

A Social Theory Dialogue Between Peter Manicas and Patrick Baert

Peter Manicas and Patrick Baert are highly regarded social theorists with quite different views on the relevance of realism in and for social theory. Here each reviews the others recent work and in a series of exchanges lays out the basis of some of their differences. In addition to the intrinsic interest that the ideas discussed here have, the dialogue that develops between Manicas and Baert should also be of interest to a number of different kinds of reader. It should be of interest to postgraduates looking for clear and succinct accounts of opposing positions by key thinkers. It should be of interest to readers who follow Alisdair MacIntyre's interest in the limits of judgemental rationality in the social sciences because the dialogue is also a fascinating illustration of the real sociological fact of perpetuated disagreement regarding fundamentals – whether one wants to label this as incommensurability or not. It should also be of interest because it illustrates that key thinkers from differing positions are prepared to engage on the issues that concern them in a constructive fashion. This in itself shows the limits of the antithesis of judgemental rationality because there is at least an ethic of public dialogue. Most interestingly what we take from those limits is itself a part of the dialogue between Manicas and Baert. Each reads its significance differently but each, nonetheless, finds the process worthwhile. With this in mind I would like to thank both Peter and Patrick for their participation and cooperation. —Jamie Morgan

Post-Modern Pragmatism and Social Science

Peter Manicas

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Introduction

Patrick Baert has written an ambitious and provocative book. He tells us that he had two objectives. The first is 'to advance a new approach to [philosophy of the social sciences] that is indebted to American pragmatism' (p. 1). In brief, once 'foundationalism' and 'naturalism' are rejected, we can acknowledge for social science a diversity of 'cognitive interests' including 'self-knowledge', and a pluralism of 'method'. To my knowledge, this is an entirely novel proposal, both in making self-knowledge an important goal of research in the social sciences, and in seeking to ground this in pragmatism. A number of writers, including, e.g., Rescher and Laudan,¹ and perhaps one could count also Quine and Putnam, have provided at least the rudiments of a pragmatic philosophy of science, but excepting perhaps Margolis, to my knowledge, no recent self-declared pragmatist has looked specifically at the social sciences.² Moreover, Baert recognizes that he is jumping into a beehive of controversy over just what pragmatism is, a problem decidedly complicated by the intervention of Richard Rorty. It is true that Rorty's work has brought 'pragmatism' back to the mainstream, but he has been severely criticized by a wide variety of interpreters of American pragmatism.³

The second goal 'is to present an advanced assessment of the main approaches in philosophy of social sciences.' The book, he says, 'is written so that it can be read in either way' (p. 1). The latter goal is provided by the first five chapters. Those interested in his new approach 'can read chapters 6 and 7 and for those already familiar with pragmatism, only chapter 7, the concluding chapter.' But 'this is not to say that chapters 1 to 6 are irrelevant to the concluding chapter' (p. 1). While the authors discussed in the first five chapters were not chosen because 'they somehow fit into narrative that ultimately leads towards [his] pragmatic view,' his pragmatic proposal 'is partly based on a rejection of other strategies in the philosophy of the social sciences' (p. 3). Accordingly, the topics and authors of the first five chapters 'were chosen because their perspectives are central in the philosophy of social sciences. Chapter 1 considers Durkheim's 'naturalism', chapter 2 discusses Weber's 'interpretative method', chapter 3, Popper's 'falsificationism', 4, 'critical realism', and 5, the critical theory of the Frankfurt school. Chapter 6, 'Richard Rorty and Pragmatism', then, provides the point of departure for Baert's concluding chapter 7, 'A Pragmatist Philosophy of the Social Sciences'.

All of the first five chapters are of interest and each provides learned exposition and assessment of the positions under discussion. One might quarrel with this or that point of interpretation, but this will not be the focus of the present review. But if the individual chapters are useful, one might say that the selection of topics and authors of the first five chapters does not in a helpful way allow us to identify the central perspectives in philosophy of social science. To be sure, much depends on how one wants to cut the pie as regards alternative positions. For example, Baert writes that Durkheim's work stands in 'an uneasy relation to positivism' (p. 13) and that indeed, 'Durkheim viewed science as capable of delving beneath the surface and uncovering underlying mechanisms that account for the observed regularities' (p. 15). If true then this is squarely a realist position. Baert's account of Durkheim may be recommended as a contribution to the literature on Durkheim, but my point here is that since the still dominating philosophy of social science among sociologists is a version of positivism, this needs representation and comment. One thinks here of

¹ See Nicholas Rescher, *Methodological Pragmatism* (Oxford: Basil Blackwell, 1975) and Larry Laudan, *Science and Values* (Berkeley: University of California Press, 1984).

² See Joseph Margolis, *Science Without Unity* (Oxford: Basil Blackwell, 1986). I consider Dewey's views of social science in 'John Dewey and American Social Science', in Hickman, Larry (ed.), *Reading Dewey: Interpretations for a Postmodern Generation* (Bloomington: University of Indiana Press, 1998).

³ See, for example, Ralph W. Sleeper, *The Necessity of Pragmatism* (New Haven: Yale University Press, 1986), Larry Hickman, *John Dewey's Pragmatic Philosophy* (Indianapolis: University of Indiana Press, 1990), Richard J. Bernstein, *The New Constellation* (Cambridge, Ma.: MIT Press, 1992), and John Stuhr, *Pragmatism, Postmodernism and the Future of Philosophy* (London: Routledge, 2003).

Lundberg, Homans or more recently, Jonathan Turner.⁴ Similarly, Weber was surely anti-positivist and a key player in the Methodenstreit, and as Baert rightly sees, he ‘transcended’ the polarization between the positivist idea of a nomothetic science and a Diltheyan version of the human sciences as ideographic. For him causal analysis and ‘interpretation’ were both essential and possible. Indeed, given his views of causation, it is not difficult to give a reading of Weber which makes him close to a realist position.⁵ Similarly, then, Weber may not be the best choice to represent interpretative sociology, generally understood to be a distinctly ‘anti-naturalist’ posture and still prominent in sociology and anthropology. Among philosophers, one thinks here of Gadamer and Natanson, and among social scientists, Geertz, any number of recent cultural anthropologists, and sociologists inspired by an anti-naturalistic phenomenological posture.⁶ Popper is an odd choice for all sorts of reasons, not least because he wrote practically nothing in the philosophy of social science and has left no legacy among social scientists. Indeed, there are plenty of signs that his major contribution to the philosophy of history, the critique of ‘historicism’ (in his idiosyncratic sense) has still not penetrated the thinking of many social scientists. The account of the Frankfurt school is both sound and appropriate, but this ignores views which are closer to Marx and perhaps more influential. One thinks here, for example of E.P. Thompson, Paul Willis, Nancy Fraser, Mike Davis and many others. Critical realism has drawn on this wing of Marxism, so perhaps Baert’s account of it is all that is necessary here. His account of critical realism is generally sound but since I share this view and Baert takes it to be an exemplary version of what he rejects, our efforts in this issue give the reader a clear choice. In any case, perhaps for Baert, the selection of writers is not that important since it seems that the problem with all the writers discussed in the first five chapters is their commitment to either ‘naturalism’, understood as ‘the search for a single scientific method appropriate for the study of both the social and the natural realms’ (p. 3) or to ‘foundationalism’, understood as ‘the effort to uncover unchanging foundations of an all-embracing framework or science of the social’ (p. 153). These are the twin bogies which ‘pragmatism’ allows us to exorcise.

My second problem is this: How well does Baert negotiate the thicket of pragmatism? Baert notes that while his view is ‘in line with recent contributions to pragmatism, specifically Rorty and Bernstein, I am not arguing that my views are necessarily consistent with those expressed by earlier generations of

⁴ See Turner, ‘Analytic Theorizing’, in Giddens, A. and Turner, J. (eds.) *Social Theory Today* (Oxford: Polity Press, 1987). Baert holds that ‘most mainstream social research complies with the intricate procedures suggested by the realist agenda’ and that ‘it is simply not the case that contemporary social researchers are satisfied with a mere recording of regularity conjunctions; they look for mechanisms that account for *how* the regularities are brought about’ (p. 102). But as Calhoun writes (following Boudin), ‘most of what passes as causal analysis in the social sciences is in fact identification of more or less ‘weak implication’ between statistical variables’ (‘Explanation in Historical Sociology: Narrative, General Theory and Historically Specific Theory’, *American Journal of Sociology* 104 (3) (1998): 866). Moreover, it is easy to show that the ‘official’ position of most social researchers is that the goal of science is to establish ‘laws’ construed as regularities which then ‘explain’ by subsumption. Here are one or two examples: Frankfort Nachmias and Nachmias write: ‘Ever since David Hume...an application of the term explanation has been considered a matter of relating the phenomena to be explained with other phenomena by means of general laws’ (Frankfort-Nachmias, Chavas and David, *Research Methods in the Social Sciences*, 4th Edition (New York: St. Martins Press, 1992: 10); Babbie writes: ‘In large part research aims to find patterns of regularity in social life’ (Earl Babbie, *The Practice of Social Research*, 10th Edition (Belmont, Ca.: Thomson Wadsworth, 2004: 13). Even somebody like Jeffrey Alexander who, as a post-Kuhnian, rejects classic positivist epistemology would not say that ‘there is no ‘objective’ knowledge in the social sciences, nor even that there is no possibility of successful predictions or covering laws (Jeffrey Alexander, ‘The Centrality of the Classics’, in Giddens and Turner, *Social Theory Today* (1987: 20). To be sure, like Durkheim, researchers very often let good sense get in the way, contradict themselves, and hint at causal mechanisms. Perhaps this is what Baert had in mind.

⁵ Peter T. Manicas, *A Realist Philosophy of Social Science* (Cambridge: Cambridge University Press, 2006: 115-125).

⁶ See Rabinow, Paul and Sullivan, William (eds), *Interpretative Social Science* (Berkeley: University of California Press, 1987). Interpretation of Alfred Schütz as anti-naturalistic is contestable, as is the understanding of work of the ethnomethodologists and perhaps also Goffman. See my ‘The Social Sciences Since World War II: The Rise and Fall of Scientism,’ in William Outhwaite and Stephen P. Turner (eds.), *Handbook of Social Science Methodology* (London: Sage, 2007).

pragmatism' (p. 147).⁷ Rather, his proposal 'is inspired by neo-pragmatism rather than derived from it.' The gist of his argument, he continues, 'is perfectly consistent with the philosophical outlook of neo-pragmatism' (ibid.). I think that it is easy to show that his view is not consistent with Peirce, James or Dewey but will not labour the point here. Here agreeing with Bernstein, I conclude that Rorty got Dewey entirely wrong. Baert, while seeing some serious problems in Rorty, nevertheless thinks of pragmatism pretty much as Rorty does. The concluding chapter very conveniently summarizes his proposal under six major headings. It will be useful here, one hopes, to address these seriatim.

