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Critical realism and recent economic methodology

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Introduction

Although critical realism is often classified as a particular type of 'economic methodology,' this classification seems inappropriate given the traditional definition of methodology. Critical realism is broadly 'methodological' – concerned with philosophical investigation into the scientific character of economics – but it is not literally a 'methodology' in the way that methodology has traditionally been defined within general science theory. Under the traditional definition, methodology is strictly concerned with 'method' – methodological rules that specify the proper relationship between empirical evidence and scientific theories for a wide range of different scientific domains. Notice that, according to this traditional definition, methodology focuses strictly on the form of the scientific investigation: the form that scientific inquiry must take in order to guarantee the cognitive/epistemic value of the products produced by the inquiry. Critical realism is not a 'method' in this sense. The goal of critical realism is to philosophically 'underlabour' for the social science of economics (Lawson 1997a: 61), and that underlabouring is not restricted to methodological rules or the form of the scientific inquiry. For example, critical realists argue that since intentionality is a fundamental feature of human action, accommodating such intentionality must be an essential feature of economics or any social science. This is more than a claim about the proper 'method' of science, it is a claim about the *content* of a particular science, specifically the prerequisite ontological posits of that science. While critical realism certainly overlaps with traditional methodological concerns, it is also clear that it is more than just one particular type of economic methodology.

The purpose of this chapter is to examine the relationship between the critical realist literature and a number of recent developments within the philosophy of science and economic methodology. I will argue that critical realism has much in common with these recent developments – recent methodological trends – and that critical realism shares a common philosophical backdrop with these trends: the backdrop of recognizing

and responding to problems associated with the core ideas of the previous generation of the philosophy of science and the economic methodology derived from it.

The argument that critical realism shares a family resemblance, or at least a common point of departure, with recent methodological trends is not, prima facie, an argument either for or against critical realism. I have criticized critical realism in the past – particularly its interpretation of mainstream economics (Hands 1997a) – and I have also discussed the criticisms offered by others (Hands 2001b), but this paper is not about criticism. This paper is about finding methodological common ground. To this end I will discuss a number of the key problems associated with the traditional approach to economic methodology – what I will call general-rules-based methodology – and make the case that critical realism also recognizes these difficulties and begins from effectively the same philosophical starting point as most contributions to the diverse and rapidly growing methodological literature.

One of the main themes in my recent methodological work (Hands 2001a, 2001b) has been to redefine the field of economic methodology in such a way that it includes a number of current research projects that are outside (sometimes way outside) of the traditional methodological rubric.[1] I have proposed the term 'the new economic methodology' to capture the vast literature that combines *economics and science theory in some way*. In a nutshell my argument is that if one sticks with the traditional narrow definition of economic methodology – as the search for a few very general rules for the proper conduct of economic science – then economic methodology is effectively dead, since it has systematically failed to produce such rules, and no such rules seem to be anywhere on the horizon. On the other hand, there currently exists a vast, expanding, and quite interesting, literature that combines science theory and economics *in some way*, and, under that broader notion of economic methodology, the field is not only alive, but alive and flourishing. The purpose of this chapter is to make the case that critical realism is *one of the approaches* that falls under the general rubric of the new economic methodology. Critical realism is *not* like traditional economic methodology in that it does not search for a few general methodological rules, but it is like much of the new methodological literature in that it *starts from* the same difficulties of the rules-seeking approach in its effort to transform mainstream economic theory.

While I do think it is useful to substantially broaden the definition of economic methodology – to include things like the economics of scientific knowledge and various philosophies of natural science that employ the tools of economic analysis – such radical broadening is not really necessary to make the main point of this chapter. For the current purposes it is only necessary to discuss the problems of rules-based methodology and explain how critical realism acknowledges these difficulties and

attempts to move forward with its own underlabouring endeavors in response to these problems. Thus when I refer to 'recent economic methodology' in this chapter, I do not necessarily mean all of the variety of different projects that I have lumped under the label of the new economic methodology; I simply mean the recent work by economists and philosophers that is responding to the various difficulties associated with the previous generation of rules-based methodological inquiry and is self-consciously engaged in methodological discourse about economic science. Similarly, it is not necessary to refer to all of the various fields within contemporary science theory (such as the sociology of science, anthropology of science, rhetoric of science, etc.) when discussing the problems associated with the general theory of scientific knowledge; for my purposes here contemporary philosophy of science will serve quite effectively as the main reference point within general science theory.

