

# **Two Responses to the Failings of Modern Economics: the Instrumentalist and the Realist**

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**Abstract** Realist and instrumentalist approaches are compared in the context of looking to overcome the problems of modern economics. It is argued that the failings of the modern discipline can be resolved only by way of adopting a realist orientation.

## **1. Introduction**

Here I contrast instrumentalist and realist projects in modern economics, as is my remit. But I do not do so in a purely abstract fashion, out of context. Rather, I examine and compare these two projects as possible resources for dealing with the somewhat unhappy situation in which the modern discipline of economics finds itself. My concern is with the illumination of social reality. And this is an endeavour in which I believe the modern academic discipline of economics, and in particular its hugely dominant mainstream component, are rather less than successful. In what follows I examine how instrumentalists and realists might, and do, respond to this situation. I adopt this strategy because many who think about methodological issues seem to suppose that the two positions, whatever their philosophical differences, amount to much the same thing in practice. I intend to show this is not so. Although my primary purpose is thus to demonstrate that the two approaches part company—and show how—I will also indicate why I regard the realist response as the more sustainable.

### **1.1. The Context**

There is little doubt that the modern discipline of economics is in a state of some disarray. Or at least this is true of its hugely dominant modern mainstream component. By the latter I simply mean that which is concerned in a central way with formalistic modelling of some kind. Why do I suppose that formalistic modelling is the essence of the modern mainstream? Notice that as substantive fashions come and go, the mainstream orientation is held to continue intact.

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There is no basic shift in its membership, or in the selection of journals or lecture courses regarded as core. And the one feature that endures, and is continually recommended (see Kirman 1989) throughout the project's various, clearly nominal, transformations, is an adherence to formalistic modelling. Indeed, for many modern economists, economics is the formalistic modelling of social phenomena—there are no alternative (non-modelling oriented) traditions (see e.g., Gee 1991; Strassmann 1994<sup>1</sup>).

As I said, if the formalistic modelling project, or the deductivist enterprise it sustains (see below), is constitutive of the modern mainstream position, it is also a project that is not in a particularly healthy state. Although rarely revealed in opening chapters of introductory text-books and the like, the problematic nature of modern mainstream economics, continually emphasised by opponents of that project of course, is often acknowledged even within the mainstream tradition itself, especially, but not only, when its leading proponents provide reflective overviews of the discipline in presentations prepared for special occasions. On such occasions, it is seemingly readily admitted that the mainstream project is, for example, poor at forecasting (e.g. Kay 1995); unrealistic (Hahn 1994); arbitrary (Leontief 1982), without clear direction (Rubinstein 1995; Kirman 1989); riddled with inconsistencies (Blaug 1980; McCloskey 1986; Leamer 1978, 1983; Hendry, Leamer and Poirier, 1990); in crisis (Bell and Kristol 1981), and; basically in a state of disarray (Wiles and Routh 1984).

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<sup>1</sup> Strassmann has captured the situation well in the course of promoting a feminist alternative approach to economics:

“To a mainstream economist, theory means model, and model means ideas expressed in mathematical form. In learning how to “think like an economist,” students learn certain critical concepts and models, ideas which typically are taught initially through simple mathematical analyses. These models, students learn, are theory. In more advanced courses, economic theories are presented in more mathematically elaborate models. Mainstream economists believe proper models—good models—take a recognizable form: presentation in equations, with mathematically expressed definitions, assumptions, and theoretical developments clearly laid out. Students also learn how economists argue. They learn that the legitimate way to argue is with models and econometrically constructed forms of evidence. While students are also presented with verbal and geometric masterpieces produced in bygone eras, they quickly learn that novices who want jobs should emulate their current teachers rather than deceased luminaries.

Because all models are incomplete, students also learn that no model is perfect. Indeed, students learn that it is bad manners to engage in excessive questioning of simplifying assumptions. Claiming that a model is deficient is a minor feat—presumably anyone can do that. What is really valued is coming up with a better model, a better theory. And so, goes the accumulated wisdom of properly taught economists, those who criticize without coming up with better models are only pedestrian snipers. Major scientific triumphs call for a better theory with a better model in recognizable form. In this way economists learn their trade; it is how I learned mine.