1. 'Methodological diversity characterizes science' (p. 147)

As noted, Baert rejects what he terms 'naturalism', 'the search for a single scientific method for the study of both the social and the natural sciences' (p. 3).⁸ Drawing on Rorty he asserts that this search has failed (Baert: pp. 131-135). A good deal depends, of course, on what is to count as 'a single scientific method.' Here one must distinguish method as technique and method in the wider sense of presupposing a philosophy of science. To be sure, astronomy does not employ experiments, ethnographers do not employ multiple regression, and surely there are differences between the practices of the physical sciences and the practices of most social scientists: a straightforward consequence of differences in ontology: the social world is meaningful and does not exist independently of the actions of persons. Thus, the physical scientist does not ask for the meaning of action of molecules; if human action is to be explained, the social scientist must seek meaning (as Weber, ethnographers, and critical realists emphasize.) Similarly, for post-Kuhnian epistemology, there is no algorithm which assures a scientific consensus (when it is achieved), no 'logic' of discovery or confirmation—or as per Popper, logic of 'falsification'. Indeed, dispelling this myth was surely the major achievement of both Kuhn and Feyerabend.⁹ But it does not follow that the successful sciences share nothing of importance, however different are their practices and techniques (both across disciplines and historically within disciplines). At the very least, the practices of the successful physical sciences have evolved norms regarding inquiry which practitioners acknowledge, usually tacitly. Publicity and consideration of evidence is one. Acknowledgement of a stubborn reality and fallibilism is another. Thus, while very powerful work in the sociology of science gives us a deeper understanding of the actual practices of the sciences, few, if anyone, would go so far as to say that scientific practices are indistinguishable 'methodologically' from non-scientific practices.¹⁰ Baert's view is especially ironical as regards the pragmatists. Peirce famously distinguished four methods of fixing belief and insisted that the 'method of science', as the only one which is self-corrective, had to be preferred—pragmatically. Similarly, Dewey put huge weight on inquiry insisting that the successful sciences were successful because they were in fact using the 'logic' which for him not only characterized science, but everyday "intelligent" problem solving.¹¹ The classical pragmatists rejected Kant's transcendental move, but unlike Rorty, they sought a reconstruction of philosophy, not a rejection of it. As we shall see, on this critical issue, Baert is strongly pulled in the direction of Rorty.

⁷ It is not clear whether Bernstein can be produced as an ally in Baert's project. While this cannot be developed here, see below for some suggestions.

⁸ Baert acknowledges that critical realism is a 'qualified naturalism' in that while it models social science on natural science, it insists on important differences between the social and natural sciences (p. 96). Similarly as regards Weber. Durkheim's naturalism is unqualified and, as Schütz pointed out, it found expression in Parsons and those who followed him. See my 'The Social Sciences Since World War II: The Rise and Fall of Scientism,' As I noted earlier, there are unqualified anti-naturalisms as well.

⁹ Versus 'logical empiricism', they displaced the 'logic' of the logicians and undermined positivist foundationist epistemology. Interesting in this regard is Baert's claim that if as realists say, most systems are open, then 'most scientific explanations cannot be properly justified philosophically' (p. 103). This surely seems like a hankering for justification which, as pragmatists insist, is neither available nor necessary.

¹⁰ See, for example, Andrew Pickering (ed), *Science as Practice and Culture* (Chicago: University of Chicago Press, 1992).

¹¹ This is doubly ironical since a persistent misreading of Dewey is that he was 'scientistic', reducing all inquiry to a narrow instrumentalist—positivist—reading. See my 'Pragmatic Philosophy of Science and the Charge of Scientism,' *Transactions of the Charles S. Peirce Society*, Vol. XXIV, No. 4 (Spring 1988) and 'Dewey and American Psychology,' *Journal for the Theory of Social Behavior*, 32 (3) (2002).

2. 'The social sciences gain from methodological pluralism' (p. 150)

For Baert there is a 'multitude of cognitive interests that underlie social research' and it is an error to reduce these to one: 'explanation, possibly prediction' (p. 150). It is not clear to me what these many "cognitive interests" (which smacks more of Habermas than any pragmatist) are or what the argument here is. Is this, for example, an empirical claim? Is it normative? As he says, if 'prediction and control' is the goal, then the 'value-free' 'vocabulary' of 'naturalistic' approaches will suffice. If, on the other hand, 'we want to treat human beings as moral individuals, then surely anti-naturalistic approaches are called for' (p. 134). This seems to be, following Rorty, a very positivist view of the matter, not shared by critical realists or Dewey. Rorty is clearly positivist. He 'sums up' by offering 'two distinct requirements for the vocabulary of the social sciences: (1) It should contain descriptions of situations which facilitate their prediction and control' and (2) 'It should contain descriptions which help one decide what to do' (Rorty 1982: 197). Rorty notes, correctly, those who aim at prediction do very poorly. He might also have noticed that not only does 'control' require very little science (it requires power), it too is limited—as regards persons, thank heaven, and as regards nature, it is entirely absent when it comes to ensuring that nature will do our bidding. The best we can do here is try to understand its dynamics, to avoid disastrous intervention, and otherwise to learn 'to cope'.

It is not clear how much of Rorty's 'summary' Baert accepts, since most obviously missing in Rorty's account is the realist idea that the 'descriptions' should have nothing to do with 'prediction' and should aim at facilitating explanation in the realist sense. That is, just as we understand why iron rusts because we have molecular chemistry, we can understand why working class kids get working class jobs. That is, we need an account of the beliefs and attitudes of working class kids (and their teachers and parents) which explains what they do, and then an account which provides the conditions and consequences, mostly unintended, of these actions. Indeed, only with such understanding can we build technologies and/or intervene successfully. May we suppose that along with prediction and control, methodological pluralism tolerates realist goals-- and others perhaps not identified? But we are entitled to ask which of the identified goals bear pragmatic fruit? On this point, there would seem to be deep disagreement between myself and Baert.

3. 'The spectator theory of knowledge is inappropriate for social research' (p. 151)

The 'spectator theory of knowledge' is, of course, Dewey's term. It was aimed at all epistemologies which fail to appreciate that knowing is an active relation between the knower and the known, and that inquiry is constrained by both the practical concern which generates it and the constraints imposed by the environment in which the inquirer is situated.¹² In his demolition of foundational epistemology, Rorty enlisted Dewey. But we need to notice that not only did they have different purposes, but they began with very different assumptions: Dewey assumed a philosophical naturalism and sought a reconstruction of philosophy in which the spectator theory of knowledge was replaced by "not another epistemology" but with a naturalistic theory of inquiry—finally elaborated in his generally ignored and misunderstood 1938 *Logic*.¹³ For Dewey, inquirers are always situated in space and time, confront a material and historically produced social world, and are motivated to inquiry to solve a problem. Indeed, it is this sense of 'practical' which is shared by all pragmatists. Knowledge, then, is simply the product of competent inquiry. A God's eye for the world is neither necessary nor possible. Thus, for example, when known as Fe, iron enters into new relations for us.¹⁴ Rorty, by contrast, wants to insist that the product of inquiry cannot be a

¹² Dewey writes: 'If we see that knowing is not the act of an outside spectator but of a participator inside the natural and social scene, then the true object of knowledge resides in the consequences of directed action...For on this basis there will be as many kinds of known objects as there are kinds of effectively conducted operations of inquiry which result in the consequences intended' (*Quest for Certainty*, London: Allan and Unwin, 1930: 196-97). This includes knowing that iron is Fe and will, *ceteris paribus*, oxidize.

¹³ See Tom Burke, *Dewey's New Logic: A Reply to Russell* (Chicago: University of Chicago Press, 1994) and my 'John Dewey and American Psychology.'

¹⁴See J.E. Tiles, *Dewey* (London: Routledge, 1988), along with Sleeper, *The Necessity of Pragmatism*, 1986), still the best overall treatment of Dewey. See also Richard J. Bernstein, *Praxis and Action*

representation of a reality even though, for Dewey, it is the hard won product of controlled experiment: the paradigm of an active effort to produce knowledge.¹⁵ Similarly, Rorty aims not at reconstruction, but thinks that the ‘problems’ of philosophy, including epistemology and ontology disappear once we see that it is all a matter of finding a ‘vocabulary’ suitable for the purpose at hand— which appears to be Baert’s main indebtedness to Rorty .

Thus, contemporary ‘textualism’, the idea that there is nothing but texts parallels the idealist notion that there is nothing but ideas.¹⁶ But there is, he insists, a critical difference between current textualism and classical idealism. In repudiating the tradition, textualists reject the framework which allows for epistemology and ontology. Thus, unlike idealists (or naturalists or materialists) so-called ‘post-modern’ writers reject the idea that what is important is not whether what we believe is true, but what ‘vocabulary we use’. Finally, then, for Rorty, pragmatism joins post-modern thinking in repudiating metaphysical argument between idealist/naturalists—and the epistemological idea of truth as correspondence with reality.

Baert has some misgivings with aspects of Rorty’s thought—for example, his polemic against Marx and his politics, but finds that most of what Rorty offers is convincing. Similarly Rorty sees that Dewey does not exactly fit his larger picture. In agreement with Santayana, Rorty insists that Dewey’s efforts at a ‘naturalistic metaphysics’ betrays ‘a recurrent flaw in Dewey’s work: his habit of announcing a bold new positive program when all he offers, and all he needs to offer, is criticism of the tradition’.¹⁷ To be sure, Dewey does offer ‘a bold new positive program’ —a naturalistic metaphysics with epistemology replaced by his version of ‘logic’. And he needed to do this because he could not step out of history and argue, as Rorty does, that knowledge and truth are pseudo problems which will go away once we abandon the claims of philosophy. Indeed, it is quite one thing to try to convince us that ‘warranted assertability’ could replace ‘truth’, understood as a certainty, and quite another to say that, for pragmatists, ‘there are no constraints on inquiry save conversational ones—no wholesale constraints derived from the nature of objects, or the mind, or of language, but only those retail constraints provided by the remarks of our fellow-inquirers’.¹⁸ Worse, ‘the Socratic virtues—willingness to talk, to listen to other people, to weigh the consequences of actions on other people—are simply moral virtues... The pragmatists tell us that the conversation, which it is our moral duty to continue is merely our project, the European’s intellectual form of life’.¹⁹ Perhaps we can put aside this remarkable provincialism and consider more broadly ‘conversation’ as the key to ‘successful’ outcomes.

4. ‘Social research is a conversation’ (p. 153)

This would seem to be the most obvious of the influences of Rorty on Baert’s project. Baert sees rightly that classical pragmatism rejected what is generally termed ‘foundationalism’, the search for secure epistemic grounding of truth claims. But there is a critical difference between what might be called ‘the situated objectivity’ defended by Dewey—who as noted, agreed that there could be no God’s eye view of the world and the position taken by Rorty. As with metaphysical controversy, which dissolves once we see that all that is necessary is an appropriate vocabulary, relativism can similarly be evaded — not answered. One we rid ourselves of the Enlightenment framework, we ‘would no longer be haunted by specters called

(Philadelphia: University of Pennsylvania Press, 1971), who develops the strong similarities between Marx’s notion of praxis and Dewey views on action.

¹⁵ The work of Hacking and Pickering is very Deweyan. Thus Hacking writes: “The theories of the laboratory sciences are not directly compared to ‘the world’; they persist because they are true to the phenomena produced or even created by apparatus in the laboratory and measured by instruments that we have engineered” (‘The Self-Vindication of the laboratory sciences,’ in Pickering, *Science as Practice and Culture*, 1992: 30). See also Pickering’s idea of the ‘mangle of practice’. Critical realists can, of course, share in this view. The so-called correspondence theory of truth cannot, to be sure, provide a mode of establishing truth, even if ‘true’ may still *mean* ‘corresponds to the facts’.

¹⁶ Richard Rorty, *Consequences of Pragmatism* (Minneapolis: University of Minnesota Press, 1982: 139).

¹⁷ *Ibid.*, p. 78.

¹⁸ *Ibid.*, p. 165.

¹⁹ *Ibid.*, p. 172.