The paper has two main sections. The first examines the recent trials and tribulations of rules-based economic methodology and how those difficulties are related to more general problems within contemporary philosophy of science. The discussion in this first section will be rather brief and focus on only a few of the many controversial issues involved in the literature. For those interested in a more comprehensive treatment I suggest the book-length discussion in Hands (2001b). The second section turns to critical realism and a few of the ways in which it is consistent with, and starts from the same philosophical problem situation as, the literature discussed in the first section. The conclusion provides a brief response to some of the potential criticisms that might be raised about the claims made in the first two sections.

Economic methodology and the philosophy of science

Critical realist books and papers do not generally start with the philosophy of science, or even problems within the philosophy of science; they generally start with the *problems of mainstream economics*. For example, the first chapter of Tony Lawson's *Economics and Reality* opens with the sentence: 'Contemporary academic economics is not in a healthy state' (Lawson 1997a).[2] Lawson's real concern is economic theory; its difficulties may stem from the fact that the discipline has (explicitly or implicitly) adopted an inadequate positivist conception of scientific knowledge and its associated characterization of explanation and concept of a scientific law, but problems in the philosophy of science are only part of the diagnosis, not the subject of the inquiry. The original starting point for Lawson is the sad state of economic science. Of course this immediately aligns critical realism with the interests and concerns of various heterodox economists (Austrian, Marxian, Sraffian, Post Keynesian, feminist, etc.), who also start from the position that mainstream economics is generally in bad shape and needs to be replaced.

Such criticism of mainstream economics does not automatically align critical realism with other approaches to economic methodology – even recent approaches – since there is not any general agreement about the current 'state' of mainstream theory within the methodological literature. For some methodologists, the current state is, as with critical realism, generally quite bad; for others it is globally just fine; for still others it is fine in certain sub-fields and problematic in others; for still others the whole project of methodological appraisal is a philosophical dead end and should be replaced by historical, sociological, or rhetorical inquiry; and there are of course many other different views. Since my purpose is to emphasize the ways in which critical realism is 'like' other recent methodological discourse, I will not begin by discussing the current 'state' of mainstream economic theory, but rather by considering the current 'state' of the philosophy of science, a subject where there seems to be much more agreement.

While the science theory literature of the last forty or so years has been both expansive and extremely diverse, it is possible to distill a few points of general agreement from among the wide range of different commentators. First, the positivist-inspired 'Received View' (Suppe 1977) that was dominant within mid-twentieth century Anglo-American philosophy of science – what Philip Kitcher (1993) calls 'Legend' – has irrevocably broken down.[3] The causes of this breakdown remain hotly debated, but it is possible to discern a few particularly influential elements in the overall set of destructive forces. Thomas Kuhn's *Structure of Scientific Revolutions* (1970), Paul Feyerabend's *Against Method* (1975), and two key papers by W.V.O. Quine (1951, 1969) clearly played important roles in the eventual breakdown of the Received View. Of course this is not to suggest that the philosophical establishment was felled by the blows of a few key individuals, only that a wide range of disparate critical forces coalesced around a few key texts. Recent studies of the positivist movement such as those by Cartwright, Cat, Fleck, and Uebel (1996) and Friedman (1999), as well as recent re-examinations of Kuhn's life and work such as Fuller (2000), make it quite clear that the issues surrounding the demise of the Received View are extremely complex. Second, while the Received View is discredited, there has not been any single approach (or even set of approaches) that has emerged to replace the once dominant positivist-inspired framework. Third, while there is certainly not any consensus about a replacement program, there is a partial consensus about the *general features* that any adequate framework must have. These general features are, in no particular order, that: it must be sensitive to the *social* character of the scientific endeavor; it must recognize, and in some way defuse, both the problem of the theory-ladenness of empirical observations and the Duhem–Quine underdetermination problem; it must view science as more contingent and less methodical than Legend would have it; it must to some degree be

