

## SCIENTIFIC SOCIALISM: A POSITIVIST DELUSION?\*

Russell Keat\*

In *Radical Philosophy* 21, Roy Edgley replied to some criticisms I had made of his article in *Radical Philosophy* 15, in which he had argued for a socialist conception of social science as inherently critical of real contradictions in society. I don't find his comments at all convincing, and I will try to say why. At the end, I will briefly indicate what, for me, are some of the more general issues involved in this exchange.

Edgley deals with three objections I made to his original article. First, showing that social science can be critically opposed to real social contradictions doesn't amount to showing that it's socialist. Second, acceptance of a Weberian fact-value distinction doesn't commit socialists to reformist, ethical, or Utopian socialism. Third, Edgley's conception of scientific, theoretical knowledge is provided with no criteria of validity, in performing its critical, practical function, beyond what he terms the 'Enlightenment' standards of correct description, explanation, and prediction. I will concentrate on his responses to the second and third of these, with a short comment on the first later on.

### Relativism and Reformism

Edgley tries two separate, mutually reinforcing, ways of establishing a link between the fact-value distinction and reformist politics. The first goes like this. Acceptance of this distinction involves treating moral values as emotive, subjective, and relativistic; and since, on this view, "nobody's moral position is objectively truer than anybody else's", it is natural to adopt "the human rights principles of freedom, tolerance, dialogue, moderation, and compromise". But to do this makes it impossible to engage in any kind of effective, non-reformist class politics.

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\* [2013] Published in *Radical Philosophy*, 23, 1979, pp. 21-23; citations should be to this. The main text has been left unchanged, except for typographical corrections and bibliographical information. An Appendix has been added, providing references to other publications related to the debate to which this article contributed. My apologies for the small font size, which could not easily be changed from that used in the original publication.

\* [2013] School of Social and Political Science, University of Edinburgh: russell.keat@ed.ac.uk. Previously, Department of Philosophy, Lancaster University.

There are two main errors here. First, though tolerance is one possible outcome of a subjectivist view of moral values, it is by no means the only possible one. It is perfectly consistent to regard such values as rationally arbitrary whilst setting little value on the merits of tolerance and compromise in realizing these 'non-rational' commitments. (If actual examples, as well as 'mere' logical possibilities, are demanded, I suggest Nietzsche, for a start).

The second error is to assume that the fact-value distinction involves a subjectivist view of moral values. Of course, this depends on precisely what sense is given to the expression 'the fact-value distinction'; but, at least in his original article, it seems that Edgley regards the key feature as the claim that value-judgments cannot be derived from scientifically established results – thus, e.g., his quotation from Poincaré (*RP* 15, p.3), as a representative advocate of the position, consists mainly in this claim. But this position neither presupposes, nor entails, that value-judgments are non-objective. This further claim would only follow if we make another, quite independent, assumption: that scientific knowledge is the only legitimate, or objective, or genuine form of knowledge.

Such an assumption - which can be called, for convenience, 'epistemological positivism' - has often been associated with the fact-value distinction, and can clearly provide support for it. But the non-derivability claim can perfectly well be maintained without accepting epistemological positivism. For instance, in Habermas's theory of knowledge-constitutive interests, there are said to be three different forms of knowledge, each with its own, distinctive, criteria of validity. One of these forms, the 'empirical-analytic', corresponds roughly to standard conceptions of scientific knowledge; whilst the third, critical-emancipatory, includes within it the validation of normative judgments. This overall doctrine constitutes an explicit and systematic challenge to epistemological positivism; but it does not involve the claim that value-judgments can be derived from empirical-analytic statements. Thus the fact-value distinction is logically compatible with an objectivist or rationalist conception of values. (Another, rather different example of the consistent adoption of both positions would presumably be Kant).

### Value-Neutrality and Reformism

Edgley's second way of establishing the link between this distinction and reformism (which, he says, was suggested to him in correspondence by Martin Barker) is this. "The fact-value distinction yields a practical distinction between means and end", and the latter distinction is naturally operated so that *production* - conceived as technology, and thus as the realization of "the power of a science that is independent of and impervious to any kind of morality or politics" - is regarded solely as a *means*. It thereby escapes from the sphere of moral or political values, which concern only *ends*. In this way, morality and politics are restricted, in the manner typical of reformism, to matters of *distribution*.