‘relativism’ and ‘irrationalism’’.²⁰ Instead of directly addressing this question, Baert calls our attention to another sense of ‘foundationalism’, ‘the effort to uncover unchanging foundations of an all-embracing framework of science or science of the social’ (p. 153). Critical realists are the ‘purest expression’ of this, but it is true of Parson’s structural functionalism, Giddens’s structuration theory (which I consider a version of realism), and the theories of Luhmann, Habermas, and rational choice theory. These metatheories, like the search for a method, presuppose both an epistemology and an ontology of science and, as with Rorty, more generally as regards philosophy, they similarly fail.

Perhaps remarkably, a solution to both senses of foundationalism seems to be found in the work of Richard Bernstein and what he called a ‘dialogic encounter’. Baert is clear on what he means:

In a dialogic encounter people do not wish to score points by exploiting the weakness of others; they try to listen to them by understanding them in the strongest way. They strengthen their own arguments so as to make them most credible and to learn from them. Academic communication, then, becomes a more like a proper conversation, which encourages the participants to think differently. The ultimate aim is not to defend a refine a particular system but to use academic conversation to enhance our imaginative faculties. (p. 154)

It would seem foolish to quarrel with the good manners of this good advice—even if it is a council of perfection. The more difficult question is how this bears on the question of addressing deep differences in frameworks, between say Giddens and Parsons, or rational choice theorists and critical theorists? Indeed, having enhanced our imaginative faculties, are we now to say that one can only be tolerant of differences since there is no true, correct, valid, warranted, justified answer to the questions of epistemology and ontology? Rorty at least would seem to be content to stop here.

5. ‘Knowledge as action’ (p. 154)

As above, for Baert, the question of method depends in part at least on the goals of research, but ‘methodological questions cannot be reduced to questions of ontology’ (p. 154). Accordingly, for him, ‘there is nothing essential about the social that compels the use of a particular method’ (p. 154). Assuming different goals, strictly speaking, this seems correct: If prediction and control are our aims, perhaps we can think of human action in the same way we think of the movement of planets. But indeed, this seems like a reduction ad absurdum of the ontology! This is not merely a philosophical prejudice: the empirical evidence shows it to be nonsense. That is, as Weber insisted, even if a goal of science is to ‘predict’ action, one needs *verstehen* to do this. Moreover, as Weber, correctly saw, as a presupposition of social interaction, science is hardly necessary!

6. ‘Self-understanding opens up alternative scenarios’ (p. 155)

A ‘dialogic encounter’ has additional fruits. Baert recommends ‘self-knowledge’ as a worthy cognitive object for social science. Following Gadamer, ‘understanding ought to be seen as an encounter, firstly, in which we rely upon our cultural presuppositions to gain access to what is being studied, and secondly, through which we articulate and rearticulate the very same presuppositions’ (p. 155). To do this, we need to reject the idea that ‘the right interpretative method would allow us to touch upon the ‘reality-out-there,’ but we gain sensitivity to views not held by us (ibid.). The third consequence of the hermeneutic circle, so conceived, is that ‘‘understanding’ is closely lined to ‘self-understanding.’’ Baert concludes the volume with illustrations from several ongoing research programs, including cultural anthropology, archaeology, and the genealogical work of Foucault.

Again, there is surely no complaint about social scientific efforts at self-understanding. And no doubt encountering alien cultures and deconstructing historical constructions can be a most useful means to this. On the other hand, as Baert acknowledges, other genre: fiction, biography, narrative history, well serve this aim, as of course, does travel. But more pertinent, the question arises whether some forms of social science serve this role as part of a larger goal: the possibility of human emancipation.

²⁰ For criticism of Rorty’s effort here see, Richard J. Bernstein, *The New Constellation* (Cambridge, MA.: MIT Press, 1992: 270-273.

Baert notes that as regards self understanding there is the 'emancipation effect'. '[E]ncountering difference may allow people to question some of their deep-seated beliefs,' 'to distinguish the necessary from the contingent, the essential from historical specificity' (p. 156). But why stop here? Why not also argue that for exactly the same reasons, understanding the causal conditions and consequences of action is also emancipating? This is, of course, a critical consequence of realist social science. Indeed, it provides a sufficient ground for immanent critique, surely an important task for social science. That is, understanding why one believes what one believes and seeing that such belief is false but essential to the reproduction of a practice, gives one good reasons to challenge the practice. Baert gives a sympathetic reading of Foucault along these lines. He might well have taken more seriously Bernstein's penetrating criticism of Rorty and his powerful defense of immanent critique against all those post-modern writers who, having dismissed philosophy, have lost all confidence in the rational grounding of critique.²¹

Patrick Baert Replies: Pragmatism Misunderstood

I would like thank Peter Manicas for properly engaging with my book and for presenting a sharp challenge to its central arguments. I will take his points one by one.

1. The canon

Let me first deal with Peter Manicas' preliminary point about my selection of authors and traditions. Manicas would agree that this is not his most important criticism, but it was the first on his list, so I ought to discuss it before tackling his more substantial points. Manicas seems to disagree with my choice of authors on the basis that 'one might say that the selection of topics and authors ... does not in a helpful way allow us to identify the central perspectives in philosophy of social science.' The grounds for Manicas' dissatisfaction with my selection lie in the fact that, according to him, the authors and traditions discussed had little effect on the social sciences (or, in the case of Max Weber, fail to represent a particular school). I disagree with him.

Take Durkheim, for example. Whilst Manicas has no qualms with my account of Durkheim and argued that it '... may be recommended as contribution to the literature on Durkheim...', he does not think that Durkheim deserves a chapter. Why not? According to Manicas, the reason for this is straightforward. He agrees with my argument that Durkheim has wrongly been portrayed as a straightforward positivist and that some aspects of Durkheim's writings reveal views which are closer to a realist notions of causality. He then uses this argument against me, by asserting that '... since the still dominating philosophy of social science among sociologists is a version of positivism, this needs representation and comment'. So the gist of his argument comes down to the well known realist view that most social scientists are positivists of some kind - open or closeted. I discuss this point in my commentary on Manicas' book: very few social scientists subscribe to or act according to positivism, at least not in the way in which he defines the term. My chapter on Durkheim shows that 'positivism' is an overused label, so often quickly employed by lazy commentators to denigrate their opponents without properly engaging with the arguments presented. I show that even Durkheim's work - considered by so many to be the archetypal exponent of positivism - cannot be portrayed as straightforwardly positivist. Now, the fact that I show that *some* aspects of Durkheim strike a cord with contemporary realism does not mean Durkheim's work can be equated with realism because it differs in important respects: for example, Durkheim articulates the relationship between agency and structure in a different fashion and he has a very different view about the relationship between social science and social engineering. Durkheim's distinct views about social science had a tremendous impact on the history of sociology, not just in France where he managed to dominate intellectually and institutionally the intellectual scene, but also in the US. Various structural models in sociology and recent contributions to cultural sociology are deeply indebted to Durkheim (e.g. Merton 1968; Alexander 1988; Alexander and Smith 2005).

In the case of Weber, Manicas changes tack. He does not argue that Weber has little influence on the discipline of sociology, but takes issue with the presentation of Weber as representative of its interpretive approaches. Again, he agrees with my reconstruction of the author concerned and uses it against me. He accepts that Weber was not a clear-cut hermeneutic author, but then follows that up with the assertion that '... Weber may not be the best choice to represent interpretative sociology, generally understood to be a

²¹ *Ibid.*, p. 316.

distinctly 'anti-naturalist' posture and still prominent in sociology and anthropology.' Now, it was never my intention to discuss interpretative sociology in its ideal-typical form (to use a Weberian term). I decided it was more important to focus on an author, like Weber, who took on board hermeneutic views but also influenced the social sciences considerably. Manicas mentions Gadamer, Natanson and Geertz as anti-naturalists who I should have considered instead. While he is right that they belong far more clearly in the anti-naturalist camp than Weber ever did, the influence of these authors on the actual practice of social science pales into insignificance compared to Weber's. Gadamer was (and is) a colossal figure in philosophy but his impact on social science has always been limited, while Natanson was a very interesting interpreter of Continental and American philosophy, but with little influence on contemporary philosophy, let alone social science. As for Geertz's work, it was undoubtedly a *cause célèbre* in social and cultural anthropology but only for a limited period of time. In contrast, there is a huge tradition of Weberian sociology which is still thriving today - think of the very important work of Randall Collins (1986) and Michael Mann (1986, 1993) - and the very fact that Manicas himself draws heavily on Weber undermines his own comment that I should have focused on others.

Regarding Popper, Manicas' charge of non-centrality is similarly difficult to accept. He argues that '... Popper is an odd choice for all sorts of reasons, not least because he wrote practically nothing in the philosophy of social science and has left no legacy amongst social scientists.' It is certainly true that Popper did not know a huge amount about the social sciences (as I explain in the book at length), but this was never an impediment for him to write about the philosophy of the social sciences. *The Poverty of Historicism* (Popper 1991b) might be the best known of his works, but as I explain in my book, there are also large sections devoted to the social sciences in other works, including well known ones like *The Open Society and its Enemies* (Popper 1971a, 1971b) and *Conjectures and Refutations* (Popper 1991a). I show that Popper's contribution to the philosophy of the social sciences is not simply a negative one (criticising historicism and holism) but that it also develops a coherent proposal for how to conduct social research. As I mention in my book, Karl Popper's influence in the social sciences is enormous, and many contemporary advocates of rational choice theory (and rational action theory) still see Popper's writings as inspirational. Manicas additional comment that Popper's critique of historicism '... has still not penetrated the thinking of many social scientists...' seems baffling. Very few social scientists these days are historicists in the way in which Popper defines the term. From postmodern scepticism towards meta-narratives to the empirical orthodoxy of contemporary social and political science, there is an anti-historicist consensus.

Manicas seems happy with the inclusion of critical theory but complains that I ignored '...views which are closer to Marx and perhaps more influential.' Manicas suggests names of people who could have been included: E.P. Thompson, Paul Willis, Nancy Fraser and Mike Davis. Authors such as these were omitted on the principle that critical theory ought to be included because of the philosophical strength of the arguments presented. This explains the inclusion of Adorno, Horkheimer and Habermas. Whilst it is difficult to deny the quality of the empirical work of E.P. Thompson and the work of Nancy Fraser in political theory and their importance in their respective areas, it is equally difficult to conceive of their work as major contributions to the philosophy of social science. As for Willis and Davis, the suggestion to include them is even less obvious. Willis' book on working-class kids might be regarded by some social scientists as a good piece of ethnography, but it does not include any significant insights regarding philosophy or epistemology. The same applies to Mike Davis' work, which might have considerable political relevance but does not tackle philosophical issues.

2. Pragmatism and pragmatisms

Manicas makes a second preliminary point, which deals with whether my argument is pragmatist. 'How well does Baert negotiate the thicket of pragmatism?' It is unclear what to make of Manicas' answer to his own question - partly because of the brevity of his reply - but he seems to present the condemnatory syllogism that my view is close to Rorty, that Rorty presents a bastardised version of pragmatism, and that therefore my view is not genuinely pragmatist. I have never been particularly interested in the quest for the one and true pragmatism, not just because there are so many versions of it, but also because questions of this kind are irrelevant to my own intellectual agenda, which is to put forward a proposal for a new philosophy of social science. Manicas writes that '... it is easy to show that his (i.e. my, P.B.) view is not consistent with Peirce, James or Dewey...' but then decides not to follow this up, so one is left having to take his word for it. I am not sure why the possible differences from the holy trinity of pragmatism are at all relevant, given that I made it very clear in my book that '... my suggestions are in line with recent

contributions to pragmatism, specifically Rorty and Bernstein. I am not arguing that my views are necessarily consistent with those expressed by earlier generations of pragmatists.’ (Baert 2005, p. 147) Interestingly, Manicas acknowledges this passage, but still decides to make this unfair point. Even leaving this aside, his arguments about my relationship to classical pragmatism are unfounded. Of course, some of my views differ from those of the three authors mentioned by Manicas, but the three philosophers exhibited substantial differences (to such an extent that Peirce distanced himself from the label ‘pragmatism’) and I do share a number of substantial points with all three. There are other important reasons not to stick to classical pragmatism and to move beyond it. Let me briefly mention two. First, as I have tried to explain in my book, philosophers of science can no longer ignore insights from the sociology and history of science. Those social scientists have shown a very different picture of scientific activity than that portrayed by John Dewey and his contemporaries, and to such an extent that it becomes difficult to think about science in the way in which they did. Second, there have been various developments in analytical and continental philosophy in the course of the twentieth century, which compliment Dewey’s views but are important steps in their own right and force us to think differently about science, social science and philosophy. Manicas makes disparaging (and largely unsubstantiated) remarks about Rorty, but at least Rorty realised that contemporary philosophy had gone a long way, even to the extent that it altered how it should see itself as a discipline in relation to the world and to other intellectual activities. Amongst those developments is what has sometimes been referred to as the linguistic turn (Rorty 1992).

