (p. 107). Indeed, Jarvie could have been responding directly to Diesing when he cautions against our having unreasonably high expectations of the social sciences. We should not, he argues, forget their successes in our zeal to catalogue their failures. In particular, he points out that we need to keep in mind that 'the successful solution to a problem always leads to the appearance of new problems and a revision of the schedule of urgency with regard to still unsolved ones' (p. 116).

Jarvie identifies and rejects three common responses to theoretical failure in the social sciences. We should neither stick to our theories dogmatically, nor engage in developing new ones of ever increasing banality (disguised in high-sounding language), nor conclude that there is no use trying to find a correct theory since nothing will work. (Interestingly, Jarvie takes ethnomethodology to be a form of scepticism [p. 117] as well as one of the theories that contributes to the healthy diversity of contemporary sociological theory [p. 107].) What we ought to do instead is embrace 'critical rationalism' and recognize 'refutation as a success, as a clear growth in our knowledge' (p. 116). On this Popperian account, disagreement is a boon rather than a drawback to progress: it enhances criticism and increases the possibility of our learning from our mistakes.

The main difficulty for such an approach is one that always besets negative methodology, namely, that it leaves success in the full sense of the word unaccounted for. We do not learn by getting things wrong but rather by learning something positive in the course of making mistakes. For sceptics worried about the point of it all, Jarvie's proposal that we regard failure (or refutation) as success will only confirm their worst fears. From their standpoint, the problem is that social science has been all too successful in achieving falsifications. To be sure, this would not matter were the various social theories now in play alternative perspectives rather than competitors. But as long as they remain uncoordinated, we have no reason to see them as anything other than a babble of competing voices. Jarvie is right to insist that the social sciences have accomplished a great deal, but we should nonetheless be worried by its all too frequent failures.

VII

The central argument of Spiro Latsis' 'The Role and Status of the Rationality Principle in the Social Sciences' is that Karl Popper's view about explaining behaviour can be improved by combining it with his theory about the connection between mental states and behaviour. Latsis' contention is that we can clarify the so-called rationality principle, according to which 'agents behave adequately or appropriately to the situation' (p. 123), by making use of the idea that 'decision behaviour is plastically controlled by mental states—among other things' (p. 139). In Latsis' view, when we proceed this way, we can readily understand why Popper has at different times taken the rationality principle to be 'almost empty', 'not a priori valid', 'clearly false', 'a good approximation to the truth' and 'the consequence of a methodological postulate' (p. 133).

What we need to remember, says Latsis, is that Popper denies that behaviour must be 'either predictable and determined or random and unpredictable' (p. 137). Were we committed to one or other of these possibilities, we would have to view the rationality principle as an empirical generalization or a translation principle, and hence regard it in one of the many confusing ways that Popper mentions. However, once we recall that Popper holds that behaviour is under 'plastic control' (i.e., he takes it to be 'soap-bubble-like' rather than 'clock-like' or 'cloud-like'), the rationality principle appears in a new light. For we can then appeal to Popper's claim that minds and bodies are causally unrelated to show that the principle is not an empirical generalization, and we can invoke his suggestion about plastic control to show that it does more than spell out a 'conceptual connection'.

Whether many philosophers will find this account of the 'role and status' of the rationality principle helpful is, however, another question. I suspect that even strong proponents of the principle will be troubled by Latsis' introduction of Popper's rather idiosyncratic views about the mind-body problem into the discussion. At the very least, they will want to know more about what plastic control involves, especially since—as Latsis admits—Popper himself is unclear about it (p. 139). Certainly, little is gained by observing that 'the role of the rationality principle is to function as a 'plastic interface' between mental states and behaviour', and it is more confusing than helpful to insist that the principle 'has no specifiable content just as a thin layer of molecules that constitutes the surface of a soap bubble has no specifiable shape' (p. 140). As far as I can see, nothing is achieved by coupling the rationality principle with vague remarks about the behaviour of clocks, clouds and soap bubbles.

VIII

Werner Leinfellner's 'Marxian Paradigms versus Microeconomic Structures' is a long and somewhat rambling discussion of Marx's views about alienation and their bearing on Marxist economic theory. Leinfellner proceeds by defining the notion of a 'microeconomic fundamental structure' and by specifying what he calls the 'agnate sets' (which specify the background knowledge) for capitalist and Marxist microeconomics. His claim is that the Marxist agnate set establishes a particular interpretation of the fundamental structure of microeconomics, so that 'Marx could have taken all his knowledge of microeconomics from [the] underlying invariant structure [of microeconomics, and simply reinterpreted it] in a different sense under the influence of the alienation paradigm' (p. 195). Finally, Leinfellner draws from his analysis 'a typical Marxian dilemma: If we have inequality of labor values for work done in the same time, then sooner or later alienation will set in. But if we have an ideal, ethical equivalence between

labor values and values-in-use, [then] we may throw overboard our whole present microeconomic system' (p. 197). Our best bet, he says, is to modify the agnate set 'in the direction of a welfare economics' while retaining 'the invariantly given structure of microeconomics' (p. 198).