consistent with the actual history and practice of successful science (it cannot be just arm-chair philosophizing); it must be naturalistic in spirit in that it must build on the best contemporary science and approach the subject of scientific knowledge in a relatively scientific way; and finally it must steer some kind of middle ground between, on one hand, the old narrow (epistemically) normative philosophy of science of the Received View and, on the other hand, total relativism about the cognitive status of scientific knowledge. Fourth and finally, philosophy of science must respond to the sociological literature, inspired in part by Kuhn (although not endorsed by him), that emphasizes the social character of science in a way that tends to debunk or undermine the cognitive status of science. This literature – such as the early influential contributions by Bloor (1991), Collins (1985), Latour and Woolgar (1986), Latour (1987), and Knorr Cetina (1981) – reduces the activities and beliefs of scientists to the same type of social forces that determine the activities and beliefs of any other social agents, resulting in either the 'mere description' of scientific behavior without a normative-epistemic evaluation, or in the negative evaluation that scientists are not epistemically 'special' at all. Much of the recent work in the philosophy of natural science has focused on recovering some (positive) version of epistemic normativity from such sociological accounts, while at the same time accepting many of the assumptions of the sociological literature (particularly the social character of scientific knowledge).

So the Received View is gone and its demise has left a particular philosophical problem situation within the philosophy of natural science, but how have these changes affected the recent literature in economic methodology? Well, there are certainly many effects, but the most obvious is that it is simply not possible to continue to do economic methodology in the same way that it was done during most of the twentieth century. The traditional approach to rules-based methodology is best characterized as the 'shelf-of-scientific-philosophy' (Hands 1994) view of economic methodology. The philosophy of natural science was viewed as a 'shelf' of potential methodological rules, and economic methodology, according to this view, consisted of taking things off the shelf and then 'applying' them to the particular science of economics. There are at least two features that need to be emphasized about this shelf approach to economic methodology. First, the items on the shelf were generally taken off *fully assembled*; the ideas were seldom reconfigured or reconditioned for their use in economic science. The general attitude seemed to be that if the items didn't fit very well, the problem was with economics – it was economics that needed to be reconfigured, not the philosophy of science. In a few cases – say Austrian economic methodology – the items on the shelf were rejected, but even here they were still considered to be fully assembled (just not suitable). Second, the shelf was a *philosophy of science* shelf – primarily products produced by Positivist & Popperian (P&P)

philosophical industries – in particular, it was not a general Philosophy shelf and it was not an actual Science shelf. It was not a general Philosophy shelf in that the items were manufactured exclusively by only a few philosophical firms – P&P philosophers of natural science – and not by the discipline of philosophy more generally; the shelf was bare of Hegelian, pragmatist, Aristotelean, or other broad philosophical ideas. It was also not an actual Science shelf in that the items on the shelf were not based on the activities of flesh-and-blood natural scientists – Kuhn's concern – they were based on what philosophers of science thought good scientists ought to do. The point is that rejecting the shelf-of-scientific-philosophy view of economic methodology does *not* necessarily mean rejecting all philosophical ideas or rejecting all consideration of how natural scientists go about their day-to-day activities; rejecting the shelf only means rejecting the idea that all there is to the field of economic methodology is to take prepackaged ideas off the philosophy of natural science shelf – 3' x 5' card philosophy of science (McCloskey 1994) – and 'apply' those ideas to economics.