I will now move to the more substantial points raised by Manicas.

1. Methodological diversity

Manicas questions the validity of my argument that methodological diversity characterises science. He is not convinced about my rejection of the naturalist search for a single scientific method across the social and the natural sciences. Now, it is worth emphasising I did not write that scientific activities have absolutely nothing in common. Rather, my position is that the various sciences have nothing in common that would enable philosophers to infer meaningful prescriptions for scientists to follow. That is why I wrote, in the context of a discussion about Kuhn, that ‘... there is no neutral algorithm to science in the sense of there being no overarching set of rules (applicable to all paradigms), except for the most bland and uninformative propositions.’ (Baert 2005, p. 149) Manicas mentions two things which scientific activities have in common: ‘publicity (sic.) and consideration of events’ plus ‘acknowledgement of a stubborn reality and fallibilism’. While ‘acknowledgement of a stubborn reality and fallibilism’ are clear, it is not entirely clear what Manicas means by ‘publicity’, but I assume he is referring to the public nature of scientific debates – the extent to which disagreements (and agreements) are played out through open discussions, often involving peer-reviewed journals. And I assume he is referring to ‘consideration of events’ in the same breath because those discussions allegedly centre round the extent to which empirical evidence supports or undermines the theoretical positions involved. It is not entirely clear what ‘acknowledgement of a stubborn reality and fallibilism’ are meant to add that is not already included in ‘publicity and consideration of events’, so I will focus on the latter.

Basically, I have no qualms with the proposition that science operates according to procedures of open debate and criticism, although I would probably put it less sharply and make a number of qualifications, and I would leave out the emphasis on the ‘consideration of events’. Firstly, not all scientific activities tackle empirical phenomena in the way in which Manicas puts it. A significant amount of scientific work deals with problems of a more theoretical kind – for instance, mathematical model-building in astrophysics or economics – for which ‘consideration of events’ is simply not an issue. Secondly, even leaving aside those purely theoretical endeavours, scientific debates are not limited to the plausibility of the empirical claims that are being made, but also to a wide range of other criteria: for example, the relevance, sophistication, clarity and depth of the arguments involved. Thirdly, while most modern scientists might subscribe to the principles of Habermas’ notion of communicative rationality, their actual practices often fall short of it, ignoring the arguments presented. One of the significant implications of Kuhn and Lakatos’ work is that it acknowledged that scientists often fail to take on board obvious counter-evidence, and furthermore that they might have very good reasons for doing so because empirical evidence is theory-laden and therefore fallible. Sometimes, the deviation from the Habermasian ideal has positive effects: the ‘irrational’ disregard for empirical evidence can be crucial for the emergence and preservation of new, innovative research programmes – as Paul Feyerabend (1975) pointed out.

However, my main argument against Manicas is not that modern science is not characterised by procedures of open debate (because at some level it clearly is, or at least it is to a far greater extent than it used to be), but that he thinks it matters. As I mentioned earlier, my position against naturalism rests on the view that there is so little commonality even between the natural sciences that we cannot infer meaningful prescriptions from that to what they supposedly share. Manicas' reference to 'publicity' proves my point. It is so broad that no clear methodological guidelines can be inferred from this commonality. Even if, as Manicas thinks it is, the elusive 'consideration of events' is applicable to all scientific activities and could be added to the list of common features – which it is not (supra) – it would not help us any further in developing precise methodological guidelines. That is why I wrote that naturalism can only lead to 'the most bland and uninformative propositions'.

2. Pluralism and its benefits

Manicas takes issue with the way in which I argue that the social sciences gain from methodological pluralism. I find his critique confusing. Firstly, he is not satisfied with my use of the notion of cognitive interests in this context and argues that it is not clear what it means. It is difficult to see what Manicas is getting at as I make it quite clear in the text what this notion means. I clarify that cognitive interests include explanation (possibly prediction), understanding, emancipation (and critique in the sense of critical theory) and self-understanding (see, for example, Baert 2005, p. 150). Secondly, Manicas argues that this notion of cognitive interests '...smacks more of Habermas than any pragmatist...' However, I acknowledge Habermas in the text (see, for example, Baert 2005, p. 150) so fail to see what the problem is with this. If Manicas is hinting again at the fact that I am not as pure a pragmatist as he expected, then it is probably worth mentioning that Habermas' notion of cognitive interests was influenced by Peirce (not just a pragmatist, but one of the old guard of which Manicas approves), something which Habermas himself acknowledges at length (Habermas 1987; see also Aboulafia, Bookman and Kemp 2002). Thirdly, Manicas asks whether my argument regarding cognitive interests is an empirical or a normative claim, but he fails to clarify what is meant by this distinction so it is difficult for me to deal with this critique. Fourthly, Manicas follows this up by accusing me of views which are not mine but Rorty's. *Pace* Manicas, I never argued that the value-free vocabulary of naturalist approaches will suffice if prediction and control are the goals of inquiry. Nor did I assert that anti-naturalist approaches will be appropriate if we are concerned with moral issues. These ideas are not mine but Rorty's, and Manicas erroneously takes my discussion of Rorty's views as an endorsement of them, this is in spite of my critical comments on Rorty throughout the text, including my criticisms of passages in Rorty that are directly relevant to the specific topic which Manicas raises here. For example, I argue against Rorty's position by claiming that '... it is possible that a certain methodological path might lead the way towards a particular objective in one field of inquiry but not in another. It is equally possible that the nature of a particular object of study excludes the possibility of obtaining a particular objective altogether.' (Baert 2005, p. 154)

Manicas wonders whether methodological pluralism implies that I tolerate 'realist goals'. I thought my chapter on realist philosophy of social science, to which Manicas in a rare moment of generosity refers as 'generally sound', should have made clear the extent to which I disagree with this perspective (Baert 2005, pp. 87-105) Manicas remains silent about my main criticisms of this perspective, failing to seize this opportunity to defend his views. My arguments are set out in the book so I will not repeat myself at length here, except to say that one of my central arguments against realism relies on the own notion of open systems. My point is that you cannot at the same time presuppose the openness of systems and make methodological prescriptions of a realist kind. If systems are open in the way in which realists think they are (and they have a strong case for this), then social researchers cannot be expected to gain access to the deeper level in the way in which realists claim they are, for references to the empirical realm would be potentially contaminated by the inferences of various mechanisms or powers (see, for example, Baert 2005, p. 102).

3. Dewey

Manicas disagrees with my use of Dewey's rejection of the spectator theory of knowledge. He contends that I draw on Rorty's reconstruction of Dewey's account and that this reconstruction is flawed. It is difficult to see what precisely Manicas' point is in relation to my own argument as his critical comments apply to Rorty and I never intended to say anything substantial about Dewey. Also, Manicas does not

properly engage with Rorty but keeps on hammering the point that Rorty is not a loyal follower of Dewey as if that is really important.

Even leaving these reservations aside, Manicas' comments on Dewey and Rorty are confusing. For instance, he writes that '... Rorty (...) wants to insist that the product of inquiry cannot be a representation of a reality even though, for Dewey, it is the hard won product of a controlled experiment: the paradigm of an active effort to produce knowledge ...', thereby wrongly assuming that the two positions are necessarily mutually exclusive. In this context, I am surprised that Manicas speaks so positively about Dewey's rejection of the spectator theory of knowledge, obviously not realising that it does not square with his own realist proposal. For Dewey, this problematic but widely held theory states that '... the true and valid object of knowledge is that which has being prior to and independent of the operations of knowing. They spring from the doctrine that knowledge is a grasp and beholding of reality without anything being done to modify its antecedent state – the doctrine which is the source of the separation of knowledge from practical activity.' (Dewey 1960, p. 196) Dewey is not renowned for the elegance of his writing style but the meaning of the passage is perfectly clear, and although there is lack of clarity in his own proposal as to whether he believes that inquiry engenders knowledge of objects or the objects of knowledge themselves, it should be equally obvious that contemporary realism, at least in the way in which Manicas develops the argument, is an almost quintessential example of the spectator theory of knowledge (and its attendant correspondence theory of truth) that Dewey rejects.

It remains unclear what ought to be gained from Manicas' cursory references to the alleged links between post-modern writers and Rorty's pragmatism, especially given the vague nature of the label 'post-modernism' (notably the lack of clarity as to whom is included) and given that Rorty has made his own position (and the differences) quite clear in relation to a host of authors who might be labelled as such. Manicas exhibits the unfortunate tendency, which you also find in Bhaskar, to use categories or labels (such as 'positivism', 'empiricism', 'textualism' and 'post-modernism') as ways of vilifying other authors, thereby not properly engaging with the subtle differences buried under each label.

4. Theory preference

Manicas seems sympathetic towards my view that social research ought to be conceived in terms of a conversation, but wonders '... how this bears on the question of addressing deep differences in frameworks, between say Giddens and Parsons, or rational choice theorists and critical theorists.' He asks whether it follows from my arguments that '... one can *only* (my italics, P.B.) be tolerant of differences since there is no true, correct, valid, warranted, justified answer to the questions of epistemology and ontology.'

Unlike other philosophies of social science such as falsificationism or realism, my proposal is neutral *vis-à-vis* theory choice in so far as it refuses to invoke external criteria - such as falsifiability, explanatory power or predictive success - to decide on the value of a given theory. Instead, it suggests that we should take into account the context of the dominant presuppositions of the discipline or indeed of a community at large before evaluating the theory under consideration because it is only against this background that such an evaluation can be achieved. Alfred Schutz's notion of 'stock of knowledge at hand' is particularly applicable here because it captures very well how, in their everyday life, people approach the social world in terms of 'familiarity and pre-acquaintanceship' (Schutz and Luckmann 1973). Just as everyday life is embedded in the *Lebenswelt* - a world of everyday life governed by the 'natural attitude' - social researchers take for granted a number of theoretical and metaphysical beliefs and methodological strategies. It follows from the above that theories ought to be evaluated on the basis of how much of a *Gestalt*-switch they manage to bring about - how much they could bring researchers to rethink those hitherto deeply entrenched and often unacknowledged presuppositions. In opposition to the ritualistic hero-worship, which is so endemic in the social sciences today and which is tied in with the representational model of social researcher, the pragmatist-inspired perspective calls for less deference and bolder claims – an intellectual iconoclasm of sorts. The question should no longer be how we can apply the works of our intellectual heroes or preferred models (whatever they are) to the empirical data, but how we can learn from the encounter with the unfamiliar to challenge them and think differently. This explains why I am particularly critical of Manicas' use of structuration theory because it fails to challenge, and actually confirms and reinforces, a particular orthodoxy within the discipline.