Therefore, imagine my reaction when I heard feminists from other disciplines apply the term *theory* to ideas presented in verbal form, ideas not containing even the remotest potential for mathematical expression. “This is theory?” I asked. “Where’s the math?” (1994, p. 154).

Thus, for example, on examining the record of 34 forecasting groups in the United Kingdom, including the most quoted ones, Kay (1995) admits:

“Economic forecasters do not speak with discordant voices; [keeping an eye on each other] they all say more or less the same thing at the same time. And what they say is almost always wrong. The differences between forecasts are trivial relative to the differences between all forecasts and what happens” (p. 19)

Leontief puts things rather more forcefully:

“Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions...Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system” (Leontief 1982, p. 104).

And Rubinstein, in a paper celebrating the award of the Nobel Memorial Prize to John Nash, worries how modern economic theory is to be interpreted, and even about its purpose and direction:

“The issue of interpreting economic theory is, in my opinion, the most serious problem now facing economic theorists. The feeling among many of us can be summarized as follows. Economic theory should deal with the real world. It is not a branch of abstract mathematics even though it utilises abstract tools. Since it is about the real world, people expect the theory to prove useful in achieving practical goals. But economic theory has not delivered the goods. Predictions from economic theory are not nearly as accurate as those by the natural sciences, and the link between economic theory and practical problems, such as how to bargain, is tenuous at best. Although I have never heard an economist seriously claim that the Nash bargaining solution is a good predictor of bargaining in real markets, this solution is a standard tool in modelling interactions among negotiators. Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question ‘Where does it lead?’” (1995, p. 12).

In the following pages I compare different responses to this unfortunate situation. It is clear that at the heart of the noted problems lie questions about choice of method, so that serious reactions are bound to be methodological in some way. Here I want to compare two types of methodological resort: the first, which appears to carry a good deal of favour amongst modern mainstream economists, albeit often only implicitly, can be termed instrumentalist; the second, almost entirely avoided by modern mainstream economists, I designate realist. My aim is to draw out significant differences between these approaches and to indicate why I regard the latter to be the better grounded and more useful. I will define my various categories and terms in the course of considering the types of response I have in mind. It is the instrumentalist reaction that I turn to consider first.

## **2. Instrumentalism in Modern Economics**

I understand by instrumentalism the thesis that theories are to be interpreted merely as practical tools or instruments for some purpose other than causal explanation. That is, although the construction of theories, or hypotheses or models, etc., serves some practical purpose or aim, this does not include or necessitate giving true accounts of the structure of the world. The qualification is essential. We all use theories as instruments of some kind, even if we wish to give a causal explanation of observed events. In this sense we are all instrumentalists. Instrumentalism is used as a label when the role of the theory as an instrument is limited in some way, to some practical activity other than describing the structure of reality.

It is the case that not everyone who adopts an instrumentalist orientation admits explicitly to it, of course. Here I must do some reconstructing. And given that most economists appear not to be especially reflexive about their positions—indeed many seem to oppose methodological reflection on principle—it is obvious that, in imputing to them a philosophy-of-science orientation, I must operate a principle of charity. I must consider their seemingly strongest position(s). Of course, it is always essential to treat one's opponent charitably anyway.

Several variations on the instrumentalist theme are conceivable, and indeed in evidence. The most common statement of purpose found in modern economics is that economic (formalistic) theories are instruments for generating (presumably successful) predictions of actualities, the actual course of events or states of affairs. I include under this head the goal of accommodating such actualities after, as well as before, they have occurred.