I have three objections to this line of argument.

The first is that, even if it were true that the fact-value distinction yields a distinction between means and ends, according to which moral and political judgments can be applied directly only to ends and not to means, it would not follow that production was thereby 'sheltered' from moral or political judgment. Quite the contrary. If a negative judgment is made of distribution (e.g. in terms of its inequality), and it is then shown that a specific system of production is the means through which this pattern of distribution is generated, then, other things being equal, the negative judgment would be transferred to the system of production.

Second, it seems doubtful that the fact-value distinction *does* yield this form of distinction between means and ends. For to show that some course of action or practice is a means to an end that is positively valued does not remove that means from potentially negative evaluation by reference to some value or values other than those specifying the end. Thus Weber - the usual bogeyman for Marxist critics of value-free social science - argued strenuously against those who, in the name of economic efficiency or progress, mystified people into believing that, once the ends were accepted, it was possible to show by scientific argument alone what means should best be adopted. The fact-value distinction cannot be invoked to protect 'mere means' from moral or political judgment.

Third, there is no reason why the fact-value distinc-

tion should be taken to protect science/technology *in particular*, from such judgments. For there is no contradiction in *separating* the criteria for the validity of scientific knowledge from those for moral or political values, whilst at the same time making value-judgments *about* scientific knowledge, e.g. of its role in forms of economic production, the character of the social practices involved in its generation, and so on. To invoke the example of Weber once more: on Edgley's apparent view of the implications of a value-free science position such as Weber's, one of the main features of Weber's work would become highly paradoxical, viz. his concern with the consequences for 'the human spirit' of the process of 'rationalization', a central element of which was the growing influence of science and the scientific attitude in the organization of society.

Weber saw no inconsistency in denying that value-judgments could be made scientifically, yet making such judgments about the practice of science and technology, and I can't see that he was mistaken in this. (Curiously, in the passage Edgley quotes from Poincaré in his original article, a similar mistake seems to be made: Poincaré apparently regards the non-derivability claim as establishing *both* that "it is not possible to have a scientific ethic" *and* that "it is no more possible to have an immoral science" (RP 15, p.3)).

### Knowledge as Practice and as Theory

I turn now to Edgley's response to my third objection to his original article, concerning the criteria for the theoretical knowledge achieved by a critical social science. He says that in stating this objection, I assume a distinction that his whole article criticized, "between 'theoretical knowledge' and 'the critical practical function of scientific knowledge'". Against such a distinction he insists upon a unity of the two, which "is expressed in the central category of contradiction, since this category is both critical and explanatory...": we can and should conceive of "explanation in the evaluative mode of criticism..." (RP 21, p.31). I must say I find it extremely difficult to understand what Edgley is getting at here; in fact, I don't think I do. But I get the impression he didn't understand what I was saying, so I'd like to try again.

I accept Edgley's view that, in the social sciences, there's a legitimate sense (absent in the natural

sciences) in which there can be 'contradictions in reality'; and that a social theory, in identifying these: is at least implicitly critical of them. My point was, and is, that the criteria by which these relations are judged to be contradictory and thus criticized, derive exclusively from the criteria governing the truth or falsity of the statements constituting the theory: the *critical* function of theoretical knowledge involves making judgments about social reality by reference to what has been established by the theory, i.e. its contradictory character. For instance, in his original article Edgley said that:

“In this way social science, in criticizing other, ideological social theories and ideas as deeply contradictory, and so contradicting them, at the same time criticizes as contradictory, and so contradicts, the society in whose structure those inconsistent and muddled theories and ideas are realized. Marx's critique of what he calls 'the system of bourgeois economy' attacks *at one and the same time* both theories and concepts of political economy and capitalism itself (RP 15, p.7, my emphasis).

