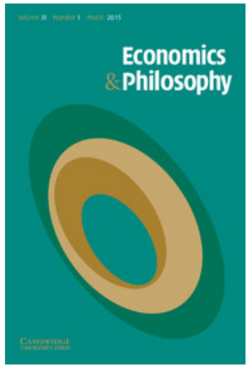

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PROBLEMS WITH REALISM IN ECONOMICS

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This essay attempts to distinguish the pressing issues for economists and economic methodologists concerning realism in economics from those issues that are of comparatively slight importance. In particular I shall argue that issues concerning the goals of science are of considerable interest in economics, unlike issues concerning the evidence for claims about unobservables, which have comparatively little relevance. In making this argument, this essay raises doubts about the two programs in contemporary economic methodology that raise the banner of realism. In particular I argue that the banner makes it more difficult to relate the concerns of those who wave it (Tony Lawson and Uskali Mäki) to those of other methodologists. Although this essay argues that many of the debates in this century between scientific realists and their opponents are not relevant to economics, it does not attack scientific realism, and it does not urge economists or economic methodologists to reject it.

After some general words concerning realism (in Section 1), Section 2 develops the contrast between realism and instrumentalism, which Section 3 illustrates with reference to the work of Milton Friedman and Fritz Machlup. Section 4 develops the largely orthogonal contrast between realism and epistemological anti-realist philosophies such as van Fraassen's. Section 5 argues that the debate between realists and epistemological anti-realists is largely irrelevant to economics on the grounds that economics does not postulate unobservables in the way that physics does. Sections 6 and 7 are devoted to the realist programs of Lawson and Mäki, and Section 8 draws together my conclusions.

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1. REALISM

A philosophical position is a form of realism if it holds that entities or properties of some kind exist or are real, or that some class of propositions is true. Given how many different categories of entities, properties, and propositions there are and given how many different notions of existence, reality, and truth philosophers have considered, there are hundreds of different forms of realism. A complete taxonomy would accordingly be an immense undertaking. Furthermore, it may be costly and confusing to label specific problems concerning existence, reality, or truth as problems concerning realism, because one will have to distinguish the particular question from all the many other questions concerning realism. For example, those who argue for the relativist and idealist views that characterize some 'post-modern' philosophies ('the world is just a text') can certainly be regarded as anti-realists, and the consideration of their views could be regarded as an inquiry concerning realism. But to raise the problem of whether the world is anything more than a text as a problem about realism forces one into a long discussion distinguishing these questions from other questions about realism. In addition there are serious dangers of confusion, since most anti-realist philosophies of science reject the relativism of the post-modernists and thus, with respect to that dispute, count as 'realist'. A better strategy is to pose questions about relativism as questions about relativism, rather than as questions about realism and thereby to avoid having to distinguish in painful detail the sort of realism that opposes relativism from other sorts of realism.

Despite the costs involved in bringing together disparate inquiries concerning existence, reality, and truth under the heading of inquiries concerning realism, philosophy thrives on the contemplation of such abstract unifications. Insights may be gained by examining the common features of inquries into the reality of the mental or the social, into the possibilities of truth in ethics or in aesthetics, or into the existence of subatomic particles. Cataloguing the different varieties of realism, revealing the common features of argumentation in distinct fields, and articulating precisely what distinguishes the many different debates are accordingly important philosophical tasks.

Since most varieties of realism are not relevant to economics, I question, however, whether such a catalogue is relevant to the concerns of economists or of particular interest to economic methodologists. Accordingly, I shall not offer any such taxonomy.[1] This essay will instead

[1] Readers interested in more general discussions of varieties of realism might start with Mäki (1998) and then consult Nagel (1959, Chapter 6), Putnam (1979), Devitt (1984), and Leplin (1984). For another appraisal of Lawson and Mäkis' realist programs, see Boylan and O'Gorman (1995).

consider only four questions about realism that have been of interest to philosophers of science. These questions have concerned (1) the goals of science, (2) the existence of entities postulated by scientific theories, (3) the status of claims concerning such entities and their properties, and (4) the possibility of gaining knowledge of such entities and properties. Actually it will be possible to simplify further, because, as I shall argue in Section 4, the answers to the ontological and semantic questions (2) and (3) turn on the answer to the epistemological question (4). So I shall focus on just two questions: (1) What are the goals of science? and (2) Is it possible to acquire knowledge of the unobservable entities and properties postulated by scientific theories? Realists hold that such knowledge can be gotten and that science should aim to get it.

2. REALISM VERSUS INSTRUMENTALISM

Instrumentalism maintains:

I (*Instrumentalism*)

The ultimate goals of science are and should be exclusively practical and scientific theories are tools that should serve these goals.

Instrumentalism is a thesis about the goals of science and the goals of theories. Science serves practical interests by enabling people to anticipate and to control phenomena. Theorizing is an important part of science that serves these same ends. Scientific realism, in contrast, holds that science should also have purely cognitive goals and that theories should serve those goals, too. Scientific realism thus also consists of theses concerning both science and scientific theories. Although there have been realists who have argued that practical concerns are no part at all of pure science (Karl Popper, for one, comes close to defending such a view), most scientific realists maintain that the goals of science are *both* cognitive and practical. Those who hold that theories are instruments designed to serve both practical and purely cognitive goals are instrumentalists about theories, but not about science itself. They hold a realist view of science and an instrumentalist view of theories, and they have often been called 'instrumentalists'. I shall not follow this practice, and shall call them 'instrumentalists about theories'.

The controversy between realism and instrumentalism is ancient; yet it has also been important in modern philosophy of science. There are three sources of instrumentalism: pragmatism, positivism, and pessimism – that is, philosophies that place human action and interest at the center (such as American pragmatism), empiricist epistemological and semantic worries concerning theoretical postulates, and pessimism about making literal sense of particular successful scientific theories.

For example, according to Aristotelian physics, celestial bodies are composed of a luminous material that is inclined by its nature toward a circular motion around the center of the universe, which Aristotle holds to coincide with the center of the earth. Aristotle speculates that the sun, the moon, the planets, and the stars are all mounted on rotating concentric crystalline spheres, whose axes are fixed on the inside of the surrounding sphere. Aristotle also argues, with more certainty, that there cannot be a vacuum, because motions are always limited by resistance, and without resistance motion would become infinite. Although Aristotle's account made some physical sense and was able to capture qualitatively some of the complexities of the observed motions of the planets, it was difficult to work with and never enjoyed much quantitative success.

In the centuries after Aristotle's death, his account was supplanted by Ptolemy's astronomy, which retained the earth at the center, but which represented the motions of the planets with different geometrical devices. The best known of these is the epicycle, a small circle whose center rotated around the sun. Ptolemy's astronomy made possible progress in fitting the motions of the planets with quantitative precision, and it thereby made possible a much more accurate calendar. Yet the geometrical constructions of Ptolemaic astronomy made no physical sense. There was no way to fit undecaying epicycles into a vacuumless heaven filled with crystalline spheres that moved naturally around the center of the universe.