5. *Verstehen* and prediction

Manicas takes issue with my argument that there is nothing intrinsic to the social that compels the use of a particular method as such. He accepts that ‘...assuming different goals, strictly speaking, this seems correct: If (sic) prediction and control are our aims, perhaps we can think of human action in the same way we think of the movement of planets.’ However, he then immediately follows this with a confusing sentence: ‘But indeed, this seems like a *reduction* (sic) *ad absurdum* of the ontology!’ It is unclear what he means precisely by ‘*reduction* (sic) *ad absurdum* of the ontology’ in this context (‘*reductio ad absurdum* of something’ does not exist in the English language), so it is difficult for me to deal with this criticism.

Then follows what seems to be rejection of the position with which he previously agreed: ‘... as Weber insisted, even if a goal of science is to ‘predict’ action, one needs *verstehen* (sic) to do this.’ Now, leaving aside the issue whether Weber would agree with this in the way in which it is put, I disagree with Manicas’ position that *Verstehen* (or ‘interpretative understanding’ as it is sometimes called in English) is a necessary condition for prediction. Three points will clarify the difference between my position and his. Firstly, it is surprising to read Manicas’ discussion of how prediction relies on *Verstehen* given that he spent large sections in his own book arguing that prediction is not possible in the social sciences. Why bother with which methods lead to prediction if you hold, as Manicas does, that prediction cannot be achieved in the first place? Secondly, contrary to Manicas, I believe that social research can establish predictions, though predictions that are always limited by the parameters set by the research design. For instance, research might enable us to make probabilistic predictions about how children of various social categories fare in a given educational system during a certain period, but it is highly unlikely that those findings are applicable to different educational settings or during different periods. Thirdly, *Verstehen* is not a *sine qua non* for limited predictions of this kind. It is perfectly possible to make predictions about, for example, working class children’s educational achievements in contemporary Britain based on the statistical data that are available to us, without any interference of *Verstehen*, at least in the way in which Weber defined the term. Likewise, there exist epidemiological and econometric techniques which establish associations, leading to limited predictions, without any of the causal theorising implied by *Verstehen*.

6. Emancipation

Manicas seems to have no quarrel with my notion of the emancipating effect of self-understanding, but argues that ‘... understanding the causal conditions and consequences of action is also emancipating’. I feel that, in practice, he and I are less far apart on this score than he suggests. For instance, my account of Foucault’s genealogy, as outlined in *Philosophy of the Social Sciences: Towards Pragmatism* (Baert 2005, pp. 163-165), is perfectly compatible with Manicas’ view that an essential task of social research is to understand why we have come to believe what we believe and why those beliefs are no longer tenable in the light of the research conducted.

I would like to conclude by thanking Peter Manicas again for his constructive criticisms, which have helped me to strengthen and clarify the arguments set out in my book.

Peter Manicas Rejoins:

I suspect that after two or three more exchanges, the *mis*understanding between Baert and myself would probably increase. He surely puts words in my mouth. For example, I did not say that Durkheim was a bad choice for his book because he had little influence. (See note 5); I did not say that social science could not make predictions: any good correlation gives one predictability. But understanding comes only with a knowledge of the causal mechanism. And he chooses to ignore much of what I do say—several times. Thus, despite my repeated efforts to the contrary, Baert insists I am a very old-fashioned naïve realist who holds to the correspondence theory of truth. (He got me on one typo, adding an ‘n’ to ‘reductio’) Perhaps I am equally guilty of distortion. I confess that I took it that his point of departure was a Rorty-like view of the limits of philosophy as regards questions of both ‘true’ and ‘real’. Now I am even less clear on where he stands on this. Regarding pragmatism, while Peirce, James and Dewey, as pragmatists, could accept a plurality of goals for science, it is easy to show that all would insist that success in any possible goal, comes with understanding: the effort to make intelligible the (real!) world. (See my note 7, above.) But perhaps we are closer to one another than it at least seems to me. In any case, I appreciate all the trouble that he took with my texts.

Unreal Promises: On the Limits of Realist Philosophy of Social Science.

Patrick Baert

A Realist Philosophy of Social Science; Explanation and Understanding. By Peter Manicas. Cambridge: Cambridge University Press, 2006. 336 pp. 0521678582 paperback, £15.99

Two intellectual conflicts must be declared at the outset. Firstly, as will become clear in what follows, I am not sympathetic to the tradition in which Peter Manicas' work is situated, and am inevitably sensitive to the underlying issues that he does not address. Although Manicas' *Realist Philosophy of Social Science* (2006) is a work of quality, and its line of argument probably amongst the better executed within the realist point of view, I believe that Manicas, like other realists, operates within a problematic intellectual framework, not least because it fails to take on board major developments in contemporary philosophy. Secondly, I am not well disposed to journals, like this one, who are singularly devoted to the promotion and development of a particular doctrine because it leads to pointless navel-gazing and the reinforcement of entrenched positions and orthodoxies. If there is something to be retained from Popper's position, it is probably the suggestion that it is more fruitful to open oneself up to the critical glare of those who are sceptical of one's position rather than talk incessantly to the likeminded. This is certainly one of the central messages which I have tried to get across in my *Philosophy of the Social Sciences: Towards Pragmatism* (2005) and related articles: social research can only gain from an ongoing dialogue with different forms of life and different perspectives so that our deep-rooted presuppositions and *idées fixes* are challenged and overturned. In this context, it was encouraging (if slightly surprising) to be invited by this journal to comment on Manicas' book. Given my outspoken views against realist philosophy of social science, the invitation shows intellectual openness and a willingness to engage with other viewpoints. This engagement is absolutely crucial for the future of this school of thought because quite a few of its disciples (including, as I will try to show, Manicas himself) have so far failed to take on board significant theoretical and philosophical developments since the late 1980s.

Two decades ago, Peter Manicas made a name for himself with his *A History and Philosophy of the Social Sciences* (1987). In some respects, the book was wrongly titled because it was not a textbook or a general historical introduction but a monograph that aimed to establish the philosophical foundations of a realist perspective on the social sciences. The book followed neatly the realist programme, as established by Rom Harré, Roy Bhaskar, John Urry and Russell Keat, but Manicas managed to put a distinctly historical gloss onto the doctrine, depicting it within the context of the history of philosophy of science. In Manicas' picture, the social sciences have been working for a long time within a rudimentary positivist-cum-empiricist agenda, hence the relevance of the realist alternative presented. This view has now become commonplace amongst those who ally themselves with the critical realist position, confirming their siege mentality and the conviction that they are surrounded by hardcore empiricists and that they are the only ones remotely capable of fighting the fortress of neo-positivism.

Manicas' *Realist Philosophy of Social Science* continues along the same line with the same heroes and villains. Even Rom Harré's blurb for the back cover reiterates in evangelical fashion the now well known realist argument that blatant empiricism has been rampant amongst the social sciences and that realism is their saviour, ('One can only hope that, at last, the spectre of naïve empiricism will finally be exorcised from the social sciences.') and, indeed, this position also runs throughout the book. However, in comparison with *A History and Philosophy of the Social Sciences*, *Realist Philosophy of Social Science* brings an analytical edge to the debate, carefully outlining how realism, at least in Manicas' version, makes a difference to philosophy and social research. For those familiar with the standard realist argument, this text does not have many surprises in store. Manicas follows a well-trodden path and his general arguments have a familiar ring to them. However, on the positive side, he presents them with diligence and seriousness, which keeps readers on their toes and makes the chapters perfectly sound and *prima facie* coherent. Manicas' writing style is clear and to the point, avoiding the unnecessary jargon, neologisms, acronyms and initialisms which often mark Bhaskar's and Archer's writings. Manicas also shows the links with empirical research, giving the book particular usefulness as a reference book for students, not to mention relevance for practising social scientists. Manicas draws on a wide variety of empirical sources, demonstrating a certain breadth of understanding and interests. As far as realist philosophy of social science is concerned, this is a coherent and comprehensive defence.

The book starts with the well-known realist critique of Hempel's nomological-deductive model, arguing that science aims at understanding of general processes of nature - not explanation of concrete events - and that any attempt at proper understanding requires references to causal mechanisms conceived in terms of productive powers. Following Giddens' double hermeneutic, Manicas argues that the social sciences differ from the natural sciences in that they deal with an already pre-interpreted realm to such an extent that understanding the social realm necessarily entails an element of *Verstehen*: the re-enactment of the purposes, concerns and accounts of the people involved. But people might be mistaken about how they perceive the situation in which they find themselves, so an additional task of the social sciences is to point out those errors, and this is their emancipatory dimension. Another difference from the natural sciences is that the generative mechanisms in the social sciences are always historically situated, and whenever the social sciences seek to explain concrete events, they need to refer to the historical context in which those events are situated. Manicas applies this realist model to economics and criticises neo-classical economics, contending that it draws on an outdated and erroneous positivist epistemology and problematic notion of the rational actor.

The problem with this book is not necessarily the way in which Manicas executes the realist position; it lies at the core of the intellectual enterprise itself. I have elsewhere mentioned the problems of the realist programme (Baert 1996; 1998: ch7, 2005: ch8). In what follows I will limit myself to a discussion of Manicas' position, though some of my points of criticism also apply to other self-styled realists.

1. One of the main criteria for judging a work is whether it is an innovative contribution to the field. Does it bring something new to the intellectual community – does it have surprises in store for us? Does it enable us to see things differently – does it question our deep-seated assumptions? Manicas' proposal for a realist philosophy of social science fails on this score. When the likes of Harré, Bhaskar and Keat and Urry introduced their realist social science thirty years ago, it was new and it provided a welcome alternative to the hegemony of positivist-inclined research, especially in fields like social psychology (Bhaskar 1978, 1979; Harré 1961, 1964; Harré and Secord 1972; Harré and Madden 1975; Keat and Urry 1982). One might have disagreed with their views, but they were novel in their outlook and in their practical propositions for how to conduct social research. Manicas' *A Realist Philosophy of Social Science* is not so obviously innovative. It goes through the usual realist motions, from the rejection of the so-called Humean concept of causality (the usual realist trademark) to the now-tired attempt to transcend the opposition between agency and structure. Manicas' views on social theory are to a large extent those of Giddens' structuration theory (Manicas 2006, pp. 42-102) – now twenty five years old. Even Manicas' reference to Willis' *Learning to Labour* as a prime example of structuration theory, can be found in Giddens' own *Constitution of Society* (see Giddens 1984). Manicas presents these views about agency and society (including Giddens' notion of the double hermeneutic) as if they are ground-breaking contributions to the field, but this is no longer the case. They are the unquestioned views of a group of self-proclaimed realists and have been basically for the past three decades. Likewise, his criticisms of neoclassical economics (Manicas 2006, pp. 126-150) do not differ substantially from those developed by heterodox economists like Tony Lawson and other realists, though at least those were introduced more than a decade later than structuration theory and they still constitute a challenge to the dominant views.