Now I can more or less accept the 'unity' of theoretical knowledge and practical criticism that is indicated in the phrase I have italicized. But this seems to me a unity which gives priority to theoretical knowledge in the sense that the legitimacy of the criticism is dependent on the legitimacy of the theory that has enabled us, in the example quoted, to detect these inconsistencies and muddles in the theory and practice of 'the system of bourgeois economy'.

For me, this point is crucial because it indicates a serious limitation on the *scope* of the 'critique' of capitalism that is possible from the standpoint of Edgley's conception of scientific socialism.

I do not believe that the concept of 'contradictions' can encompass the range of criticisms that have traditionally (and correctly) been made by socialist opponents of capitalism, unless this concept is illicitly extended to include features whose 'contradictory' character could *not* be established by reference to correct theoretical knowledge, given that no attempt has been made to provide any criteria of validity for that knowledge, other than those of 'the

Enlightenment'. As examples of such criticisms I suggest: the alienated character of work; the competitive and individualistic nature of social relationships; the division between mental and manual labour; the absence of genuine democratic forms of control, and so on.

### Criticism and Contradiction

This brings us back to my first objection, that showing social science to be critically opposed to real social contradictions doesn't amount to showing that it's socialist. Edgley's reply is that, in effect, it *does*, since “a science that takes objective social contradictions as its target *must* be socialist...”. It must be socialist, because, being opposed to the contradictory character of the structure of capitalist society, and requiring its transformation, it must “take up the class position of the proletariat as the only class capable of understanding and eliminating those contradictions”. (RP 21, p.29).

I can't say much of any use about this, since a lot depends on just what *are* the contradictions of capitalism that Edgley thinks can be scientifically established. But I am sceptical about the way that, in this response, he *apparently* takes belief in the revolutionary potential of the proletariat as either *the* or *a* defining characteristic of socialist critique. I think this removes the main emphasis, in articulating a socialist standpoint, from where it should be: namely, in the distinctive nature both of what it takes to be objectionable, oppressive, etc. about capitalism, and of its conception of an alternative form of society that is historically realizable.

### Science and Socialism

I'll conclude by mentioning what, for me, are the bigger issues that make it worth engaging in what may seem a rather nit-picking exchange. What I say will, I'm afraid, be very sketchy, and consist mainly in assertions rather than arguments; but it may help to explain the standpoint from which the earlier arguments were constructed.

I take the central question to be: can a *socialist* critique be founded exclusively on a *science* of society? I read Edgley's articles as claiming that it can, and I think both that this is mistaken, and that it impoverishes the character of socialist theory and practice. To this extent, I am sympathetic to some of

the sentiments expressed in one of E. P. Thompson's recent forays, 'The Poverty of Theory' (in *The Poverty of Theory and Other Essays*, London: Merlin Press 1978, 193-399) in which he attacks the scientific anti-moralism and anti-humanism of most 'Marxisms'. Thus:

"I can only suppose, from certain references of theoretical practitioners to 'moralism', that these imagine amoral choice, or a choice between values, to be a kind of grunt, and a grunt which is the reflex of 'ideology'; and that they suppose that one grunt is as good as any other, and have never noticed that it may take the form of a discipline with its own arduous and rigorous 'discourse of the proof'... And in so far as the full disclosure of choices between values is inhibited, in so far as the articulate 'discourse of the proof' is actively suppressed, so any value informed view of life will rot away into rhetoric and hypocritical moralistic oratory." (p. 368)

And Thompson claims that it is precisely the *suppression* of this 'discourse' of values - a discourse which, he insists, is neither scientific nor irrational (p.367) - that has led to the distorted form which moralistic critique has so often taken when it *has* emerged. For instance:

"... one form of the protest against Stalinist ideology and forms has very often been 'moralistic', but, since it has been denied every opportunity for open articulation, it often appears as a kind of displaced, illusory, and, of necessity, 'utopian' moralism - as a reversion to Greek Orthodox faith, as nationalist self-exclusion, as personalist self-isolation, or as Solzhenitsyn - as the agonized heartbeat within a heartless world. (p.369)

Now Edgley might well object to my quoting these passages as if in opposition to his position, on the grounds that he certainly does not view all value-judgments as 'unscientific grunts'. But this is only because he believes that *some* such judgments - namely those specifying 'contradictions' - are establishable by a social *science*; and it seems to be his view that this set of judgments exhausts the nature of socialist critique. My response is that there is much that is socialist that falls outside this set, and that any position which restricts socialist

critique in this way, and dubs non-scientific, 'moralistic' critiques as (in pejorative senses) 'ethical', 'utopian', or 'reformist', may easily obliterate essential areas of moral and political discourse in the name of socialist *science*.