Those who were nevertheless impressed with the ability of Ptolemy's theory to represent the data concerning the positions of the planets came to see the goal of science as merely to predict (or represent) phenomena and thereby to guide human practices. If the goal of astronomy is only to represent the motions of the planets and thereby to define calendars and to guide navigation, then Ptolemaic astronomy is highly successful, and one need no longer worry about making sense of the constitutions of planets or the mechanics of their motions. Many natural philosophers in this century, who have been equally pessimistic about the possibility of making sense of quantum mechanics, have in the same way been inclined to take an instrumentalist view of science. In their view, one does not need to ask whether what quantum mechanics says about the constituents of matter are true. The assessment of quantum mechanics, like the assessment of Ptolemaic astronomy, turns on the success of its predictions.

These pessimists are instrumentalists. They maintain that theories are instruments to make predictions concerning (observable) phenomena that matter to human beings, and it does not matter whether they are true or false. In the work of the American pragmatists, one finds a deeper rationale for instrumentalism. The pragmatists argued that all meaningful inquiry, no matter how abstract, must ultimately serve practical ends. They held that failure to appreciate this point explains the cul-de-sacs of previous philosophy and that appreciation of this point promises clarification and progress in intellectual life. Once one recognizes that science has ultimately a practical aim, one can avoid fruitless controversies involving questions whose answers are irrelevant to human interests, and one can see better how to address questions whose answers are relevant.

Some instrumentalists went on to define meaning and truth in terms of human interests and thus to deny that claims concerning unobservables could be meaningful or have a truth value. Sidney Morgenbesser calls this group 'non-cognitive instrumentalists'. But, as he points out, there are 'contextualist' instrumentalists, too, who are for the most part agnostic about whether claims concerning unobservables can be true or false (1960, p. 202). One can assert that the truth of claims concerning unobservables does not matter and that science should not aim at such theoretical truth without also asserting that claims concerning unobservables are never true or false. As I will explain shortly, Milton Friedman could be regarded as a contextualist instrumentalist.

Some instrumentalists arrived at a view of theories as tools because of philosophical worries about unobservables. Empiricists hold that ultimately all evidence concerning matters of fact derives from sensory experience and that terms have no meaning if it is impossible directly or indirectly to tell by means of observation whether they apply. Empiricists have consequently found twentieth-century physics disturbing, since it refers to entities, such as electrons, that cannot be perceived. How can one tell whether claims about electrons are true or false? How can such claims have any meaning? One way to satisfy these empiricist qualms is to deny that such claims need have any meaning or need be true or false. They are instead 'inference tickets' – syntactic strings that permit one to draw meaningful conclusions concerning observables.[2] If claims concerning unobservables cannot be known to be true or false, science cannot aim at the truth concerning them. Its goals must be circumscribed. The most it can aim at is the truth concerning observables (Frank 1988). One thus reaches a view of the goals of science that largely coincides with that defended by the pragmatists and pessimists.

Notice that instrumentalists are not anti-theoretical. They are not

[2] 'The empirical examination of a physical theory ... is not made by interpreting and understanding the axioms and then considering whether they are true on the basis of our factual knowledge. Rather, ... [w]e construct derivations in the calculus with premisses which are singular sentences describing the results of our observations, and with singular sentences which we can test by observations as conclusions ... Only singular sentences with elementary terms can be directly tested; therefore, we need an explicit interpretation only for these sentences' (Carnap 1939, p. 67).

behaviorists or operationalists who insist that science confine itself to observable phenomena. To the contrary, instrumentalists welcome theory, no matter how outrageous its theoretical postulates, provided that it has some practical payoff. Instrumentalists are, if anything, readier to encourage ambitious theorizing than are scientific realists, because instrumentalists place fewer constraints on such theorizing. As a matter of human psychology, realism is, I believe, the philosophical environment in which theorizing thrives best. It is harder to be enthusiastic about an inference ticket than about an underlying reality. But this is a matter of psychology, not logic. Instrumentalists welcome theorizing.

Notice also that there is an ambiguity in the notion of an instrument and in the instrumentalist's account of the goal of science. A barometer and a hammer are both instruments, but they are instruments of very different kinds. A barometer measures air pressure and thereby helps people to anticipate weather changes. If people had other tools, barometers might help them to control the weather, but the barometer itself only assists in measurement or observation. A hammer, in contrast, helps one to act. To say that theorizing should aim to provide people with good tools is thus ambiguous. Should it aim to give them tools to measure and passively to predict, or should it aim to give them tools to act and to control? These goals are often compatible, but they are not the same. For a while at least, the Phillip's curve permitted economists to predict the rate of unemployment associated with rates of inflation, but the relationship broke down when governments attempted to use the relation to control the rate of unemployment. To predict requires knowledge of stable correlations, while to control requires knowledge of causation.

Instrumentalists generally passed over this ambiguity, which helped to cloak the disagreement between, on the one hand, pragmatists who took 'prediction' (true implications concerning observable phenomena) to be the goal because only predictions serve practical purposes of active agents and, on the other hand, pessimists and positivists who took prediction to be the goal, because no other knowledge could be had. The failure to recognize that for practical purposes agents want knowledge of causes, not just regularities, is unsurprising, once one recognizes the general uneasiness earlier this century about the notion of causation and the importance attached to explicating science in purely extensional terms. But the ambiguity concerning goals is real, and, as I have argued elsewhere (Hausman, 1998), a concern with control implies that the search for *causes* is central to science.

Scientific realists, in contrast, defend a realist thesis about the goals of science - science aims at truth. Since most realists have regarded explanation as a cognitive activity that requires truth, most of them have also held that science aims to provide explanations. The possibility that the goals of seeking truth and seeking explanations may sometimes compete has not been taken seriously, and realists have taken these two goals as fully compatible and indeed sometimes virtually as one. The scientific realist maintains that questions about whether crystalline celestial spheres permit epicycles must be faced by Ptolemaic astronomers and questions about how measurement leads to a collapse of the wave function have to be faced by quantum theorists. If these questions cannot be answered, then the goals of science have not been met, though such theories can nevertheless serve practical purposes.

To hold a realist view about the goals of science commits one to realist views concerning ontology, semantics, and epistemology. There is no point in asking questions about unobservables unless (1) it is possible that unobservable entities and properties exist, (2) claims concerning them can be true or false, and (3) there is some possibility of getting evidence that supports some answers rather than others. One can thus formulate realism as four claims:

R (*Scientific realism*)

1. *Goals*: Science aims to discover the truth about its subject matter as well as to assist human practices. Scientific theories should serve these aims.
2. *Truth*: The claims theories make, including the claims involving unobservables, are true or false and should be true.
3. *Existence*: The unobservable entities referred to by true theories exist.
4. *Knowledge*: It is possible to have good reason or evidence for scientific theories, including theories that talk about unobservables.

Scientific realism thus fuses claims about the goals of science with ontological theses about the existence of unobservable entities and properties, semantic theses about the meaning and truth conditions of claims concerning unobservables, and epistemological theses about the possibility of gathering evidence concerning unobservables.

Although debates within economics between realists and instrumentalists have died down now, they were central to economic methodology in the 1950s and early 1960s. The debate about goals was so central and lively, because so much was believed to depend on whether one adopted a realist or an instrumentalist view. Milton Friedman and Fritz Machlup argued that adopting an instrumentalist perspective enables one to dismiss spurious empirical criticisms of economic theory. Lee Hansen maintains that many young economists in the 1950s saw Friedman's instrumentalism as 'liberating' economics. Although this is not the occasion for yet another discussion of the 'realism of assumptions' debate, a few words of history will help clarify my abstract points concerning instrumentalism and will help highlight the peculiarities of the very different discussions of realism in the methodological literature of the 1990s.

