

REPLY

## Marxism, materialism, and pragmatism: A reply to Simons

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Philosophy of education, at least for Martin Simons, can be a dangerous business:

I think ... that if certain priests, rabbis, ayatollahs or other tribal witchdoctors were to understand what I was, and still am, saying, they would consign me metaphorically, metaphysically and, if they had the chance, materially to the flames (Simons, 1983, p. 9).

Continuing to say what he had been saying (e.g. Simons, 1978, 1981, 1982), he neglects the option of commending to his readers the advice of one well-known priest, Dean W.R. Inge:

The object of studying philosophy is to know one's own mind, not other people's.

He does, however, in these more recent reflections on marxists and magicians, persist with his previous circumspectly indirect tactics, declining specifically to identify any marxists as magicians or ayatollahs:

It may be clear why I did not make such reference to particular magical or marxist incantations. I shall hardly do so now, either (1983, p. 9).

Contributors, especially marxist contributors, to the Australasian debate over the position of marxism in philosophy of education (mainly Stevens, Archer, and Harris) may or may not, as I suggested in the article (Walker, 1983) to which Simons' 1983 paper is in part a reply, have been puzzled as to the identity of the tribal witchdoctors whom Simons had in his sights, but can now be assured that he did not have them explicitly in mind:

My 1978 paper which, presumably by accident, appeared next to another article by Warren which was critical of marxist doctrine, was not an attack on marxism (Simons, loc. cit.).

And yet, Simons is happy enough to say that if the cap fits wear it:

... some marxists may have taken offence when they had no need to do so ... another possibility, which I suspect may be correct, is that the magicians do recognise their own irrationality and, reinforcing my main thrust against them, do not care if they are so (ibid.).

In keeping with his circumspect strategy, Simons leaves it up to the magicians to identify themselves. Fair enough; let afficianados of this debate draw their own conclusions.

I am happy to let them draw their own conclusions, too, on my dispute with Simons over his membership of the regiment of responsible scholars (his metaphor). That said, let's bury the hatchet, and focus amicably on the issues. I would like, briefly, to do no more than try to clarify some points in our exchange on two issues, the relationship between materialism and marxism, and pragmatism in epistemology.

Simons warmly and graciously concurs with the thrust of my critique of althusserian marxism, in which I argued that many of his criticisms applied to althusserianism and not to all forms of marxism. In doing so I rejected the label "marxist" in favour of "historical materialist", suggesting, however, that historical materialism had its origins in Marx's work. It seemed to me then that Marx's central ideas and methods pertaining to the analysis of the development of capitalist society

needed to be, and could be, reformulated in a fashion consistent with philosophical materialism - to be precise, physicalism. Marx himself, and most of his followers, present dualist formulations of historical analysis of social conditions. They rely on distinctions unavailable, I believe, to the physicalist - e.g. being/ consciousness, material base/ideological superstructure. Broadly, on the face of it, marxism has explained the non-material by reference to the causal priority of the material. But the material/non-material distinction cannot be drawn within physicalism, either ontologically or epistemologically (Walker 1985a). I now doubt that the main body of marxist causal hypotheses, as a body, can be reformulated in a way congruent with materialist pragmatism, the position I was sketching in the later parts of the paper to which Simons responded (for an overview of this position, see Walker & Evers, 1984). Historical materialism, in Marx's sense, is both deficient as a global theory of society and philosophically flawed. This is not to deny that the marxist tradition has made specific valuable positive contributions to social theory; it is to subscribe to the view that the theory best placed to make good use of these contributions is a materialist version of pragmatism.

In my critique of Simons and Watt I endeavoured to set the theory of dialectical contradictions within a pragmatist problems/solutions framework. This now seems to me a mistake. It has been argued by many commentators that the theory of dialectics adds nothing to our understanding of scientific method; it also seems to me quite redundant within pragmatism.

Setting these matters aside, then, let me try to clarify some points on the other main issue, pragmatism in epistemology. Here I think Simons may have misunderstood my position on three main points: the role of the theoretical underdetermination thesis within Quinean holistic epistemology, the pragmatic theory of truth, and the adequacy of pragmatist criteria for theory choice. I shall not attempt to argue in favour of the pragmatist position here: reference to other discussions will have to suffice. Nor shall I comment on Simons's detailed exposition of his own epistemological views.

Simons commences his discussion of holism by mentioning my acceptance of the Duhem-Quine thesis. Quine's version of the thesis has it that we can persist in holding true any hypothesis come what may in the way of observation and experience without being logically inconsistent. Simons comments that this raises an issue of principle, as put in the title of Sandra Harding's collection on the Duhem-Quine thesis, *Can Theories be Refuted?*<sup>1</sup> He then glosses the thesis as the view that "we cannot falsify any theory whatever in anything more than a very limited, qualified sense", and observes that "no theory stands alone but is always a member of a theoretical structure, a mutually supporting system" (Simons, 1983, p.12).