The fall of the Received View and the corresponding disruption within Legend philosophy of science have necessitated a move away from the shelf-of-scientific-philosophy approach to the methodology of economics. The cupboard is simply bare; there is nothing on the shelf that even the majority of philosophers of science would endorse as the proper scientific method. Given this, those working in the field of economic methodology have been forced to look elsewhere for ideas and inspiration. Where have they turned? Well, they did not turn to just one new shelf, but rather to a dizzyingly wide array of different sources and resources. Those writing in the field of economic methodology have looked outside of the philosophy of science to areas of science studies such as the sociology of scientific knowledge and science and technology studies (see Hands 1997b for a discussion and references); they have turned back to nineteenth science theorists such as Mill (Hausman 1992), to contemporary philosophy of mind (Rosenberg 1992), or to post-positivist-empiricism (Boylan and O'Gorman 1995); they have approached directly the question of how practicing economists employ various concepts such as idealizations (Mäki 1994) or models (Morrison and Morgan 1999); they have turned to classical rhetoric and the literature on the rhetoric of science (McCloskey 1994, 1998); they have even looked to economic theory itself for inspiration (Sent 1999; Wible 1998). There is no formula for doing economic methodology today; the shelf-of-scientific-philosophy is gone and in its stead is a flurry of innovative and exciting new activities.

Critical realism and recent methodological trends

This section will discuss a few of the many ways in which critical realism is consistent with the recent developments in economic methodology and

post-positivist philosophy of science discussed in the previous section. Critical realism certainly starts from the breakdown of the Received View and the demise of the shelf-of-scientific-philosophy, and turns to other resources in its underlabouring efforts, but the consistency runs much deeper and is more broad-based than merely this initial starting point. Since there are so many different facets to the critical realist research program, and since these different facets often relate to the demise of the shelf-of-scientific-philosophy and the new approaches to economic methodology in very different ways, the most effective way to proceed seems to be to simply list a number of significant features that critical realism shares with recent non-rules-based methodological literature. The list is weakly ordered – from general to specific – but the ordering is far less important than the overall consistency that emerges from the ensemble of the various items discussed.

(i) First of all, critical realism draws on certain philosophical resources – specifically the transcendental realism of Roy Bhaskar (1978, 1987, 1989) – but not on resources from mainstream philosophy of science. Bhaskarian realism, like most general philosophical frameworks, involves a particular characterization of science, but it is *not just* a philosophy of natural science (and certainly not a P&P-based philosophy of natural science). Also in keeping with the spirit of contemporary methodological developments, critical realism has systematically reconfigured the transcendental realist program in various ways to better fit the needs of those interested in economics and economic methodology. While much of the early work in critical realism did take ideas directly off the Bhaskarian shelf (a philosophical, but not philosophy-of-science, shelf) and apply them to social science in a relatively prepackaged way, this pattern has changed in the more recent literature; as the program has matured, critical realists have increasingly modified the Bhaskarian concepts in order to make them better suited to the particular concerns of contemporary economics and other social sciences.[4] The bottom line is that critical realism is not a shelf-of-scientific-philosophy-based approach to the philosophy of social science; when philosophical ideas are borrowed, they are not taken from the philosophy-of-science portion of the shelf, and the philosophical ideas that are used have increasingly been reconfigured to meet the particular needs of contemporary social science.

(ii) Perhaps the most obvious point of common ground is that critical realism is self-consciously anti-positivist and begins its analysis from the recognition of many of the problems associated with Legend philosophy of science discussed above. While critical realists focus their attention on underlying objects, structures, and forces that exist independently of scientific investigators, the *knowledge produced* by such scientists is fundamentally social, theory-and-interest laden, situated, and underdetermined:

> [A]lthough the emphasis throughout has been realist, ... this stance is complemented not by an absolutist or foundational position in knowledge but by an *epistemological relativism*, by the thesis that we can only know these things under particular (historically and socially relative), and potentially transformable, descriptions. Clearly, if the (intransitive) objects of knowledge exist (largely) independently of our knowledge of them, such knowledge as we actually possess cannot be identified, or be said to be in correspondence, with such objects; it is not reducible or equivalent to them. Knowledge, rather, exists in a historically specific, symbolically mediated and expressed, practice-dependent, form.
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> (Lawson 1997a: 58–9)

Thus while critical realists would defend the independence of the intransitive domain of natural and social objects/forces, the scientific knowledge we possess about such objects/forces is socially conditioned, contingent, and potentially interest-laden. This is more than just the rejection of the Received View; it is also an endorsement of many of the generally accepted features of post-Legend philosophy of science. It is also important to note that critical realists similarly reject another cornerstone of the Received View: the deductive-nomological (D-N) model of scientific explanation. Critical realists characterize explanation in abductive terms – the identification of the deep underlying (intransitive) causal forces responsible for the phenomenon in question – and while this is just one of many competing explanatory schemes, it begins from the same problems of the D-N model as many other contemporary approaches.