3. INSTRUMENTALISM IN ECONOMICS IN THE 1950s

Milton Friedman expresses the basic thesis of instrumentalism explicitly: 'The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed' (1953, p. 6).[3] This much is obvious. More controversially, I would also maintain that he is not at all concerned with ontological, semantic, or epistemological questions concerning unobservables, and indeed he never addresses the question of whether economics refers to unobservables. When he discusses 'unrealistic assumptions', he has in mind propositions that are not descriptively complete or that are false, not propositions concerning unobservables.

Friedman takes seriously questions about what the goal of science ought to be and holds consistently to his instrumentalist answer, both because he thinks it is important for economists to recognize their real mission and because he thinks that an instrumentalist view of the goals of science has important implications for the assessment of economic theories. Unlike more philosophical instrumentalists, who take every incorrect implication concerning something observable to be a black mark against the theory, Friedman denies that scientific theories need to be all-purpose instruments. In his view, the goal of economics is to make true and contentful predictions concerning the phenomena that are of interest to economists. If the theory has mistaken implications concerning, for example, the results of Richard Lester's surveys (1946), that is neither here nor there. The goal is what I have called 'narrow predictive success' (Hausman, 1992b, p. 218), not true implications concerning any and all observables.

From his view that the goal of economics is 'narrow predictive success' – that is, predictive success with respect to the phenomena of interest to economists – Friedman takes it to follow that the only question relevant to the appraisal of an economic theory is how successfully it has predicted those phenomena. This is fallacious. It is just like arguing that since the only thing that matters about a computer calculation program is whether it calculates correctly, there is no point in studying the algorithm that the program is based on (Hausman 1992a, pp. 162–9; 1992b). Of course, if one could check all possible calculations of a

[3] Although Friedman states that the goal is prediction, it is clear that he is really concerned that theories make predictions that permit control. As I argue in 'Explanation and Diagnosis in Economics' (1998), he is thus committed to the view that science ought to search for causes.

program and discover that they were all correct, then there would be no point in studying the algorithm. But one cannot, and by studying the algorithm one might find further reason to trust the program or grounds to believe that the program will make specific kinds of mistakes. The relevant point in this context is that Friedman takes his form of instrumentalism to have strong implications concerning how to appraise economic theories. So the debate between realism and instrumentalism was seen as going to the very heart of economic methodology.

Fritz Machlup offers a more philosophical challenge to realism in economics. His denial that one need be concerned about the apparent falsity of the postulates of economics seems to be motivated by his understanding of the nature of theorizing rather than by an instrumentalist view of the goals of science. Unlike Friedman, Machlup (1960, pp. 563–77) maintains (at times) that the claims of theories are neither true nor false, because of the abstraction or idealization that is central to all theorizing. Sometimes he argues on these grounds that the basic postulates of economics cannot be false, and I will comment on this general view below in Section 5.2. At other times, Machlup argues that the basic postulates of economics are not open to direct observation or test. For example, he compares the notion of 'money illusion' to that of the neutrino:

> With the help of the new construct the consequences deduced from the enlarged system promised to correspond to what was thought to be the record of observation; but the construct is without direct reference to observables and no one could reasonably claim to have any direct experience of illusions suffered by other minds. The reference to observed phenomena is entirely indirect. (1960, p. 579)

Since one cannot test the basic postulates of economics directly, one can only assess them indirectly by testing the observable consequences one can derive with their help. Machlup suggests an instrumentalist view, whereby it is inappropriate to assess the truth or falsity of theoretical claims at all. The only relevant question is whether such claims are good tools for making predictions concerning observable market phenomena (Machlup, 1955; 1956).[4] Just as sophisticated logical positivists recognize the legitimacy of theories in physics that concern unobservable phenomena, yet have correct observational implications, so should economists recognize the legitimacy of theories in economics that employ 'pure constructs'.

Machlup's analogy between the unobservable claims of particle

[4] At other times Machlup (1960) suggests instead that such theoretical claims are 'partially interpreted' through their links with observational consequences and may justifiably be judged true or false, according to whether their consequences are true or false.

physics and the false claims of economics is misleading. As he sometimes recognizes, the problem with claims such as, 'People's preferences are always transitive', or 'All firms attempt to maximize profits', seems to be that they are false rather than untestable.

> [T]he assumption of consistently profit-maximizing conduct is contrary to fact.
> ... [H]ere we are defending an assumption of which we are certain that it does not always conform to the facts. If the deviations are insignificant we can safely neglect them. But we do not know *how* significant they might be ... What then should be done? Just what is being done: to accept maximizing conduct as a heuristic postulate ... Again, the 'indirect verification' or justification of the postulate lies in the fact that it gives fairly good results in many applications of the theory. (1956, p. 173)

Machlup gives instrumentalism a twist. Unlike the logical empiricists, he is not trying to show how statements might be legitimate even if one cannot test them directly. He argues instead that one *should not test the basic assertions of economics individually and that one should ignore their apparent falsity*. In Philippe Mongin's terminology (1988, p. 311), he endorses an unlimited semantic liberty: in the event of the inconvenient falsification of a statement *S*, simply reinterpret terms in *S* so that they no longer refer to, or denote, anything observable. There is nothing in the distinction the logical positivists drew between observable and theoretical statements that justifies this semantic liberty, which, in effect, licenses one not to test or not to heed the results of tests. I shall return in Section 5.2 to the question of whether Machlup's variant of instrumentalism has any justification.

4. REALISM VERSUS ANTI-REALISM

Rather than focusing, as Friedman does, on the goals of science, one could instead emphasize epistemological questions concerning whether knowledge of unobservables is possible. Debate that is narrowly focused on these epistemological questions has become more prominent in philosophy during the last generation. An anti-realist, such as Bas van Fraassen (1980), agrees that the goals of science are explanatory as well as predictive. Science aims at the truth, insofar as it is possible to tell what is true or false. Unfortunately, in his view, it is not possible to have evidence for or against claims that refer to unobservable things. So science cannot aim at the truth concerning any underlying unobservable reality. Science aims (or should aim) instead at 'empirical adequacy' – that is the truth about everything that is observable. Van Fraassen accordingly formulates his anti-realism (which he calls 'constructive empiricism') as follows:

> CE (*Constructive empiricism*) Science aims to give us theories which are empirically adequate: and acceptance of a theory involves as belief only that it is empirically adequate. (van Fraassen, 1980, p. 12)

Although van Fraassen, in contrast to Duhem (1906), believes that science helps people to explain phenomena, like Duhem, he sees theories as devices for organizing and representing knowledge of observables. An anti-realist such as van Fraassen or Duhem is as opposed to Milton Friedman's views as is a scientific realist. Unlike the instrumentalist, who challenges the first realist thesis concerning goals, what divides the realist and the constructive empiricist is fundamentally epistemology, not ontology, semantics, or views about goals: can humans get evidence concerning unobservables?