2. Even leaving aside the vintage of the theories selected, they are also problematic for other reasons. Structuration theory again provides an example. One of the problems with this theory is that it presents a series of obvious statements about the social world: structures are a precondition for and outcome of agency; structures are not merely constraining but also enabling; people know a great deal about social life though not always explicitly; etc. Few would disagree with the core features of structuration theory because they are undisputed and widespread truths about the social world, clothed in nebulous academic language (see also Baert 1998, ch.4). It is not at all clear that this series of self-evident propositions about the workings of the social world are the way forward for the social sciences. Indeed, social scientists have moved away from structuration theory partly because the actual empirical pay-off (i.e. the benefits for empirical research) appears limited. Furthermore, as I try to explain in *Philosophy of the Social Sciences: Towards Pragmatism* and other writings, the question is not which theory would be superior right from the start, but how the encounter with a subject matter allows us to learn, think differently and move forward. It is absolutely essential for social research to enable researchers to think differently about the social world, i.e. to question our deep-seated and unacknowledged presuppositions. Enquiry ought to be sufficiently

open-ended to allow this questioning to take place effectively. One of the problems with current social research is that, for various institutional reasons, researchers are committed to a particular theoretical framework from the outset and then routinely apply it to the empirical case which they study. Of course, research is always a located activity – it always occurs within a particular theoretical context - but there is a significant difference between the ritual act of imposing a particular framework onto the empirical subject matter, and the openness which I am advocating. This openness requires researchers to assess their presuppositions and re-evaluate (not just corroborate) their theoretical and methodological standpoint in the light of the confrontation with difference. In my view, theories ought to be evaluated on the basis of how much of a *Gestalt*-switch they bring about - how much they allow researchers to rethink those hitherto deeply entrenched presuppositions. In opposition to the ritualistic hero-worship or framework-worship, which is so endemic in the social sciences and which is tied in with a representational model of social research, my pragmatist-inspired perspective calls for less deference and bolder choices among various approaches. The question should no longer be how we can apply the works of our intellectual heroes or preferred models (whether they are Giddens, structuration theory or whatever) to the empirical data, but how we can learn from the encounter with alterity to challenge them and think differently.

3. It is therefore not surprising that Manicas himself comes up with an endless series of truisms. Two examples will suffice. Trying to demonstrate the intrinsic unpredictability of the social world, he comes up with the following proof: ‘... suppose that just as I am about to start this sentence, an errant throw of a baseball shatters the window in my office. The sentence I started to write does not get written.’ (Manicas 2006, p. 54) Needless to say, although it might well be true that he would not have written the sentence under those circumstances (and the reader might be forgiven for regretting the absence of an errant baseball), it is stretching a point to infer from this the unpredictability of the social. Likewise, attempting to show the relevance of his reflections on structuration, history and social change, Manicas notes that if we were to compare the ‘USSR’ (sic) and the United States, then ‘... differences in the structure of the two economies, their political arrangements and so on, made for enormous differences in the capacities and constraints of persons, from leaders, such as Putin and Bush, to ordinary workers and citizens.’ (Manicas 2006, p. 62). It is difficult to see what such a vague and uninformative observation ought to tell us something significant about the relationship between agency and structure that we did not already know.

4. It is worth noting that not all realists come up with self-evident statements. Within the realist camp, attempts have been made to link realist philosophy to an evolutionary perspective in social theory and economics. I am thinking here of the work of Bhaskar, Harré and Hodgson (Jensen and Harré 1981; Harré 1993; Hodgson 1993, 1999; 2004). It is surprising that Manicas doesn’t pay much attention to this strand of thought because it provides more insightful and counterintuitive propositions about the social world than, say, the structuration theory which he feels so strongly about. Manicas could also have updated his views about social theory by taking on board more recent developments, including those that potentially question his views. He pays remarkably little attention to more recent debates, especially to those that show that the premises of those theories which he promotes have been seriously challenged. For instance, Manicas happily identifies people as primary causal agents (Manicas 2006, page 75), thereby ignoring the debate, initiated by actor-network theorists, about the causal efficacy of non-human agents and the implications of this for social theory. I am not necessarily advocating Bruno Latour and Donna Haraway’s position but you cannot simply ignore their work (e.g. Haraway 1991; Latour 2007). The ‘post-human turn’ has permeated very different fields, from archaeology to science studies, and it is surprising that Manicas does not take this new movement on board, especially given its repercussions for his own views.

5. His own criticisms of existing theories are rather thin and sometimes misguided. For instance, his critique of rational choice theory (Manicas 2006, p. 55) is superficial and based on a misunderstanding of the subtleties of the arguments involved, erroneously conflating rational choice theory with decision theory, and mistakenly assuming that rational choice theorists take people always to act rationally. There is no further discussion of the various subtle versions of rational choice theory, including for instance Elster’s or Sen’s. Likewise, Manicas’ critique of Harré’s sophisticated attempt to avoid naïve realism is devoid of any substantial argument, simply arguing that, for instance, the category of ‘class’ is ‘... a theoretical term which, by abstraction, might well explain a structured pattern of practice. More generally, sceptical of a slip toward Durkheim, Harré has been reluctant to extend his realism to institutional facts. His account, accordingly, is vulnerable to a materialist critique of the sort mounted by Porpora and Archer.’ (Manicas

2006, p. 73) There is no attempt to delve further into the precise nature of this 'materialist critique', let alone into why it ought to be so devastating for Harré's position.

6. Like other realists, Manicas assumes that the social sciences are immersed in a dark cloud of neo-positivism and empiricism (and therefore in need of serious salvation.) This simply isn't true. A substantial amount of literature in the social sciences operates roughly within a realist agenda, with all the problems that entails. Take even Emile Durkheim, who has often been misrepresented as a positivist or empiricist, and not just by realists. (Incidentally, Durkheim himself rejected the positivist label because of the associations with the disreputable Auguste Comte.) Leaving aside the issue that positivism is a slippery term with a variety of meanings, Durkheim doesn't match the positivist (or empiricist, for that matter) view of science in any sense of the term. Even his earlier writings do not draw on a Humean regularity notion of causality but search for underlying generative mechanisms that account for the empirical data (see Baert 2005: ch1). The problem he faces is a different one: because of the wide range of mechanisms that interact with one another, his empirical findings are not, and can never be, a sufficient basis from which to infer or test with a high degree of reliability claims about causal powers (more of this later). Likewise, it is not surprising that Manicas finds it difficult to identify contemporary examples of the 'Humean' research which he abhors. Most contemporary researchers follow knowingly or unwittingly the realist orthodoxy, searching for underlying mechanisms that might account for the 'demi-regularities' which operate at the surface level. No wonder he gives us an endless list of researchers who do so, from Charles Tilly to Erving Goffman. Manicas' realist philosophy of social science does not provide the prescriptive value for social research that he thinks it does because most social scientists already operate on this basis and have been doing so for a long time.

7. Like other naturalist philosophers of science, Manicas believes that he is able to identify *the* method of scientific inquiry, thereby ignoring implications of the vast material from the history and sociology of science which shows the huge methodological variety in the natural and indeed in the social sciences. This is in spite of Manicas' awareness of these developments (as can be gleaned from Manicas 2006, page 30, footnote 6). There is no neutral algorithm that unites the natural sciences, let alone the natural and social sciences. Different branches of science do things differently, and there are also cultural divergences, not to mention the different practices between laboratories. Peer review limits but does not overcome these differences, and in some cases may even generate rivalry that reinforces them. To search for the golden key that would unlock the magic door of scientific understanding is a wild goose chase. Given the huge variety within the natural sciences, the project of a realist-based naturalism seems ill-founded as well as absurdly ambitious.

8. In the light of the immense diversity within the social sciences, it is surprising that Manicas assumes that '... understanding is the primary goal of the sciences.' (Manicas 2006, p. 2). As I have explained elsewhere (Baert 2005: ch 6), a pragmatist angle conceives of knowledge acquisition as tied to a variety of cognitive interests – including understanding but also description, explanation, emancipation and self-understanding - and there is no good reason to attribute epistemological priority to one of them. Related, it is not entirely clear what is to be gained from distinguishing, as Manicas does, understanding from explaining, especially given that the meaning he attributes to understanding differs substantially from its meaning in most social science and philosophical literature. (For Manicas, understanding refers to the process of making sense of general mechanisms, whereas explanation ties in with the accounting of concrete events.) It is perfectly possible to redefine concepts in philosophy but only if the pay-off is clear. It isn't here. Inevitably, when he introduces the same terms – 'understanding' and 'explanation' - in the context of the Methodenstreit, they acquire a very different meaning.

9. It is difficult to see how the notion of openness of social systems can be squared with the belief that empirical research will allow us to gain a thorough and secure understanding of the precise nature of the generative mechanisms. It seems to me that Manicas and other realists want to have their epistemological cake and to eat it. Their aim is to combat the regularity notion of causality on the grounds that the various mechanisms might interfere with one another (so that the empirical realm cannot be trusted in the way in which positivists do), whilst assuming that one can use empirical evidence (e.g. his discussion of 'demi-regularities': Manicas 2006, pp. 97ff.) to gain access to the generative mechanisms. You cannot do both without inconsistency. In realist parlance, once you acknowledge the discrepancy between the 'actual' and

the 'deep', then you must also question the use of empirical regularities to infer or test statements about the deep. If realists want to hold on to their notion of openness of systems in the way they do, then they ought to acknowledge the problematic nature of any use of empirical research as a reliable basis for claims about the causal power of underlying generative mechanisms (see also Baert 2006, ch. 4).

10. Manicas seems to disregard major developments in analytical philosophy over the last half a century when he writes that theories '... are meant to represent reality, as it is in-itself.' (p. 27) Equally surprisingly, after conceding that '... it is quite impossible to 'observe' anything independently of some conceptual frame of reference ...', he continues with the assertion that '... this does not undermine the quest for a true representation of reality'. (p. 43) Given the consensus within current analytical and continental philosophy that naïve realism of this type is no longer tenable, surely the onus is on Manicas to elaborate on why he takes this position. There is a vast literature on the philosophical problem of reality which Manicas decides to ignore, thereby leaving the reader wondering which type of realism he embraces, and what he means precisely by 'reality' and 'true representations'. Interestingly, in this context, he makes sympathetic nods to philosophical views which are at odds with his – 'We are all situated and there is no God's-eye view of the world' (Manicas 2006, p. 65), without realising that is what they are. Long before the likes of Putnam (1981, 1983) and Rorty (1980, 1991) showed the problematic nature of naïve realism of this kind, Dewey (1930) coined the term 'spectator theory of knowledge' to refer to the fallacious notion that knowledge somehow captures or mirrors the inner essence of the external realm. Interestingly, Manicas acknowledges Dewey's position (second-hand via Bourdieu and Wacquant) and refers to it approvingly (p. 65), but he fails to realise its radical consequences and the extent to which it undermines his own position. Like Dewey, I am not arguing for a relativist position if by this is meant that there are no criteria to judge and compare between different theories. Rather, my point is that there is no neutral algorithm that provides the basis for those criteria. It is perfectly feasible to argue that some social theories are better than others in various ways, but not because they mysteriously 'match' reality, capture or approximate the 'truth'. If they are regarded as better, it is because they are seen as more successful in accomplishing various objectives, and that decision, together with the decision as to the importance of those objectives, are meant to be the outcome of an open discussion amongst social scientists.

Peter Manicas Replies: True and Truisms

I was certainly not surprised that Patrick Baert and I differ so fundamentally. I am, however, a bit disappointed to see that, from his point of view, my book disregarded 'major developments in analytical philosophy over the last half a century,' and mostly offered 'an endless series of truisms'. These charges hurt. My book did not, to be sure, discuss the many arguments from proponents of the many varieties of anti-realism — and of realism. Doing this would result in a very different book. Instead, I took a position in the philosophy of science, my version of Harré/Bhaskar — a version which might satisfy neither. My particular contribution, if there be such, was three-fold: First, I tried to develop the idea that achieving understanding via appeal to causal mechanisms is the major goal of the successful sciences—an empirical, not normative, claim, but a position rarely considered in the current philosophical literature. I was anxious to show that explaining events was a very different kettle of fish than achieving understanding. I was anxious also to show that understanding in this sense should be the primary goal of social science and that, if so, one needs an account of social mechanisms which acknowledges the deep differences between the natural and human sciences. While the idea of social mechanism is sometimes suggested by social scientists, often incoherently joined to deductivist ideas about explanation (as demonstrated by my account of the debate between Somers and Hector, Appendix C), to my knowledge, there is no systematic account which draws on pertinent issues regarding causality and, in particular, to the question of agency/structure, a point to which I return. To be sure, there is a good deal of good social science. I tried to show why it was good. Finally, drawing on a realist analysis of causality, I made an effort to resolve 'nurture/nature' debates, to identify the misuse of quantitative methods and to resolve several of the long standing debates in historical sociology between very major social scientists.