The belief in the superiority of science to other forms of knowledge, and in the possibility of making moral and political discourse properly scientific, is one of the unfortunate inheritances of early 19th century positivism that much Marxist theory has been saddled with. It recurs in many forms - today, for instance, in the curious belief that the defects of 'sexism', 'racism', the 'bourgeois family', etc, can be adequately characterized by attaching labels that immediately qualify them as 'ideological', 'contradictory', etc; and rejected without bothering to say just what *is* objectionable about them, and so without the possibility of engaging constructively with people who haven't already made the (presumably arbitrary) leap into radical categories with their scientific pretensions.

Finally, I suggest that Weber's attempt to specify the place of values in social science can be seen, not as a bourgeois threat to the possibility of socialist social science that it's so often taken to be, but instead as a valuable defence against this aspect of early 19th century positivism. For a fundamental theme in his writings on this issue was his opposition to political and moral values being given a misleadingly scientific status. This could happen in at least two ways: by claiming that value-judgments can be justified solely by scientifically established results; and by using one's authority as a scientist to give a quite spurious status to one's politics or ethics.

Now Weber also argued against social scientists *making* value-judgments, at least in certain contexts (e.g. especially, in teaching). But he never claimed that this prohibition could be established by epistemological or methodological arguments: it was itself a value-judgment, which he tried to justify. There are good grounds, I think, for rejecting this judgment, and for constructing a social science that is both scientific *and* socialist; but what emerges will not be scientific socialism, and this will be no loss.

**Appendix** [2013] Roy Edgley's article, 'Reason as Dialectic: Science, Social Science and Socialist Science', was published in *Radical Philosophy* (henceforth *RP*) 15, 1976, pp. 2-7, and formed part of a series of debates in *RP* during the 1970s on the nature of 'the dialectic'. My initial, brief response was published as a 'Comment' in *RP* 16, 1977, p. 48. It was followed by a longer critical response by Peter Dews, 'Misadventures of the Dialectic', in *RP* 18 1977. Edgley responded to both sets of criticisms in 'Dialectic: A Reply to Keat and Dews', *RP* 21, 1979, pp. 29-34. The present paper responds to his 'Reply', though it also returns to his original article. In 'The Trouble with Contradictions' (*RP* 23 1979, pp. 24-30), Joe McCarney examined critically the positions take by all three participants. All of these articles are available (some freely, others not) in the archives section of the *RP* website: [www.radicalphilosophy.com/archive](http://www.radicalphilosophy.com/archive) (at which can also be found moving and informative obituaries of Roy Edgley, by Joe McCarney, and of Joe McCarney, by Chris Arthur).

Many of the issues raised in this debate re-emerged more recently in an exchange between Geoffrey Hodgson and Andrew Collier about the relationship between Roy Bhaskar's 'critical realism' and socialist critique: this can be accessed in the journal *Alethia*, 2 (2), October 1999, pp. 1-9, or at [www.geoffrey-hodgson.info/debates-2.htm](http://www.geoffrey-hodgson.info/debates-2.htm)

The arguments I presented in this article drew on the defence of value-free social science in Chapter 9 ('Values, Theory and Reality') of R. Keat and J. Urry, *Social Theory as Science*, Routledge 1975/1981; they were developed further in 'The Critique of Positivism' (1980), and in Chapter 2 ('Value-Freedom and Socialist Theory') of *The Politics of Social Theory*, Blackwell 1982. These are available at [www.russellkeat.net](http://www.russellkeat.net)