Starting with an ontological anti-realism – that is, with the denial that anything unobservable exists – one could arrive at an anti-realist view of the proper epistemic attitude toward theorizing, such as constructive empiricism. Ontological anti-realism implies semantic anti-realism: if unobservables do not exist, claims about them (apart from claims about their non-existence) cannot be true. Ontological or semantic anti-realism implies an epistemological anti-realism that says that the only thing one can know about unobservables is that they do not exist. Verificationists made the inverse argument from epistemology to semantics and ontology. If the meaning and truth conditions for statements consist in their method of verification, and statements concerning unobservables cannot be verified, then statements about unobservables cannot be true or false. (And can the unobservables exist that such statements putatively refer to, if what is said about them is never true or false?)

Since van Fraassen is not a verificationist, he does not have to draw this dubious inference, and he is neither a semantic nor an ontological anti-realist. His position is entirely epistemological. He does not deny that unobservable things exist – indeed on his view, evidence against their existence is as hard to come by as evidence for their existence. An anti-realist like van Fraassen only denies that we can have evidence or knowledge concerning unobservables. Because he denies that we can ever get knowledge concerning unobservables, van Fraassen must disagree with the realist about the goals of theorizing, and he could be called an instrumentalist concerning theories. But the basis for this disagreement is entirely epistemological. Van Fraassen does not share the practical view of the goals of science that is central to instrumentalism.

The crucial point is that despite this epistemological path toward a sort of instrumentalism, questions about the goals of science and questions about what one can know are largely orthogonal to one another. Since realism consists of four theses, challenges to realism may challenge different theses. Disputes about goals between realists and instrumentalists are largely orthogonal to epistemological disputes between realists and constructive empiricists. The following table may help to clarify this point:

Goals	*cognitive and practical*	*practical only*
View of unobservables		
knowable	scientific realism	cognitive instrumentalism (Friedman?)
unknowable	anti-realism: van Fraassen's constructive empiricism	non-cognitive instrumentalism (Machlup?)

To classify Friedman as a cognitive instrumentalist is questionable, because there is no evidence that he believes that economic theories involve unobservables and no evidence that he holds any view concerning whether knowledge of unobservables is possible. His instrumentalism is entirely governed by his view of the goals of economics. One can also question whether Machlup is a non-cognitive instrumentalist, because his position concerning the goals of economics is equivocal. Some of his formulations suggest that he should be regarded as a non-instrumentalist anti-realist. The crucial point is that there are two debates here, and these debates are largely (but not completely) independent of one another.

5. DOES ECONOMICS REFER TO UNOBSERVABLES?

As hinted above in the discussion of Machlup, the ontological, semantic, and epistemological issues separating realists from anti-realists and from some instrumentalists, are largely irrelevant to economics. The reason is simple: economic theories for the most part do not postulate new unobservable entities.

5.1 Are preferences and expectations 'theoretical entities'?

It seems absurd to maintain that economics does not refer to unobservables. Surely the preferences and expectations that explain and predict choices are unobservable. Who has ever seen or smelt a preference? Who has ever tasted a belief?

In some 'absolute' sense of 'observable' or 'perceptible', beliefs and preferences are not observable. *But neither are tables or chairs.* With the help of suitable illusions (or 'virtual reality' devices), humans could have sensory experiences like those typically caused by interactions with tables and chairs, even if there were no tables or chairs. Though there is surely no conscious process of inference from sensory experience to perception of tables or chairs, there is also no 'direct' perception of them – or, if one prefers, direct perception of tables and chairs involves an implicit inference from sensory effects to their material causes. An insistence that knowledge must rest on an unmediated sensory basis leads to the morass of phenomenalism and (at best) to the unsatisfying conclusion that none of the things referred to in scientific theories is observable. Insisting that only sense data can be known does not justify the distinction anti-realists make *among* the entities and properties referred to by sciences.

A better response is to reject as chimerical any 'absolute' distinction between what is observable and what is not. Instead, one can draw the distinction naturalistically (as van Fraassen does) with the help of scientific theories of the human perceptual apparatus. In this sense amoebae are unobservable, because humans need magnifying glasses to see them. If human sight were more acute, amoebae would be observable. In this sense, mass is unobservable; and it is harder to imagine what changes in human perceptual apparatus could make it observable. One can *measure* mass by means of its consequences, but one cannot literally observe it. Beliefs and preferences are surely unobservable in this sense, too. They cannot be detected without assistance by any of the five human senses.

One might reasonably question why observability in this naturalistic sense is supposed to matter so much epistemologically. Why should the fact that the lenses of our eyes suffice to detect minnows make it possible to give evidence that bears on claims about minnows while the fact that the lenses of our eyes do not suffice to detect amoebae make it impossible to give evidence that bears on claims about amoebae? Since the vast bulk of knowledge necessarily depends on observations made by different people, van Fraassen owes us an account of why perceptual reports of others justify conclusions when messages from reliable and well-understood devices that detect imperceptible entities do not (Shapere 1982).

This is not, however, the occasion for a critical examination of van Fraassen's epistemology (see Churchland and Hooker 1985); and if my argument for the irrelevance to economics of the epistemological issues dividing realists and anti-realists depended on the epistemic irrelevance of the limits to (naturalistic) observability, it would have to be an argument for the irrelevance of these issues to all of science.

The point I want to insist on is a different one. Anti-realists seek to draw a line between the relatively unproblematic claims of everyday life and the problematic theoretical posits of science. Physics postulates new unobservables, to whose existence commonsense realism does not commit us. Although economics refers to unobservables, it does not, in contrast to physics, postulate new ones. Its unobservables – beliefs, preferences, and the like – are venerable. They have been a part of commonsense understanding of the world for millennia.

Why does this matter? However venerable they may be, they are unobservable all the same. Their long history is a dubious epistemological credential. Their part in the commonsense understanding of the world does, however, mean that one cannot be an anti-realist about the unobservables of economics and a commonsense realist. An anti-realist about economics must be a radical skeptic. She must deny that she can know that her son prefers chocolate ice cream to vanilla or that her aunt believes that airplanes fly. Those who hold that people can know things like this are realists about beliefs and wants. One could take this as an argument for realism in economics, but to do so would misleadingly suggest that scientific realists and anti-realists concerning economics could share the common ground of everyday reality and disagree merely about unobservables postulated by economics. They cannot. The common ground of everyday reality preempts the controversy. There is no issue concerning realism versus anti-realism in economics that is not simultaneously an issue concerning the everyday understanding of the world.