Here I intend to leave the question of the accuracy of Leinfellner's characterization of Marx's views to the experts. Moreover, I shall not attempt to clarify his account of the 'Marxian dilemma', nor even hazard a guess at whether changing the 'agnate sets which represent the cultural and hermeneutic heritage of economics' does the trick. I shall simply lodge a complaint against Leinfellner's use of set-theoretical ideas to ornament his argument. The apparent precision of his definitions seems to me to be almost entirely spurious. A relatively simple example is his discussion of dialectical processes (pp. 169-72). We are told that complementation, C , represents the function of effective negation and set-theoretic union of two sets represents the function of the traditional 'concilare' of the dialectic. So if S is the thesis and CS the antithesis (with $S \cap CS \neq 0$), the synthesis is $S \cup CS$. But how helpful is this? Of course, there are many functions, C , such that $CCS \neq S$, but this adds nothing to our understanding of negation and synthesis, which is after all what the fuss is about.

IX

As the dust jacket notes, the last three essays deal with 'other topics'. In 'Paradise not Surrendered', Hillel Levine outlines some of the different responses that Jewish scholars made to Copernicus' theory during the sixteenth, seventeenth and eighteenth centuries. In this essay, which is one of the best in the book, Levine shows that Judaism was marshalled in defence of a wide variety of different views. Unsurprisingly perhaps, some Jewish scientists felt called upon to challenge Copernicus to protect their religion, while others maintained that the new science was consistent with their religious views and could even provide them with additional support. The essay concludes with a brief but fascinating comparison of the development of science in Jewish Europe with its development in Catholic Europe, Protestant Europe and Confucian China.

In 'The Peculiar Evolutionary Strategy of Man', Lawrence Slobodkin argues for 'the hypothesis of an innate human propensity to develop a normative introspective self-image' (p. 228). His view is that the capacity for having a self-image is a biological property produced by natural selection while its content is culturally determined. Again, I leave the evaluation of the technical argument to the experts. For my part, I found it disconcerting to be told that the author will 'be discussing areas in which [his] expertise is minimal' (p. 227). And I was especially puzzled by his contention that the denial of 'Man is naturally aggressive' is neither 'Man is naturally nonaggressive' nor 'Man is not naturally aggressive' but rather 'the statement, "Man is naturally aggressive"', is a non-sense statement which should not be taken as scientifically valid or even meaningful' (p. 244).

Langdon Winner's aim in 'Technologies as Forms of Life' is to reorient how we think about technology. In his view, the problem with the conventional view of technology is that it 'disregards the many ways in which technologies provide structure for human activity' (p. 251), and we should recognize that 'as they become woven into the texture of everyday life, the devices, techniques and systems that we adopt become part of our very humanity' (p. 255). But if this is to provide a new basis for the philosophy of technology, much more needs to be said. Certainly, little is accomplished by observing that technologies function

like 'forms of life'. If Winner's view is simply that 'telephony, automobility, and electric lighting are forms of life in the sense that life would scarcely be thinkable without them' (p. 254), the prospects for a distinctive philosophy of technology would seem to be as dim as ever.

X

Reading these essays, one is struck less by the way they go beyond the positivist (or rather logical-empiricist) point of view than by the extent to which they remain within its general orbit. Goldstein's aim is to develop a 'logic' of historical construction; Gould aspires to a general model of social explanation; Latsis asks whether the rationality principle is empirical or conceptual; Leinfellner is mainly concerned to provide (set-theoretical) explications of different micro-economical theories; and Winner's object is to develop a general characterization of technology. Although the language has changed—the contributors to this volume are more prone to talk of models and theories—the arguments are largely molded by positivist presuppositions and aims.

Part of the problem may be that the move away from positivism is all too often thought to require a movement towards phenomenology, Marxism, hermeneutics, action theory or something similar. (See in particular Cohen and Wartofsky's introductory remarks, p. vii.) However, what recent developments in philosophy seem to show is that it is all the standard traditional views, not just positivism, that must be replaced. As is becoming increasingly clear, we do not require new solutions to old problems but rather a radical critique of the problems themselves. In particular, we need to ask whether a general theory of the social sciences is possible, why one is needed and whether general philosophical accounts of society provide us with anything that cannot be provided in a piecemeal fashion by careful historical and sociological analysis. Philosophy of science that genuinely goes beyond positivism should not reproduce positivist dogma, still less reinstate repositivist superstition.