Baert finds this discussion boring and not particularly new or exciting. What can I say? But as he also generously notes, since 'the writing style is clear and to the point', the book is a 'coherent and comprehensive defense' and 'shows the links to empirical research', it may be useful to students and of 'relevance for practicing social scientists' - that, of course, was my hope. The problem, for Baert, then, would seem to be that any realist account is unnecessary and doomed, unnecessary because according to

him, practicing social scientists are all already guided by a coherent realist philosophy, and, curiously perhaps, doomed, because realist philosophy of social science is philosophically untenable.

There are two specific relevant criticisms which deserve some comment. I certainly did not pretend that my account of agency/structure was 'ground-breaking'. And it is just not true that realist views on this matter 'have been basically unchanged for three decades'. First, there is no one 'realist' view and second, the differences, which regard the causal status of social structure, are very important. If I am correct, the uncritical use of the concept, by Baert and much prevailing social science, assumes an untenable conception of causality and tempts a regrettable lapse into determinism. The post-modernist alternative (and some readings of ethnomethodology, rational choice theory and so forth) is to suppose that since the social world is our product and is all 'in our heads', we are unconstrained in our 'making' of it, and in acting in it. I am not clear where Baert stands on these issues.

Finally, I am accused of incoherence because I hold (with the consensus) that we lack a God's eye view of the world and that theory seeks to represent reality 'as it is in-itself'. Putting aside questions about what Bhaskar (1978: 36) called 'the epistemic fallacy', it is not clear whether Baert is (1) denying that there is a causally efficacious independently existing natural world or (2) that we just cannot know it. Realists assume that there is an independently existing reality, and that, constrained though we are — by our biology, our histories and by the character of that reality—our truth claims are about 'it'. Perhaps, for Baert, the effort to establish such claims is a misconceived ideal? Perhaps, as in empirical and naïve realisms (including pragmatist versions), what bothers Baert is the claim that entities postulated by theory exist and have the causal powers attributed to them by theory? These are, of course, representations, and to be sure, we can never be sure that our representations of reality are true. But it is sufficient for the purposes of Baert's charge of incoherence that we can have good reasons to believe that our theory is true and that it does adequately represent reality. For present purposes, 'good reasons' can be unpacked in a number of ways, including sensible readings of Kuhn, Peircean arguments regarding abduction, pragmatic arguments, including Harré's policy realism and Dewey-type arguments as developed by Hacking's ideas on 'intervening'. Of course, confidence in a representation does not come easily and is not final. Hacking writes: 'In physics and much other interesting conversation we make representations — pictures in words if you like. In physics we do this by elaborate systems of modelling, structuring, theorizing, calculating, approximating. These are real, articulated, representations of how the world is' (Hacking, 1983: 145). But 'in physics there is no final truth of the matter, only a barrage of more or less instructive representations' (ibid.). A fallibilism is not a skepticism.

In this regard, Baert argues that the notion of openness of systems 'cannot be squared with the belief that empirical research will allow us to gain a thorough and secure understanding of the precise nature of the generative mechanisms.' I did not claim—who has?—that empirical research can give us 'a thorough and secure understanding of the precise nature of generative mechanisms'. We must settle for much less, suggesting here that his position is driven by what Dewey termed 'the quest for certainty'. On the other hand, he also offers that some theories are better than others, but not because they 'match' reality but because they are 'more successful in accomplishing various objectives'. 'Representing', to be sure, is not 'matching' and does not require that we compare theory to reality. I offered arguments against instrumentalist ideas of theory which concluded that success in understanding requires that theories be true. This doesn't matter, of course, if the goal is 'prediction', as unsophisticated pragmatists have it. Indeed, his pluralism regarding the goals of science leads him not merely to reject attributing 'epistemological priority' to any of them, but to deny, by implication at least, the most obvious achievement of the successful sciences, namely, their capacity to provide us a modest understanding of our world, an understanding which makes intelligible the patterns of life, makes possible the explanation of events, and allows us to build technologies and to intervene in the effort to achieve our goals, too often, perhaps, misconceived.

It is one of the misfortunes of the social sciences that they have been burdened by bad philosophy leading to spurious assumptions about their methods, goals and possibilities (Manicas, 1989). Baert and I seem to share in this, even if we disagree on the problem and the remedy.

Patrick Baert Replies: Realism Revisited; A Reply to Peter Manicas' Reply.

I would like to thank Peter Manicas for giving proper attention to my comments and for replying to them with the seriousness and thoroughness that is so typical of his writing. I will take his various points one by one.

1. I did not intend to come across as dismissive of Peter Manicas' views, and I am genuinely sorry that he found my criticisms as hurtful as he did. However, my views, and my whole outlook, are so different from his that it is difficult for me to state my disagreements clearly without putting them in the strongest possible terms. In my review, I tried to be as direct and honest as possible, even if this meant ruffling a few feathers. But I never intended to be disrespectful towards Manicas. As I made clear throughout my review, there is a substantial amount of value to Manicas' book as long as one is willing to accept the realist framework. The book is rigorously argued and comprehensive in scope; and the main problems lie with its realist premises (see also Baert 2005: 87-105). But I cannot ignore that there is also a real problem with the fact that the book lacks a substantial new argument. Manicas lists his notion of understanding as an innovative contribution, but it is not clear to me how this notion differs from, say, Harré's account of scientific processes. I am also surprised that Manicas believes that '... there is no systematic account which draws on pertinent issues regarding causality and, in particular, to the question of agency/structure.' Surely, Manicas knows that a huge amount of realist literature has dealt with this, and that indeed a substantial number of sociologists have tackled the agency-structure debate *ad nauseam* (e.g. Archer 1995; Bourdieu 1977; Bhaskar 1998; Giddens 1984). Those debates are quite old and the positions entrenched. This might not cause too much anxiety for Manicas, but from my perspective, which emphasises the need for conceptual renewal and the questioning of our deep-seated presuppositions, social research does not benefit from the rehashing of old themes.

2. In defence of his position, Manicas writes that '... the uncritical use of the concept (of social structure, P.B.), by Baert and much prevailing social science, assumes an untenable conception of causality and tempts a regrettable lapse into determinism.' I am not sure how much current social science falls under this 'prevailing social science', but I doubt that it is as much as Manicas seems to suggest. More importantly, it is a total mystery to me why he puts me under this umbrella. What did I write about social structure to warrant this absurd allegation? Where is my 'regrettable lapse into determinism'? If anything, my early writings on the significance of G.H. Mead's concept of time reveal a concerted effort to steer away from any determinism (e.g. Baert 1992a, 1992b). One of my arguments was that people have the ability to reflect on hitherto unquestioned structures, and that in particular collective forms of reflection can be an important source for structural change.

3. From Bhaskar and Archer, Manicas has inherited the unfortunate tendency to present stereotypes of a number of theoretical schools and to fit them into various dichotomies. It is in this context that he puts 'the postmodernist alternative' and 'some readings' of ethnomethodology and rational choice theory under one banner: they allegedly depict the social world as all 'in people's heads', and people's actions are seen as 'unconstrained'. Such claims lead to a number of problems, which I shall examine in turn.

Firstly, the way Manicas makes his point is confusing. When he describes those theories as conceiving of the social world as all 'in people's heads', the question arises as to which people he is referring to – the people who are being studied, the researchers, or both categories? Also, it is not because the social world is very much a human construction that people's actions are necessarily 'unconstrained'. Even if the social world is taken as constructed, people's categorisations can still be conceived as constraining because they unconsciously restrict what people see, envisage and wish to do. It might even be argued that those categorisations are particularly coercive for the people involved because they are by definition unacknowledged.

Secondly, as I have mentioned before, labels such as postmodernism are a recipe for conceptual disaster as it is unclear to whom or what this term refers, especially because very few social researchers are self-declared postmodernists, and especially if, as is the case here, postmodernism remains undefined. Sometimes postmodernism is used to refer to authors, like Frederik Jameson (1991) and David Harvey (1990), who identify a new, postmodern condition, but surely Manicas cannot have them in mind when he writes about the postmodernist alternative because their work touches upon very different issues and is not necessarily incompatible with the views he himself holds. It is probably more likely that he is referring to the likes of Michel Foucault because of the way in which he and his followers emphasise the role of discourse in the making of society. If yes, then I am afraid Manicas' critique does not hold because Foucault's notion of discourse is not simply all 'in people's heads' because discourse ties in with power and structural constraints (Foucault 1969, 1980; McHoul and Grace 1997). The bottom line is that Manicas'

postmodernist is a straw man, without clear identity, and therefore an unsound basis for critique of the kind Manicas develops.

Thirdly, there is Manicas' vague reference to 'some readings' of ethnomethodology and rational choice theory. Whose readings are we talking about? I have never been an apologist of rational choice theory (see my critique in Baert 1998: ch.7) but I do not know of any form of rational choice theory that conceives of social life as 'unconstrained' in any meaningful sense of that word. To depict people's actions and choices as rational is not to conceive them as unconstrained. As Karl Popper already pointed out, people's actions and choices always take place in a given situation, which entails not only a number of opportunities, but also real constraints (Popper 1983). For rational choice theorists, it is in the face of these possibilities and constraints that choices are made (e.g. Coleman 1990). Furthermore, they also acknowledge that people's preferences do not spring out of nowhere. Our preferences are shaped by our experiences and indeed we sometimes adjust our preferences to the constraints that we face (see Elster 1985). Likewise, Manicas describes ethnomethodology as a theoretical perspective that conceives of the social as all 'in people's heads'. This is a complete misunderstanding of ethnomethodology, ignoring one of the central tenets of this theoretical school. Ethnomethodologists have always been wary of the subjectivist fallacy of which Manicas now accuses them, as can be inferred from their commitment to a practice-based philosophy. In this context, I suggest Manicas reads Heritage's authoritative book on ethnomethodology, which elaborates on the link between ethnomethodology and the later Wittgenstein (Heritage 1984; see also Garfinkel 1967). In line with this philosophy, Garfinkel and other ethnomethodologists always intended to steer away from the view that people's concepts simply create the social world that they are supposed to represent.

4. Manicas asks whether I believe in a '... causally efficacious independent natural world...' and whether I believe that '... we simply cannot know it'. To deal with the crucial first question, we need to specify which independence is at stake – independence of what or whom? I assume Manicas' question is whether I believe in an external world that is independent of the knowledge claims about that world. From those who take pragmatism seriously, this question is problematic itself because it entails a particular dualism, which they wish to avoid. The question, posed by Manicas, only makes sense if you accept the age-old dualism between knowledge and the world, or, if you prefer, between the knower and the known, or between subject and object. As Manicas should have inferred from his reading of John Dewey's works and from my own lengthy discussions about knowledge and action (Baert 2005), pragmatist-inclined philosophers are not so willing to accept this conceptual separation because they define knowledge in terms of intervention in the world. The separation between knowledge and the world ties in with the metaphor of vision according to which knowledge mirrors the essence of the external realm. Once we abandon this metaphor, we are free to conceive of knowledge in a more productive fashion – as something active, engaged with the world, rather than merely copying it. This explains why pragmatists do not wish to conceive of the world as either created or discovered, because either position assumes the dualism that they want to avoid. Consequently, the second question - namely, whether we can ever know that world - needs rephrasing as well.