What has been taken to be commonsensical can, of course, be challenged. Some philosophers question whether there are such things as beliefs and preferences and suggest that some day we may learn to interpret the world without making spurious references to them. In the context of considering these philosophical views, claims about beliefs and preferences cannot be regarded as observational, but someone who has no quarrel with commonsense reality and then turns to assess economic theories must regard their claims about expectations and preferences as no more problematic than are claims concerning observables.[5]

As Richard Bradley pointed out to me, decision theorists might insist that 'preferences' are not the same things as ordinary wants and values and that subjective probabilities are not the same things as degrees of belief or commitment (Kaplan 1996, Chapter 5). Indeed it is perfectly possible to accept a folk-psychological view of human action as arising from beliefs and desires while denying that people's degrees of belief satisfy the probability calculus, or that people possess the complete and transitive preference orderings postulated by decision theorists. The resemblance between theoretical posits, such as subjective probabilities or expected utilities, and folk-psychological entities, such as degrees of

[5] Notice that I am only arguing that claims concerning subjective probabilities or expected utilities are no more problematic than claims about observables, not that subjective probabilities and expected utilities are observables. In this way the case here differs from Alexander Rosenberg's classic discussion (1976, pp. 142–52), although it remains in accord with his fundamental insight.

belief and intensities of wants, does not establish that the theoretical posits are identical to the folk-psychological entities, or that the existence of the posits and the truth conditions of claims concerning them are no more problematic than the existence of the folk-psychological entities and the truth conditions of folk-psychological claims. Whether subjective probabilities and expected utilities exist and whether we can have good evidence concerning claims about them are not settled by the respectability of folk psychology.

Part of what is claimed here is obviously correct. Nothing in folk psychology corresponds exactly to the economist's (or decision theorist's) notion of a preference ranking, and degrees of belief do not possess the precision or the coherence required of subjective probabilities. But are preference rankings and subjective probabilities new entities postulated by expected utility theory, in the same way that physicists postulate the existence of subatomic particles, or are preference rankings and subjective probabilities merely idealized variations on familiar notions of desire and belief?

I hold the latter view. Here are three arguments in defense of it. (1) The functional role of preference rankings and subjective probability judgments is virtually identical to the functional role of belief and desire. One predicts and explains actions in terms of beliefs and desires in the same way that one predicts and explains them in terms of expected utilities and subjective probabilities. (2) In testing and operationalizing rational choice theory, theorists rely on the associations between preference rankings and strengths of desire and between subjective probabilities and degrees of belief or commitment. In eliciting an agent's expected utilities, for example, one might take for granted that the experimental subject's belief that a coin is as likely to land heads as tails. (3) The plausibility of the axioms of rational choice theory depends on the close association between preference rankings and desires and between subjective probabilities and degrees of belief. These considerations do not, of course, establish that claims about preferences and beliefs are observable. Nor are they intended to. What they establish is that there is no principled epistemological divide between the beliefs and desires of everyday life and the subjective probabilities and utilities of economics. Claims about subjective probabilities and preference rankings are problematic because of their specific empirical difficulties, not because they invoke new categories of unobservable entities.

5.2 Other unobservables in economics

I do not mean to deny that economic theories ever postulate the existence of new and unfamiliar unobservables. Marx's labor values, as the amount of socially necessary labor employed to produce a commodity, seem to be unobservable entities. In the case of goods produced without any capital inputs, one could perhaps observe how much labor was expended in production, and one might be able to defend claims about how that labor was related to general, unskilled labor and to the amount of labor that was truly socially necessary. But this would be an exceptional case. In general, there is no way to observe socially necessary labor. Unfortunately, there is no non-arbitrary way to measure it, either, and little reason to regard labor values as economically significant.

More contentiously, one might argue that it is only equivocation that prevents one from realizing that contemporary economics is constantly referring to unobservable theoretical posits. When the theory talks about firms, consumers, markets, and entrepreneurs, is it talking about what economic agents mean by 'firms', 'consumers', 'markets', and 'entrepreneurs', or is it talking about theoretical posits that are misleadingly given familiar names? Until recently, the theory of the firm supposed that organizational structure was irrelevant, that every firm had a single objective (to maximize net returns), that every firm knew the production function for its outputs, and that it decided on the level of output and on how to allocate interchangeable units of inputs by maximizing the net revenue function derivable from the production function and the given prices of outputs and inputs. Do such firms bear any resemblance to McDonald's, Microsoft, or the corner-grocer, apart from employing inputs and supplying outputs and being called 'firms'? Similarly, 'consumers' in much of economic theory are not inflicted with the bad memories and quirky proclivities of real consumers. Despite recent work on auctions, the markets of most economic theories are either quiet places with given prices or strange scenes of tâtonnement. Entrepreneurs in economic theory have resembled divinities more closely than ordinary mortals. In this way one might argue that contemporary economic theory, like theories in the natural sciences, makes crucial references to unobservable things that lie behind the messy phenomena.

Obviously what neoclassical theorists until recently called 'firms' bore little resemblance to real firms. But does this show that there is an equivocation and that the relevant theoretical entity posited by neoclassical theory is only misleadingly called 'a firm', or does it show that neoclassical theorists have simplified and idealized the description of firms? Reflection on the practice of economics favors the latter interpretation. The theory of the firm has been criticized because of the inconsistencies between the claims it makes about 'firms' and what observation of firms reveals. Such criticism would be mistaken unless the 'firms' economists talk about were meant to be real firms, like IBM or Sears. Of course, one might maintain that such criticism is in fact confused, but notice that, partly owing to this criticism, the traditional theory of the firm has been superseded (except as a simplification in particular contexts) by a theory that pays heed to agency problems and transactions costs like those encountered by actual firms. Such progress would be incomprehensible if economists were not trying to talk about real firms. Similar claims can be made about markets, consumers, and entrepreneurs.

One might, however, argue that merely simplifying, abstracting or idealizing is itself enough to differentiate the concepts of theory from the concepts of everyday life. Fritz Machlup defends such a view:

> For example, the Census Bureau's concepts of 'industry', the legal or organizational concepts of 'firm', the accounting concepts of 'cost', these operational concepts cannot be substituted for the idealized concepts of the model; for 'demand' there is no operational concept at all; and no practically available operations can unambiguously identify one 'price' and one 'output' of a product in all its varieties of qualities, shapes, calipers, colors, finishes, lot sizes, delivery terms, credit terms, etc. 'Price' and 'output' in the theoretical model are 'exact' in the sense that they abstract from all the complications surrounding the price and output observations of actual business. (1960, p. 571)

The difficulty with this view is that it draws the line between what is observable and what is not in the wrong place. As Machlup himself notes, *everything* mentioned in theories – and even in detailed econometric models before the parameter values are estimated (1960, p. 573) – turns out to be unobservable. In that case, there is no reason why claims about firms or consumers or markets cannot be tested in just the same way that claims about prices or quantities are.

In macroeconomic theories one finds many quantities – such as the rate of unemployment or inflation, the level of prices or inventories, or the state of consumer confidence – that are certainly not directly observable. But these should not, I think, give rise to empiricist qualms similar to those to which quarks or neutrinos gave rise. The macroeconomic quantities are all averages of some sort, and although averages are not observable, they can be defined in terms of observables. The average height of the students in a particular lecture hall cannot be observed, but the height of each individual student can be, and the average can be calculated. As Julian Reiss pointed out to me, some macroeconomic quantities, such as the rate of inflation, are more problematic. Given interpersonal differences in consumption, changes in the qualities of commodities, introduction of new commodities, and changes in tastes and technologies, any measure of inflation is bound to be to some extent arbitrary. But I see little reason to assimilate these difficulties to epistemological questions concerning unobservables.

Some contemporary economic theories may postulate the existence of unfamiliar unobservables. Perhaps 'human capital' (Becker 1976) or 'attributes' (Lancaster 1966) would count, though even here one can display affinities to pretheoretical concepts. The important point is that such theoretical posits are relatively unimportant in economics.