While taking the point about the theoretical structure, captured in Quine's metaphor of knowledge as a seamless web (Quine & Ullian, 1978), I think the gloss is misleading, especially its reference to falsifiability. Within our theoretical structure we can falsify, or verify, theories, given Quine's theory of truth, which, as we shall see in a moment, is not a pragmatic theory (though it is consistent with epistemological pragmatism) in the only sense which can be given to "falsify" within that theory - and it is far from a limited sense. To call it limited is to speak from the perspective of some other, and incompatible, theory, an option not open to the Quinean.

But the substantive point Simons wants to make, of course, concerns the relation between theory and evidence. On this issue it is important to distinguish clearly between the Duhem-Quine thesis and Quine's thesis of the underdetermination of theory by evidence. As noted in some comments on Lakatos's misunderstanding of the Duhem-Quine thesis (Evers & Walker, 1985), that thesis does not rule out holistic criteria for choice between (acceptance/rejection of) theories, it simply doesn't propose any, though it may be inconsistent with criteria proposed by contrary epistemologies such as positivistic empiricism. The theoretical underdetermination thesis is a different matter, and a thesis much stronger than holism. Simons, quoting Quine, notes the view that any number of alternative theories

... can be at odds with each other, and yet compatible with all possible data even in the broadest sense. In a word, they can be logically incompatible and empirically equivalent (Quine, 1970a, p. 179, quoted by Simons, 1983, p. 13).

Now whether or not all cases of theory-competition could be so represented (granted the truth of the underdetermination thesis), it should be stressed that the Quinean holist is not committed to the underdetermination thesis, and that Quine himself has more recently expressed serious doubts about it (Quine, 1975). In any case, as Evers has pointed out, Quine, in the article quoted by Simons, is quite explicit that in his own case the thesis was advanced in the first place only as an argument for his more central thesis of the indeterminacy of radical translation (Quine, 1960) and that “the argument for the thesis of the indeterminacy of radical translation based on the underdetermination thesis is only a preliminary move designed to convince only those who already accept the underdetermination thesis” (Evers, 1984b, p. 47, referring to Quine, 1970a, 1975). Moreover, despite Quine’s original strategy with the underdetermination thesis, it may be doubted whether it is in fact compatible with the indeterminacy thesis. In this respect, though not others, the underdetermination thesis may be similar to the incommensurability thesis, with which it is often confused. Since the indeterminacy thesis is an important plank of materialist pragmatism, we would be well advised not to embrace the underdetermination thesis. Instead, we place our hopes on a *coherence theory of evidence* (Evers, 1984a) embracing a *touchstone methodology* (Walker & Evers, 1982; Walker, 1984, 1985b) to assist us in theory choice and theory development. Given this approach, it is strictly speaking irrelevant to claim:

Neither can we establish the final truth of any theory in any of the empirical sciences because all the evidence will never be in (Simons, 1983, p. 12).

This brings us to the next point, Simons’s comments on pragmatism and truth. Given Quine’s theory of truth, it is also misleading to speak of “final truth”, as if we could establish degrees of truth and state the conditions under which we had the whole truth. This is to confuse the relation between truth and evidence. Neither the Duhem-Quine thesis nor the underdetermination thesis suggest this. Quine’s view of the growth of knowledge is not based on a Popperian notion of verisimilitude.

Nor do Quinean pragmatic criteria for theory evaluation imply a pragmatic notion of truth. Commenting on my advocacy of such criteria, Simons claims:

What Walker relies on here, although not saying so in so many words, is a pragmatic notion of ‘truth’ (p. 13).

The explanation for my not saying so is that it is not so. The pragmatic Quinean coherence theory of evidence or justification is to be sharply distinguished from both coherence and pragmatic theories of truth. Quine’s theory of truth is Tarski’s semantic theory (Evers, 1984a, p.25; Quine, 1970b, pp. 35-46; Romanos, 1983, Chs. 4 & 5). Romanos’s recent book on Quine contains an extremely helpful discussion, highlighting at least the following four points relevant to Simons’ interpretation of my claims.

First, Quine insists on the semantic relativity of the term “true” (or “false”) to theoretical/linguistic frameworks, in delineating the sense in which Tarski’s procedure assigns the word “true” to sentences in a given language:

It is rather when we turn back into the midst of an actually present theory, at least hypothetically accepted, that we can and do speak sensibly of this and that sentence as true. Where it makes sense to apply ‘true’ is to a sentence couched in the terms of a given theory and seen from within the theory, complete with its posited reality (Quine, 1960a, p.24; Romanos, 1983, p. 158).

Second, Tarski’s procedures remove the question of truth from the context of theory-competition:

Tarski’s semantics is simply neutral with respect to the more global philosophical questions; explicating ordinary theoretical truth for given theories or languages, it attempts to provide no absolute basis or criterion for deciding between competing theoretical frameworks themselves (Romanos, 1983, pp. 158-9).

For Quine, the basis for choice is the coherence theory of evidence.

Third, this conception of truth is ontologically neutral, given canonical notation:

Once we have determined the basic logical structure of a theory and have identified its quantifiers and variables, we will get the same truth definition regardless of what objects we choose as values of those variables (*ibid.*, p. 162).