5. Regarding the issue of openness of social systems, I am not hankering after the kind of certainty that Manicas suggests I am. Moreover, there is quite a difference between the lack of certainty entailed in Dewey's depiction of scientific activities on the one hand, and the lack of certainty which we encounter in realist-inspired social research on the other. The realist notion of openness of social systems does not fit with the realist idea that we can test empirically how a particular generative mechanism is supposed to affect us all, because system openness implies that an infinite number of other mechanisms are potentially interfering with the generative mechanism that we are investigating. My point was not that we cannot have certainty about the social scientific claims we make, but that once the notion of system openness is acknowledged, we can no longer rely on empirical research to test the validity of those social scientific claims. So if realists are correct in their assertion that social systems are open in the way in which they define openness, then it follows that their own view of what social research can achieve becomes untenable.

If this discussion has revealed anything, it is that Peter Manicas and I agree on very little. But at least this debate has helped to gain some clarity as to the precise nature of our disagreements, and for this, I am grateful to him.

Bibliography

- Aboulafia, Mitchell, Myra Bookman, and Catherine Kemp (eds.) (2002) *Habermas and Pragmatism*. London: Routledge.
- Alexander, Jeffrey (ed.) (1988) *Durkheimian Sociology. Cultural Sociology*. Cambridge: Cambridge University Press.
- Alexander, Jeffrey 'The Centrality of the Classics,' in Giddens, A. and Turner, J. (eds) *Social Theory Today*. Oxford: Polity Press, 1987.
- Alexander, Jeffrey and Philip Smith (eds.) (2005) *The Cambridge Companion to Durkheim*. Cambridge: Cambridge University Press.
- Archer, Margaret (1995) *Realist Social Theory; The Morphogenetic Approach*. Cambridge: Cambridge University Press.
- Babbie, Earl *The Practice of Social Research*, 10th Edition. Belmont Ca.: Thomson Wadsworth, 2004.
- Baert, Patrick (2005) *Philosophy of the Social Sciences: Towards Pragmatism*. Cambridge: Polity Press.
- Baert, Patrick (1998) *Social Theory in the Twentieth Century*. Cambridge: Polity Press.
- Baert, Patrick (1996) 'Realism as a Philosophy of Social Sciences and Economics: a Critical Evaluation'. In: *Cambridge Journal of Economics*, 20, issue 3, September, pp. 513-522.
- Baert, Patrick (1992a) *Time, Self and Social Being; Outline of a Temporalised Sociology*. Aldershot: Ashgate.
- Baert, P. (1992b) 'Time, Reflectivity and Social Action'. In: *International Sociology*, Vol. 7, 3, pp. 317-327.
- Bernstein, Richard J. (1992) *The New Constellation*. Cambridge, MA.: MIT Press
- Bernstein, Richard J. (1971) *Praxis and Action* Philadelphia: University of Pennsylvania Press,
- Bhaskar, Roy (1998) *The Possibility of Naturalism: A Philosophical Critique of the Contemporary Human Sciences*. 3rd edition. London: Routledge.
- Bhaskar, Roy (1979) *The Possibility of Naturalism: A Philosophical Critique of the Human Sciences*. Brighton: Harvester Press.
- Bhaskar, Roy (1978) *A Realist Theory of Science*. Sussex: Harvester Press.
- Bhaskar, Roy (1975) *A Realist Theory of Science*. 2nd edition. Hassocks: Harvester Press.
- Bourdieu, Pierre (1977) *Outline of a Theory of Practice*. Cambridge: Cambridge University Press.
- Burke, Tom (1994) *Dewey's New Logic: A Reply to Russell* Chicago: University of Chicago Press
- Calhoun, Craig 'Explanation in Historical Sociology: Narrative, General Theory, and Historically Specific Theory,' *American Journal of Sociology*, 104(3)1998
- Coleman, James (1990) *Foundations of Social Theory*. Cambridge, MA: Harvard University Press.
- Collins, Randall (1986) *Weberian Sociological Theory*. Cambridge: Cambridge University Press.
- Dewey, John (1930) *The Quest for Certainty; A Study of the Relationship between Knowledge and Action*. London: George Allen and Unwin Ltd.
- Elster, Jon (1985) *Sour Grapes; Studies in the Subversion of Rationality*. Cambridge: Cambridge University Press.
- Feyerabend, Paul (1975) *Against Method*. London: Verso.
- Frankfort-Nachmias, Chava and Nachmias, David *Research Methods in the Social Sciences*, 4th Edition. New York: St. Martins Press, 1992
- Foucault, Michel (1980) *Power/Knowledge; Selected Interviews and Other Writings 1972-1977*. Ed. C. Gordon. New York: Pantheon Books.
- Foucault, Michel (1969) *L'Archéologie du savoir*. Paris : Gallimard.
- Garfinkel, Harold (1967) *Studies in Ethnomethodology*. Englewood Cliffs, NJ: Prentice-Hall.
- Giddens, Anthony (1984) *The Constitution of Society; Outline of the Theory of Structuration*. Cambridge: Polity Press.
- Habermas, Jürgen (1987) *Knowledge and Human Interests*. 2nd edition. Cambridge: Polity Press.
- Hacking, Ian 'The Self-Vindication of the Laboratory Sciences,' in Pickering, Andrew (ed.), *Science as Practice and Culture*. Chicago: University of Chicago Press, 1992.
- Hacking, Ian (1983) *Representing and Intervening: Introductory Topic in the Philosophy of Natural Science*. Cambridge: Cambridge University Press.
- Haraway, Donna (1991) *Simians, Cyborgs and Women: The Reinvention of Nature*. London: Free Association.

- Harré, Rom and Edward Madden (1975) *Causal Powers: A Theory of Natural Necessity*. Oxford: Blackwell.
- Harré, Rom and Paul Secord (1972) *The Explanation of Social Behaviour*. Oxford: Basil Blackwell.
- Harré, Rom (1993) *Social Being*. 2nd edition. Oxford: Blackwell.
- Harré, Rom (1964) *Matter and Method*. London: Macmillan.
- Harré, Rom (1961) *Theories and Things*. London: Sheed & Ward.
- Harvey, David (1990) *The Condition of Postmodernity: An Inquiry into the Conditions of Cultural Change*. Oxford: Blackwell.
- Heritage, John (1984) *Garfinkel and Ethnomethodology*. Cambridge: Polity Press.
- Hickman, Larry *John Dewey's Pragmatic Philosophy*. Indianapolis: University of Indiana Press, 1990.
- Hodgson, Geoffrey (1993) *Economics and Evolution: Bringing Life back into Economics*. Cambridge: Polity Press.
- Hodgson, Geoffrey (1999) *Evolutions and Institutions: On Evolutionary Economics and the Evolution of Economics*. Cheltenham: Edward Elgar.
- Hodgson, Geoffrey (2004) *The Evolution of Institutional Economics: Agency, Structure, and Darwinism in American Institutionalism*. London: Routledge.
- Homans, George 'Behaviorism and After,' in Giddens, A. and Turner, J. (eds) *Social Theory Today*. Oxford: Polity Press, 1987.
- Jameson, Frederic (1991) *Postmodernism, or, the Logic of Late Capitalism*. Durham: Duke University Press.
- Jensen, Uffe and Rom Harré eds. (1981) *The Philosophy of Evolution*. Brighton: Harvester Press.
- Keat, Russell and John Urry (1982) *Social Theory as Science*. 2nd edition. London: Routledge & Kegan Paul.
- Latour, Bruno (2007) *Reassembling the Social: An Introduction to Actor-Network Theory*. Oxford: Oxford University Press.
- Laudan, Larry *Science and Values*. Berkeley: University of California Press, 1984.
- Margolis, Joseph *Science Without Unity*. Oxford: Basil Blackwell, 1986.
- Manicas, Peter (2006) *A Realist Philosophy of Social Science; Explanation and Understanding*. Cambridge: Cambridge University Press.
- Peter Manicas (2002) "Dewey and American Psychology," *Journal for the Theory of Social Behavior*, 32(3).
- Peter Manicas (1998) "John Dewey and American Social Science," in Hickman, Larry (ed.), *Reading Dewey: Interpretations for a Postmodern Generation (Bloomington: University of Indiana Press, 1998)*.
- Manicas, Peter T. (1989). *A History and Philosophy of the Social Sciences*. Oxford: Basil Blackwell.
- Manicas, Peter (1988) "Pragmatic Philosophy of Science and the Charge of Scientism," *Transactions of the Charles S. Peirce Society*, 24(4)
- Manicas, Peter (1987) *A History and Philosophy of the Social Sciences*. Oxford: Basil Blackwell.
- Mann, Michael (1986) *The Sources of Social Power, Volume 1: A History of Power from the Beginning to AD 1760*. Cambridge: Cambridge University Press.
- Mann, Michael (1993) *The Sources of Social Power, Volume 2: The Rises of Classes and Nation-States, 1760-1914*. Cambridge: Cambridge University Press.
- McHoul, Alec and Wendy Grace (1993) *A Foucault Primer: Discourse, Power and the Subject*. New York: New York University Press.
- Merton, Robert K. (1968) *Social Theory and Social Structure*. New York: The Free Press.
- Popper, Karl (1991a) *Conjectures and Refutations*. London: Routledge.
- Popper, Karl (1991b) *The Poverty of Historicism*. London: Routledge.
- Popper, Karl (1983) 'The rationality principle'. In: *A Pocket Popper*, ed. D. Miller. London: Fontana, pp. 357-65.
- Popper, Karl (1971a) *The Open Society and its Enemies, Volume 1: The Spell of Plato*. Princeton, NJ: Princeton University Press.
- Popper, Karl (1971b) *The Open Society and its Enemies, Volume 2: The High Tide of Prophecy: Hegel, Marx, and the Aftermath*. Princeton, NJ: Princeton University Press.
- Putnam, Hilary (1983) *Realism and Reason; Philosophical Papers Volume 3*. Cambridge: Cambridge University Press.
- Putnam, Hilary (1981) *Reason, Truth and History*. Cambridge: Cambridge University Press.

- Rabinow, Paul and Sullivan William M. (eds.) *Interpretative Social Science*. Berkeley: University of California Press, 1987.
- Rescher, Nicolas *Methodological Pragmatism* Oxford: Basil Blackwell, 1975.
- Rorty, Richard (ed.) (1992) *The Linguistic Turn; Essays in Philosophical Method*. Chicago: University of Chicago Press.
- Rorty, Richard (1991) *Philosophical Papers Volume 1; Objectivism, Relativism, and Truth*. Cambridge: Cambridge University Press.
- Rorty, Richard *Consequences of Pragmatism* Minneapolis: University of Minnesota Press, 1982.
- Rorty, Richard (1980) *Philosophy and the Mirror of Nature*. Oxford: Blackwell.
- Schutz, Alfred and Thomas Luckmann (1973) *The Structures of the Life-World*. Ill.: Northwestern University Press.
- Sleeper, Ralph *The Necessity of Pragmatism* New Haven: Yale University Press 1986.
- Stuhr, John *Pragmatism, Postmodernism, and the Future of Philosophy* London: Routledge 2003.
- Tiles, J. E. (1988) *Dewey* London: Routledge
- Turner, Jonathan 'Analytical Theorizing,' in Giddens, A. and Turner, J. (eds) *Social Theory Today*. Oxford: Polity Press, 1987.