It does not, of course, follow automatically that debates about the existence of unobservables and the truth of claims referring to unobservables are of little importance to economic methodology. If economists and economic methodologists have mistakenly believed that issues about unobservables are important in economics, then those who aim to sort out these mistakes need to address philosophical questions concerning unobservables. Indeed that is just what I am doing here. But economists and economic methodologists should eventually be able to move on to more relevant issues.

6. TRANSCENDENTAL REALISM

The controversy provoked by Friedman's views concerning the irrelevance of questions about the 'realism' (that is truth or falsity) of the assumptions of economics led to a good deal of discussion among economists in the 1960s and early 1970s concerning the goals of economics. Although briefly reopened by Lawrence Boland's (1979) defense of Friedman's instrumentalism, this discussion has petered out, without, in my judgment, reaching any satisfactory resolution. As economists gradually escape from their unreasonable animus against explicitly causal language, they should be able to recognize the ambiguities of instrumentalism that I mentioned above in Section 2, and a more fruitful discussion of the goals of economics will become possible.

The issues dividing realists and anti-realists, in contrast, have until recently not attracted much attention from economists, and given the thesis of the previous section – that economic theories rarely postulate the existence of new unobservables – this disinterest is entirely reasonable. Yet during the past two decades, two prominent economic methodologists – Tony Lawson and Uskali Mäki – have formulated what they proclaim to be realist programs in economic methodology. Why? They would not, I think, dispute the claim that economic theories rarely posit the existence of new unobservable entities. Indeed Mäki writes explicitly that 'economics, for the most part, deals with observational or commonsense entities' (1996a, p. 428). Why then do Mäki and Lawson nevertheless maintain that realism has something very important to offer economic methodologists?

One answer is that they are insisting on the importance of unobservable *mechanisms* and of *counterfactuals* rather than on the reality of unobservable *entities* or *properties*. An ordinary 'Hicksian' demand curve states a counterfactual relationship between price and quantity demanded, holding constant incomes, other prices, and tastes. If the demand relationship is identifiable, it is possible to estimate the demand curve from data concerning quantities sold at various prices. But the curve that results from applying regression analysis to such data bears only a distant connection to the counterfactual relationship one is trying to estimate. Tastes, incomes, and other prices are constantly changing, and in any case regularities across time between price and quantity are not the same thing as the atemporal counterfactual relation stated by a demand curve. What makes realism important to Lawson and Mäki could be the fact that economics identifies unobservable *mechanisms*, counterfactual relations, or unobservable underlying relationships. Realism is important even though economic theory does not consist of claims concerning unobservable entities or properties.

Tony Lawson's reasons for defending realism are explicitly related to these considerations. In particular, Lawson ties his case to a philosophical perspective developed by Roy Bhaskar (1978, 1979). Lawson begins with a minimalist characterization of scientific realism: 'In science, a realist position, i.e., a *scientific realism*, asserts that the ultimate objects of scientific investigation exist for the most part quite independent of, or at least prior to, their investigation' (1997, p. 15). On this view, most of the instrumentalists and anti-realists of this century are scientific realists. But Lawson is not arguing merely for realism; he is arguing for what he calls 'critical realism', which is closely related to what Bhaskar calls 'transcendental realism'.

> ... according to transcendental realism ... the world is composed not only of events and states of affairs and our experiences or impressions, but also of underlying structures, powers, mechanisms and tendencies that exist, whether or not detected, and govern or facilitate actual events ... on the transcendental realist conception, the different levels of reality are out of phase with each other. (Lawson, 1997, p. 21)

Lawson's realism asserts the existence of an 'underlying' independent reality of 'structures, powers, and mechanisms' that governs 'actual events' and that does not correspond in any simple way to actual events or states of affairs. Reality is structured and consists of three domains – experiences, events, and structures or mechanisms – that are 'ontologically distinct and irreducible' (1997, p. 22). Although structures and mechanisms generate events and are manifested in events, the relationships between the levels are intricate. Events are typically generated by a complicated multiplicity of mechanisms and structures, which for this reason cannot be 'read straight off' (1997, p. 22) from observation of events.

> Science, on this transcendental realist view, is no longer confined to, or even dependent upon, the seeking out of constant event conjunctions, but

> aims at identifying and illuminating the structures and mechanisms, powers and tendencies, that govern or facilitate the course of events. (Lawson, 1997, p. 23)

For Lawson realism is necessary both to accommodate an understanding of underlying mechanisms and to explain why contemporary economics fails. The central problem with contemporary economics, in Lawson's view, is that it is committed to searching for exceptionless regularities obtaining among observable entities and properties and to explaining and predicting with the help of such regularities. Since such regularities are not to be found, the efforts of economists are inevitably wasted. Only by adopting a realist perspective and looking 'beneath' the *irregularity* of phenomenal relations, can economics begin to make progress.

Lawson offers economists a false dichotomy. Either they can accept a view of science as exclusively the search for exceptionless regularities among observable events (glorified correlation-spotting), or they can accept critical realism. Lawson has embedded his central thesis – that economics should be a search for the structures and mechanisms that generate the typically irregular data that economists gather – in a controversial metaphysics, which, I suggest, distracts readers from his main concerns.

To see how problematic that metaphysics is, notice that the three domains or categories – experiences, events, and underlying structures (or mechanisms) – are neither distinct nor exhaustive. They are not distinct, because experiences are events and so are the operations of underlying structures and mechanisms. One can restore the distinctness of the three categories only by stipulating arbitrarily that events, unlike experiences, must be non-mental and that, unlike the operation of underlying mechanisms, events must be observable. The three domains are not exhaustive, because there are, at the level of phenomena, enduring objects, structures, and mechanisms.

Consider, for example, the mechanism whereby the market equilibrates supply and demand, which is arguably the most important mechanism in economics. The story told by Adam Smith and recounted by the economic fireside ever since goes roughly as follows: Some people are willing to pay more for certain commodities than are other people. Those who want things at the going price and those who would still want them at a higher price make purchases. When especially eager to have things or worried about getting them, those who would be willing to pay more bid up the prices. Those who own things want to sell them as dearly as possible. When their stocks are diminishing or they believe that people want more than they can supply at the going price, they raise the price. When the price goes up, some of those who originally may have wanted to buy the commodity decide not to purchase it, and in that way price is determined and the quantity demanded matched to the quantity brought to market.

However inelegant the description, this seems to be a mechanism. It is certainly a causal story. But it appears to involve observables. One might maintain that the market *mechanism*, unlike particular market *interactions*, is unobservable. One could say that it is only inferred from its consequences, as tables and chairs are only inferred from their perceptual consequences. But rather than defending the paradoxical view that tables and chairs are not observable, it would be better to distinguish questions concerning categories (Is something an event, state of affairs, substance, mechanism, or whatever?) from questions of observability. Metaphysical 'levels' of reality should, I think, be distinguished both from categories of entities and from issues of observability. What is gained by assimilating questions concerning the status of, for example, social norms to questions concerning the existence of electrons? The story about markets just told is not a description of an event regularity, yet it seems to be at the 'level' of everyday experience. The haggling of the market is not a 'transfactual reality', yet it is a mechanism none the less.