(iii) Critical realism is *not* about *methodological rules*. The key idea behind any rules-based approach to science theory (P&P-inspired or any other) is that there are proper *rules* that individual scientists need to follow in order to produce legitimate scientific knowledge and that those rules are independent of the particular subject matter, social context, interests, or pragmatic concerns of the science in question. Critical realism is *not* rules-based in this sense. Perhaps the most important point of critical realism is that, regardless of how effective empirical realism has been in the natural sciences, it is not appropriate in social science, and, in particular, there are a number of subject-matter-specific features of the social world – specifically human intentional agency and the causal power of social structures – that must be part of the ontological backdrop of any adequate social science. Of course critical realism remains a *normative* philosophical approach – it does specify the ontological features that an adequate social science *ought* to include – and in that sense it is contrary to various approaches (particularly sociological approaches) within contemporary science theory that have abandoned normative considerations in favor of a purely *descriptive* approach to scientific

knowledge, but this normativity is substantially different than the normativity that emerges from rules-based philosophy of science. There are normative aspects to critical realism, but *it does not specify methodological rules* that individual scientists must follow in order to obtain epistemically justified scientific knowledge.

(iv) Critical realism is not *a priori* arm-chair philosophy of science in the sense of the Received View, and in the sense opposed by Kuhn, Feyerabend, and recent historians and sociologists of science. Critical realism, like most of the work in contemporary science theory, attempts to ground its view of scientific knowledge on scientific theory and the actual conditions of scientific practice. In this sense critical realism is more *naturalistic* than the Received View and most of the economic methodology predicated on it.[5] Since this is a rather controversial claim, I will defend it in two separate ways. First, even though critical realism draws its philosophical inspiration from *transcendental* philosophy of the Kantian tradition, and even though this is not the philosophical inspiration for most contemporary naturalisms (see, for example, Callebaut 1993a; Kitcher 1992; Kornblith 1985; Laudan 1990; Rosenberg 1996; Solomon 1995), it is possible to characterize the conditions for possible scientific knowledge in transcendental naturalist terms. Bhaskar has even been cited as a 'well-known defender of transcendental arguments within a naturalistic framework' (Callebaut 1993b: 2, n.3). Second, there are naturalistic philosophers of science – Nancy Cartwright (1989, 1999) in particular – who advocate philosophical positions that are quite similar to transcendental realism. This is not to say that Cartwright's philosophy of science (or Bhaskar's) is entirely unproblematic, but it does suggest that it is possible to start from what is essentially a naturalistic position and end up with a philosophical perspective that looks very much like critical realism. While critical realism does not support the 'naturalistic' view that social and natural science share the same basic explanatory framework, it is broadly consistent with the naturalistic turn that characterizes much of contemporary science theory.

(v) Critical realists start from the position that human *intentionality* is an essential feature of any social science, and that such intentionality is *prima facie in conflict* with P&P notions of proper scientific inquiry. Now while many contemporary philosophers of mind (Churchland 1992), and certain philosophers writing about economics (Rosenberg 1992), use this same conflict to argue for the *elimination* of all intentional 'folk-psychological' concepts from psychological and economic science – precisely the opposite of the stance taken by critical realists such as Lawson (1997a) – the *point of departure is exactly the same*. Both begin from the position that explaining human action in terms of intentionality (beliefs and desires) is in conflict with the D-N model of explanation and the associated notion of scientific laws, and therefore that one of these things – either intentionality or D-N explanations and P&P justified scientific laws – needs to go. It should also be noted that the tension between D-N and social science explanatory schemes has also been identified within the Popperian wing of the P&P tradition (see Caldwell 1991; Hands 1991a, 1991b), and while various Popperian solutions have been offered, it has recently been suggested that critical realism might be a useful resource for reconciling these difficulties as well (see van Eeghen 1996).