Consequently, fourth,

... a Tarski truth definition for the language clearly transcends all the remaining relativity involved in specifying the ontology of the language. In this sense both truth and satisfaction as defined by Tarski's methods remain significantly more absolute than reference proper (*ibid.* p. 164).

If Quine's theory is sound, truth is fixed once quantification is fixed in a language: the truth conditions for sentences in that language are laid down and there is no sense in which determination of them should be confused with other questions of method. Thus Quine, unlike many other pragmatists, leaves himself the option of being a scientific realist, and takes it.

What, then, are the suggested criteria for theory-choice? It is no part of a materialist pragmatism to assert that for any given problem one and only one theory can provide a solution. Hence Simons is justified in taking me up on my statement that "the best solution is the one that solves the problem" with its (unintended) implication that some "solutions" are not solutions, and he fairly comments that "a solution that does not solve a problem is not a solution" (p. 13). The point, of course, is that for pragmatism theories are construed as programmes proposing solutions to specified problems, and are judged according to the effectiveness of the proposed solutions when acted upon. (There may, to be sure, be unrecognised or unspecified problems compromising the effectiveness of the solutions proposed for the specified problems.) The ambiguity between "solution" (effective strategy) and "proposed solution" or to be really precise "proposal for a solution" (strategy generated by the theory) is not, however, the nub of the issue. The nub, rather, comes in Simons' claim:

Empirical compatibility, which is to say, material problem solving power, is not of itself an adequate criterion for taking up or discarding a theory (p. 15).

Here, given a qualification about "empirical compatibility", we part company. The qualification is that given a materialist coherence theory of evidence, theories themselves are material entities, behavioural programmes (Walker, 1983b), and "empirical" is defined relative to a material context which exhausts the entire context, with a shifting and relative line between empirical and theoretical statements, or, rather, empirical and theoretical dimensions of a theory. There are grades of empiricity just as there are grades of theoreticity; there are no grades of materiality (though the question of abstract entities such as sets creates a problem). Roughly speaking, the closer to the centre of our epistemic web a statement is, the higher the proportion of specific theoretical content; the closer to the periphery the higher the specific empirical content. But all statements are connected, however loosely in some cases, in the same web (as Simons acknowledges). Thus it is not the case that a pragmatist must pit theory against extra-theoretical empirical evidence, and for the coherentist pragmatist theoretical virtues such as simplicity count as evidence for a theory. Problem solving power is relative to problems within the web as well as the environment, to which it is in any case materially and causally related and within which it is a behavioural programme.

Given this approach, the hypothesis is that the elements of theoretical systematic virtue (e.g. logical consistency, simplicity, familiarity of principle, scope, fecundity, and ability to account for testable consequences - Quine, 1960b, p 147; Evers, 1984a, p. 24) are justified algorithmically - i.e. in terms of their problem-solving power. They are not justifiable separately, nor do they represent criteria distinct from algorithmic capacities.

This is neither to suggest that they never conflict with each other in specific cases, nor to deny that they are themselves part of the web. Our web has to be self-referential if we are not

foundationalists. Nor are they in an epistemically privileged position: I do not share Simons' view of the authority of metasystems. Nor do I wish to dismiss Simons' point that we have to live with theories which, however well they seem to solve the respective separate problems to which they are addressed, are inconsistent with each other, for example in their ontic commitments. For instance the apparent dualism of the folk psychology which we use in everyday life (and educational psychology) should not deter the materialist from using folk psychology while at the same time vigorously supporting neuroscience and materialist social science in the enterprise of making folk psychology redundant (Evers, 1985; Churchland, 1979, 1981, 1983). Coherence, including the baseline virtue of logical consistency, is a goal rather than a requirement we can meet absolutely at all times. In this, unlike truth, it does admit of degrees, and therein lies the overall direction of the growth of knowledge.

Finally, as I noted in my critique of Simons and Watt, there is no sense in which we can stand outside our whole web of belief (or, for Simons, way of life) and make value judgements about it. Values are within the web, and indeed no sharp line can be drawn between them and other elements of the web, especially in terms of their justification. Thus we cannot, short of suicide use values to opt out of the web: even then, it would be a self-judgement of the web, rather than an external value judgement upon it. We can modify the web, develop it, replace parts of it with new elements, but it and the solutions to problems are where we start. Simons notes Neurath's metaphor of the boat. Well, we are stuck on our boats unless another one comes along which we judge, from the perspective of our present boat, to be better: actually, to be more precise, we would have to run the boats in tandem for a while to get some experience of the problem-solving power of each. But the physicalist claims that it is never a question, really, of forsaking our entire web of belief for another: theoretical changes are repairs to the one basic ship. Just as there is no "first philosophy", so there are no "first values" derivable or justifiable from something outside our genetic constitutions and our cultural environment.

## Notes

1. Reidel is the publisher of this book, not the editor, as Simons's text suggests (See Harding, 1976).

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