This criticism of Lawson's and Bhaskar's transcendental realism is no more than a sketch. My main point is not that transcendental realism is untenable (although I believe it is). My point is rather that the issues that concern Lawson are obfuscated by labeling them as questions concerning *realism*. Lawson has both a general and a specific concern. The specific concern is to defend his variant of Bhaskar's philosophy of science. In doing so, what divides Lawson from those who disagree with him, such as Cartwright, Blaug, Mäki, or myself, is not realism – all the parties to the controversies are realists. What is at issue are the details of Lawson's views of economic mechanisms.

Lawson's general concern is to defend the thesis that science aims to identify structures and mechanisms that may not be detectable in passive observation, even though in combination they generate the data humans observe. For example, in 'actual' firms, returns to variable inputs do not always diminish. If the law of diminishing returns said anything different, it would obviously be no law at all. It captures only one factor that generates the complicated phenomena observed. One does not have to be a critical realist to recognize this crucial point, and, as argued in the concluding section, realism is largely irrelevant to it. Lawson's emphasis on realism distracts attention from the real issues. Realism has little to do with Lawson's general concern to defend ambitious theorizing, and realism is not at issue in controversies concerning the details of Lawson's position.

7. LOCAL REALISM

Uskali Mäki takes himself to be articulating a realist program in economic methodology, but – at least in terms of the notion of realism developed in this essay – his self-description is hard to understand. He rejects what he calls the 'globalizing attitude' of many scientific realists, and he seems at least as interested in attitudes toward universals, essences, or social constructivism as in what I have called scientific realism. It may be that Mäki is not in fact addressing the issues discussed above that distinguish realism from instrumentalism or from constructive empiricism. Mäki's realist program might perhaps be better understood as an exploration of details of economic theories in the light of a variety of realist views, ranging from realism about universals or moral realism to the sort of humdrum realism that disputes the relativist theses of social constructivists and post modernists. The following discussion, which attempts to relate his program to the notions of realism discussed in this paper, may thus misconstrue his intentions.

Mäki maintains that one should contextualize 'the issue of realism' (1996a, p. 427). Rather than asking whether scientific realism is correct, one should ask whether one should 'hold realism about *T*' (1992, p. 38). '*[T]he grounds for realism are theory-type specific or approach-type specific* ... I do not think that science, or the set of scientific theories, is homogenous enough to warrant such a globalizing attitude' (1992, p. 39).

These remarks are puzzling. One puzzle is that one would have thought that a critique of a realist 'globalizing attitude' would come from a contextualist instrumentalist rather than from a self-proclaimed realist. A second puzzle arises as soon as one recognizes that it is no part of scientific realism to maintain that the entities postulated by any particular theory *T* exist, or that the claims *T* makes about them are true.[6] The existence of the entities and the truth of *T*'s claims about them are, according to the realist, empirical questions, for which evidence may sometimes be available. The only globalizing attitude realism involves is that it makes sense to ask whether entities postulated by scientific theories exist and whether claims about them are true. It is up to science,

[6] Some formulations might suggest otherwise. For example, Richard Boyd (1983, p. 195) takes scientific realism to include the view that 'Scientific theories, interpreted realistically, are confirmable *and in fact often confirmed* as approximately true by ordinary scientific evidence interpreted in accordance with ordinary methodological standards'. Even this formulation does not commit the realist to any judgment about the extent to which any particular theory is confirmed. Michael Devitt (1984, p. 22) defines scientific realism as, 'Tokens of most current unobservable scientific physical types objectively exist independently of the mental'. This mistaken characterization would make an anti-realist out of someone who was skeptical about half of current theories. Notice that Devitt's loose formulation seems to imply that scientific realists are automatically physicalists.

not to scientific realism to determine which theories are true and which entities exist.

Perhaps Mäki takes scientific realists to hold the view that the *only* point of a theory is to learn the truth and that consequently theories that postulate non-existent entities or make false claims are worthless. One might then read him as arguing that particular theories may have different purposes and that different criteria of assessment may be appropriate to different cases. But a globalizing realist who disputes this is a straw man. No economists or economic methodologists that I know of endorse such a view. Realists are great fools if they do not recognize that theories can serve many purposes and that for some purposes, it may not matter whether a theory is true or false.

The only globalizing attitude held by the realist is that whether postulated entities exist and whether claims about them are true are real questions, for which evidence may be available. In 'contextualizing' realism, Mäki seems to take scientific realism for granted. Otherwise there would be little point in asking for example, 'whether Austrian theory is the kind of theory that may be true and the logically prior question of whether the entities it postulates may exist' (1992, p. 36).

What contextualized question concerning realism can one ask, apart from asking whether the entities a theory *T* purportedly refers to exist and whether the claims *T* makes about them are true? Mäki holds that quite apart from considering the evidence, one can ask whether the entities *T* postulates are the sort of thing that *could exist* or whether the claims the theory makes about them *could be* true or false. Just as philosophers have separated questions concerning realism in science from questions concerning moral or aesthetic realism, so one might distinguish among kinds of practices or among kinds of theories within science. One might maintain that different practices have different goals and that questions about the truth or falsity of postulated entities only arise for some kinds of theories and not for others. Just as it makes sense for someone who is a scientific but not a moral realist to ask whether a particular theory is a scientific theory and thus possibly true or false, so it makes sense for someone who is a realist about only certain kinds of theories to ask whether particular economic theories are of a kind that could be true or false.

In the essay just cited, Mäki argues that entities postulated by Austrian theorists, such as entrepreneurs or the mechanism of the invisible hand, are the sort of thing that might exist, while it is more questionable whether the entities postulated by general equilibrium theorists could exist. The reason is, Mäki maintains, that Austrian theories, unlike general equilibrium theories, attempt to capture a process. For example, general equilibrium theories typically either say nothing about the process of price determination, or they offer a fictitious story of tâtonnement, in which a hypothetical auctioneer announces prices, and agents announce how much of each commodity they will offer or demand at the price announced. The auctioneer then adjusts prices up or down, makes another announcement, and the process continues until a set of equilibrium prices is found.

Obviously the tâtonnement story is not meant to be taken literally. Nobody thinks there are such auctioneers announcing prices and processing responses from all agents in the economy. But there is no metaphysical divide (analogous to the difference between facts and values) between the subject matter of process theories and that of equilibrium theories. The question, 'Is the tâtonnement story true?' is easily answered and seems to involve no philosophical mistake. Mäki might respond that the question is so easily answered that asking it reveals a failure to understand that nobody intends this to be a true story. The purpose of the theory is not to explain how prices are actually determined. If this reconstruction captures Mäki's intentions, then it seems that in asking whether 'a realist reading of a theory' (1992, p. 38) is appropriate, he is raising a question about how the theory is understood or intended by some group of economists. There's nothing puzzling about this question, as a question for a sociologist or historian of science, but it is hard to see it as a philosophical or methodological question.

It is unclear whether Mäki would take issue with my suggestion that the epistemological issues that divide realists and anti-realists (with whatever semantic or ontological implications and associations they may have) are largely irrelevant to economic methodology. In earlier papers (especially Mäki, 1990), Mäki seemed to hold that realism is important to economics on the grounds that economic methodologists should take causal mechanisms such as the invisible hand as real. One sees an occasional echo of this view in recent work (for example, 1996b, p. 22), but despite the prominence of the term, 'realism', Mäki is not defending a program that aims to compete with programs of anti-realist economic methodologists, and his concern to question a globalizing realist attitude seems more consonant with a sophisticated contextualist instrumentalism than with realism.