(vi) Finally, and focusing more explicitly on *economics*, critical realists have systematically emphasized the role of *tendency laws* in economic science. The explicit discussion of tendency laws in economics of course goes back to at least Mill (1874), but the importance has recently been re-emphasized in influential works by Hausman (1992) and Cartwright (1994, 1995). While critical realists such as Lawson employ a slightly different (and less empiricist) notion of tendency laws than either Hausman or Cartwright,[6] the point is not that critical realism is identical to other recent approaches in economic methodology; it is simply that critical realism is motivated by a similar set of issues and concerns (in economics as well as in science theory). The nature of the tendency laws that are at work in economic science is an important methodological question that has been identified by critical realists as well as by a number of other recent methodological commentators.

Conclusion

The purpose of this chapter was threefold. I have tried to explain the major developments that have taken place within the philosophy of science since the breakdown of the Received View, to explain the recent turn away from both the shelf-of-scientific-philosophy and the search for general rules within the recent methodological literature, and to make the case that critical realism is broadly consistent with these changes in philosophy of science and economic methodology. While more could obviously be said about any one of these subjects, I believe I have provided an effective defense of all three parts of the argument. Critical realism clearly has much in common with other recent developments in science theory and economic methodology.

In closing I would like to make a few comments (predictions?) about how the above argument might be received among various readers within the methodological community. I fear the argument might succumb to the old adage that 'man who walks in middle of street gets hit by cars going *both* ways.' It is possible that both *defenders and critics* of critical realism will find my argument to be unsatisfactory.

Consider critical realists first. Most critical realists perceive their efforts in fairly grand-historical terms; the goal is essentially the *transformation* of mainstream economics and its replacement by an alternative (or alternatives) that is (are) committed to an adequate social ontology and

devoid of the many difficulties currently associated with mainstream economic theory. Recruits for this transformative effort will come from heterodox economists and others profoundly displeased with current economic theory; it is a grass-roots movement of critical economists and other social scientists, and while it is broadly 'philosophical,' it is not really *for* philosophers of science or other science theorists, and I suspect that many critical realists will have little interest in the question of whether their transformative underlabouring is, or is not, consistent with recent developments within the philosophy of natural science (or even economic methodology). Critical realism is about substantive transformation, not fashionable philosophical chit-chat. Thus it is possible that most critical realists – even if they do not disagree with what I have said about philosophy of science, economic methodology, or critical realism – will find my argument to be essentially *beside the point*. So what if critical realism is consistent with a number of recent developments within philosophy of science and economic methodology: who cares?

Now consider the response of critical realism's philosophical and methodological critics. While some critics will undoubtedly challenge my claim that critical realism is consistent with these recent developments, a more likely response will be: 'Okay, critical realism may start from the same set of problems, but its solutions (and the whole transcendental approach) to those problems are radically at odds with the answers offered elsewhere within contemporary science theory and economic methodology.' So what if critical realism starts from the same philosophical problem situation as much of the recent literature, their answers are systematically (perhaps blithely) contrary to that literature?

While it may not be possible to answer either defenders or critics in a way they will find satisfactory, I nonetheless conclude by offering a response to both sides of the debate. For the critical realists my response is simply that, interesting or not, critical realism *is* one aspect of broader intellectual trends within the study of scientific knowledge. This may not be the reason that most critical realists support the program, but it is a fact of intellectual life, and it is also one of the reasons that critical realism has attracted so much attention within the methodological literature. There is a substantial literature – in both philosophy of science and economic methodology – in which arguments are presented that have much in common with the arguments made by critical realists (without either side recognizing the similarity of the positions). Recognition of these commonalities would simultaneously make new intellectual resources available to critical realists, and potentially open the door for the wider dissemination of critical realist ideas. Critical realists have increasingly been willing to modify the Bhaskarian transcendental realist framework to suit the problems of the social sciences; recognizing where overlap exists with other philosophical positions provides a wealth of additional resources for these efforts.