## **2.1. Instruments That Are Neither True Nor False**

Within this form of predictive instrumentalism two sub-groupings are to be found. The first, and more traditional version, at least within philosophy, but the less common of the two in modern economics, has it that theories, in serving as instruments, are neither true nor false. This conception is discussed, for example, by Popper and Shionoya and, according to the latter, acted upon by Schumpeter in his interpreting of economic theory. Thus, Popper writes:

“By instrumentalism I mean the doctrine that a scientific theory...should be interpreted as an instrument, and nothing but an instrument, for the deduction of predictions of future events (especially measurements) and for other practical applications; and more specifically, that a scientific theory should not be interpreted as a genuine conjecture about the structure of the world, or as a genuine attempt to describe certain aspects of the world. The instrumentalist doctrine implies that scientific theories can be more or less useful, and more or less efficient; but it denies that they can, like descriptive statements, be true or false” (1983, p. 111–112)

And Shionoya concurs:

“...we should understand the central claims of instrumentalism broadly as the view that, first, with regard to the role of theories they are merely tools, and second, with regard to the cognitive status of theories they are regarded as neither true nor false” (1990, p. 194)

On one occasion Schumpeter explicitly refers to hypotheses as instruments:

“Economic theory...takes [certain hypotheses, etc.] for granted...Now hypotheses of this kind are also suggested by facts—they are framed with an eye to observations made—but in strict logic they are arbitrary creations of the analyst. They differ from [explanatory hypotheses]...in that they do not embody final results of research that are supposed to be interesting for their own sake, but are mere instruments or tools framed for the purposes of establishing interesting results [apparently meaning known empirical facts]” (1994 [1954], p. 15).

Shionoya himself notes the following two passages, amongst others by Schumpeter:

“The absolute truth of our hypotheses does not matter. Hypotheses do not belong to a part of results we have to defend, but are simply methodological auxiliary means whose value can only be judged from their fruitfulness. The role of hypotheses is merely formal; even if it were proved that they are themselves true, nothing would be gained from it for our laws” (Schumpeter, quoted in Shionoya, 1990, p. 206).

“It is extremely important for a proper understanding of our theory and, eventually, every theory to be aware of this arbitrary character of a theory and not to seek in it an expression of some ‘absolute’ truth...It is a method for description and nothing else; and as such it must be judged and organised for good or evil...A theory consists only of a measure which simplifies description and prevents it from becoming hopelessly complicated...If one realizes that only the goal justifies a theory, namely that only the result warrants it, then many objections and claims blocking our path will be removed” (Schumpeter, quoted in Shionoya, 1990, p. 206).

## **2.2. Instruments as Not (Necessarily) True**

In truth, it is not obvious to me that these quotes by Schumpeter are entirely consistent with the idea that theories cannot be regarded as true or false, as opposed merely to allowing that falsity does not matter. In any case this brings me to the second, and by far the most common, version of instrumentalism in modern economics. This is the idea that theories can be considered to be either true or false, but that their being true is not essential to the enterprise; typically their simplicity or economy or efficiency as instruments for generating predictions is considered of primary importance.

Philosophical traditionalists may not wish to consider this a form of instrumentalism. But in that theories so interpreted are accepted merely because they serve as limited-task (non-explanatory) instruments, and the truth status of such theories is considered to be irrelevant to achieving the adopted task in question, I think this perspective can be accepted as instrumentalist, given our not unusual understanding of this position. In any case, in order to strengthen my opponents hand as much as possible, to give it greater flexibility in its response to failure, I shall here include such responses under the instrumentalist head.

Certainly, the interpreting of such a position as instrumentalist is a common enough practice amongst economic methodologists. Thus, for example, the entry on instrumentalism in the recent *Handbook of Economic Methodology* contains the following statement:

“A moderate version of instrumentalism says that theories do possess such semantic properties [as referring or being true or false], but that these properties are irrelevant, the relevant being pragmatic. According to this version, it does not matter even if the terms of a theory fail to refer to anything real (even though they might so refer) or if the claims of a theory are false (even though they might be true). Reverential and veristic success—even if possible—does not add to the value of a theory. Only the pragmatic virtues of theories matter. Methodological instrumentalism of this sort incorporates semantic realism about reference and truth” (Mäki 1998, p. 254).

The best known example of an economist who makes explicit statements in support of such a position is Friedman (1953), of course. Although Friedman’s methodological essay is sufficiently inconsistent to sustain a number of competing interpretations (see for example, Hausman 1998, p. 192; Shionoya 1990, p. 208), some of its better known features do lend themselves to an instrumentalist reading. For example, Friedman suggests that a false theory can be more useful than, and is to be preferred to, a true one; and states the purpose he attributes to a theory as follows:

“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).