8. CONCLUSIONS: PROBLEMS WITH REALISM IN ECONOMIC METHODOLOGY

Much of what bothers me about the realist programs of Lawson and Mäki may be as much expositional as substantive. To label one's program for economic methodology as 'realist' inevitably suggests that the competing programs are not realist or fail to be realist enough. In the case of economic methodology, this suggestion is misleading, because there is no anti-realist school of economic methodology, and there are few methodologists (as opposed to economists) who are instrumentalists either. What is distinctive about Lawson's and Mäki's programs is not realism – which they share with the rest of economic methodology – but something else. That something else can, of course, be a particular formulation of realism, such as Lawson's critical realism. But it would be less misleading if what was distinctive was characterized in terms of what distinguishes it from alternatives, rather than in terms of what it shares with them.

Although this is partly a matter of taste, I would maintain that there are advantages to avoiding characterizing problems in terms of multiply ambiguous labels such as realism. Consider Mäki's entry on 'Realism' in the new *Handbook of Economic Methodology*. It is a learned introduction to philosophical uses of the term. The first four of its five pages sketch different kinds of realism and offer capsule accounts of existence, reality, and truth. At the end of the entry are three paragraphs on realism and economics (1998, pp. 408–9). The first begins with the question, 'Do realism and economics fit together?' Since there are different understandings of economics and dozens of different realisms, the question is, I believe, too ambiguous to be worth posing. (As an example of a version of realism that does not fit economics, Mäki mentions 'radical physicalist scientific realism', which holds that only entities postulated by physical theories are real.) The second paragraph begins with the claim that 'a number of economists have been shown or can be shown to subscribe to one or another form of realism'. Without further specification, there is little substance in this claim. Given the many varieties of realism, every economist is a realist of one kind or another. The demonstrations Mäki has in mind show what specific kind of realism economists have espoused, such as Mäki's own argument that Menger was a realist concerning universals (Mäki, 1990).

At this point Mäki finally tries to say something about how issues concerning realism might be relevant to economics:

> There are some special features regarding realism about economics, such as commonsense realism playing a prominent role ... Another feature, and an epistemologically significant one, is that the simplified and isolated settings theoretically brought about by economists usually cannot be reproduced empirically, thus making the empirical testing of truth claims particularly difficult. (1998, p. 408)

The importance of 'commonsense realism' in economics implies, as I have argued, that the epistemological (and hence semantic and ontological) issues that have divided scientific realists and anti-realists are largely irrelevant to economics. It counts against the relevance to economics of discussions of scientific realism rather than for their relevance. The practical and moral difficulties of implementing experimental isolation of relationships, on the other hand, seems to have little to do with realism one way or the other.

In his last paragraph, Mäki turns to the 'two major realist projects', about which he cannot of course say much in a few sentences, and concludes by noting that other economic methodologists and philosophers 'have contributed to the realist project without necessarily doing it explicitly under the banner of "realism"' (1998, p. 409). But, as the entry itself makes clear, there is no such thing as *the* realist project. Instead there are a great many possible realist projects, and it is hard to imagine anything written about economic methodology that does not contribute to one or another of them. Instead of revealing important features shared by different inquiries, labeling them as inquiries concerning realism reproduces the ambiguities of the term. What does one gain by talking about realism, apart from the burden of distinguishing the sort of realism that is relevant from all the other kinds? In most instances it seems to me that economists have little to gain.

Lawson would, I think, disagree. He believes that progress in economics has been impeded because economists have not adopted the proper realist perspective. They need to recognize that universal regularities cannot be observed in economics (and indeed they are in short supply elsewhere, too). So spotting them cannot be the task of science. Furthermore, many of these regularities cannot be formulated correctly without employing causal language.[7]

These facts justify pessimism about the project of hunting for correlations, and the historical experience of economists and sociologists reinforces this pessimism. The only hope one has of explaining and predicting social phenomena is to construct theories. This conclusion does not, however, justify Lawson's insistence that economists need to accept realism, because the importance of theory is not a matter of contention between realism and instrumentalism or between realism and anti-realism. As already noted, *instrumentalists are not operationalists or behaviorists* (see also Mongin, 1988, p. 322). Behaviorists insist that one stick with generalizations cast at the level of observation. Instrumentalists and anti-realists such as van Fraassen, in contrast, welcome speculative theorizing whenever it helps one to make predictions concerning matters of observation. (Thus, Machlup (1964) has no sympathy with Samuelson's operationalism (1963).) So there is nothing

[7] For example, the law of demand holds that a higher price of a commodity will *cause* people to want to buy less of it. It does not maintain that there is an inverse relation between price and quantity regardless of the causal order. When the cause of a price increase is an increase in demand, there is no inverse relation between price and quantity demanded.

in the recognition of the need for theorizing that suggests that economists need be concerned about realism.

A better case for the importance of realism might be based on the role of specifically causal considerations in economics. Instrumentalists and anti-realists do not talk of causal powers or capacities literally, and indeed the Humean notion of causation most of them accept is a thin and watery thing. The only difference between a case where a price change causes a change in the quantity demanded and a case in which a change in the quantity demanded causes a price change lies in the time order. It is hard to see how time order could make so much difference to explanation. Realists, in contrast, can recognize the existence of causal capacities, mechanisms, and powers that lie 'beneath' the phenomena and explain the bits of partial regularity that rear their heads here and there.

This is a good reason to be a realist about causes (or at least to accept a strong view of laws). But the emphasis on *realism* is misplaced. What is at issue is the interpretation of the fundamental 'principles' of economics; and this is a quarrel *among realists*, not between realists and anti-realists. Consider a principle such as, 'Agents prefer larger bundles of commodities to smaller', which most economists would regard as fundamental. Principles such as this one have the following properties:

1. They are not, as stated, true universal generalizations.
2. They do not postulate unobservable entities or properties.
3. Corresponding to them is an observable rough regularity.
4. They apparently state a causal tendency or 'force' that can be augmented or diminished by other causal factors.
5. Because of (4), their predictive value is limited (though not negligible), while their explanatory power (though not uncontroversial) seems substantial.

Figuring out what to say about such principles is a difficult philosophical task, and economic methodologists have defended a variety of positions. Disagreements concerning these positions are what divide Lawson, Mäki, and others – like me – who do not hoist the banner of 'realism', but who are in most cases realists all the same. The difficulties involved in interpreting the principles of economics are aggravated when the issues are recast as questions about realism. The question is not whether we should be realists or not, but what should we make of the principles with the peculiarities of those in economics.

Not all philosophical issues are equally germane to every science. The epistemological questions that divide realists from anti-realists are much less pressing in economics than they are in sub-atomic physics. Like Mäki and Lawson, I believe that a realist view of causes is needed to make sense of economics, but the issues that interest economic methodologists – concerning the 'principles' of economics – are largely orthogonal to questions about realism and could be more clearly addressed if they were not entangled with questions about realism.

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