For the methodological critics of critical realism my response is simply to reiterate what I said in the introduction. My point is not that critical realism is right or wrong about all of the solutions it offers for the problems in either science theory or economics – I have previously criticized aspects of the critical realist literature – my point is simply that critical realism starts from many of the same issues and concerns as other contemporary approaches. One does not need to agree with the contents of Debreu's *Theory of Value* in order to recognize it *as* an economics text, and one does not need to agree with everything written by critical realists in order to recognize it *as* literature consistent with contemporary methodological trends. The title of this chapter is 'transforming methodology'; it is neither 'transforming economics' as some critical realists might have it, nor 'transforming critical realism' as some critics might have it. Critical realism is one element within a broad set of different literatures that share a common philosophical point of departure and are actively engaged in trying to transform the way we think about science in general and economic science in particular. While this fact may not be appreciated by everyone on both sides of the debate, I believe that I have successfully demonstrated that it is nonetheless an important feature of contemporary discussion about scientific knowledge and economic science.

Notes

1 One needs to be careful about the use of 'traditional' here. The vision of economic methodology that is being replaced – the traditional view – is endemic to mid-twentieth-century economics; those writing on economic methodology during the nineteenth and early twentieth centuries seem much less likely to exhibit this 'traditional' tendency.

2 I will not provide a survey/summary of critical realism. I recommend the books by Lawson (1997a), Collier (1994), and Fleetwood (1999); or the many papers where the position has recently been summarized and defended, for example Jackson (1995), Lawson (1994a, 1994b, 1995, 1996, 1997b, 1999), or Fleetwood (1997); or my own summary in Hands (1997a or 2001b).

3 Since Karl Popper and the Popperian philosophical school were never as influential in general philosophy of science as in economic methodology, most philosophers would not include Popperian falsificationism within the Received View, but given its role in economics it seems appropriate to include it.

4 Perhaps I should say: 'that make them better suited to the particular concerns of contemporary non-Marxist economics and other social sciences.' Bhaskarian transcendental realism was explicitly a Marxist philosophical framework and much of the early debate about critical realism was confined to debates inside the Marxist tradition. Over time the work of Tony Lawson and others has moved critical realism out of the strictly Marxist sphere of discourse and into the wider realm of economic methodology and general philosophy of social science. Since these authors employ critical realism to

criticize neoclassical economics, the recent critical realist literature may still have a certain Marxist appeal; the difference is that it is not exclusively a Marxist appeal. Many institutionalists, feminists, Post Keynesians, Austrians, and countless (often non-aligned) others who believe the disabilities of mainstream economics are ontological in origin have been attracted to the recent critical realist literature.

5 While this is not the place for a detailed discussion of what I mean by 'naturalism' and the associated 'naturalistic turn' in recent philosophy of science – these subjects are main themes in Hands (2001b) – it is important to note that my use of the term is different from the way it is generally used within the critical realist literature. Most critical realists use the term 'naturalism' in the way that it has traditionally been used by philosophers of *social* science – that 'social science phenomena are susceptible to explanation in essentially the same sense as are natural phenomena' (Lawson 1997a: 45–6) – while I use it in the way that it is used in contemporary philosophy of *natural* science – roughly, that 'scientific knowledge' is a natural object that can be investigated using scientific resources in the same way that any other natural object can be investigated. These two uses of 'naturalism' or 'naturalistic' are obviously related, but they are not the same thing. It is important to emphasize the difference since I am crediting critical realism with participation in the broad naturalistic turn within contemporary philosophy of science, while at the same time most critical realists would stress that the program is not naturalistic; given the different uses of the term, these two assessments of critical realism need not be in conflict.

6 The similarities and differences among these three notions of tendency laws (and the associated *ceteris paribus* conditions) are examined in detail in chapter 7 of Hands (2001b).

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