Moreover, whether or not Friedman is consistent in this, numerous economists appear to suppose that he has sufficiently justified their own acknowledged-to-be-fictitious or superficial theoretical claims. In any case, the prominence of theories not held as realistic, and indeed admitted as at best superficial, is evident. Lucas provides another example:

“To observe that economics is based on a superficial view of individual and social behaviour does not seem to me to be much of an insight. I think it is exactly this superficiality that gives economics much of the power that it has: its ability to predict human behaviour without knowing very much about the make up and lives of the people whose behaviour we are trying to understand” (Lucas 1986, p. 425).

Quite often, especially amongst econometricians, the acknowledgment that theories and/or models are false, yet possibly good instruments, is coupled with the assessment that the derivation of true theories or models is, in any case, impossible. Behind such a stance appears to be an acceptance of something like

a correspondence theory of truth. Econometricians seem especially fond of repeating the quip that a true model would be as useless as a map drawn to the scale of 1 to 1. This is a misconception of the nature of truth, of course, as I have argued at length elsewhere (see especially, Lawson 1997, chapter 17). Here, though, the point remains that, however good or bad the reason, numerous (perhaps almost all) econometricians appear to conclude that models can be empirically adequate and useful, yet inevitably false.

Thus, for example, Hendry (1983, p. 70), suggests that “A ‘true’ model is virtually a self-contradiction as it would have to be as complicated as whatever it is supposed to represent.” Elsewhere he elaborates:

“I take it as self-evident that economic behaviour is sufficiently complex and evolutionary that it is not helpful to talk about economic theories being ‘true’ or of inferences yielding correct results...By their very nature, models are inherently simplifications and inevitably false” (Hendry 1986, 30–1).

And more recently McAleer (1994) repeats this view whilst illegitimately tying the term “abstraction” to falsity: “Models are abstractions from reality and, as such, there are no true models” (p. 324). Further on he adds: “Econometric modelling is not intended to arrive at the truth, but an adequate representation of the data” (p. 328). And so on.

### **2.3. Changing the Goal Posts Entirely**

A further distinction that can be drawn within the instrumentalist camp is between those who interpret theories as instruments for (if not necessarily exclusively) predicting, including accommodating, actual phenomena or the ‘facts’, and those who assign an altogether quite different purpose to them. Some modern ‘theorists’, for example, view economic modelling merely as a form of logic, accepting only the aim of examining the deductions which admitted-to-be absurd assumptions condition.

Hahn’s understanding of modern economic theory, for example, can sometimes be interpreted as falling into the latter category. Certainly, Hahn sees his project as different to that of the predictive instrumentalist. Referring explicitly to Friedman and his followers, Hahn writes:

“The economists I have been discussing might be taken to be engaged in the following programme: to enquire how far observed events are consistent with an economy which is in continuous Walrasian equilibrium...[Even if this programme was successful]...it would not be true that we understood the

events. For we would not understand how continuous equilibrium is possible in a decentralised economy and we do not understand why a world with Trade Unions and monopolies behaves like a perfectly competitive one. Theorising in economics I have argued is an attempt at understanding and I now add that bad theorising is a premature claim to understand.

What has happened in this instance is that ‘as if’ prediction methodology has taken over. Recall Friedman’s example...” (1985, p. 15)

Elsewhere Hahn elaborates:

“When a mathematical economist assumes that there is a three good economy lasting two periods, or that agents are infinitely lived (perhaps because they value the utility of their descendants which they know!), everyone can see that we are not dealing with any actual economy. The assumptions are there to enable certain results to emerge and not because they are to be taken descriptively” (1994, p. 246).

“What a rational expectations theory provides is an understanding of an imagined economy which satisfies the assumption. As such it may be of great use. For instance, it allows us to study pathologies which cannot be traced to expectational mistakes. Or again it leads to an understanding of how market variables by revealing information to the uninformed reduce (or negate) the benefits of special information. Or yet again it allows us to grasp the informational disturbances introduced by an unknown monetary policy. It is a vulgar misunderstanding of theory and its aims to dismiss Rational Expectation theorising because of its assumptions” (1985, p. 11–12)

However, with neither the assumptions nor the derivations considered to have empirical relevance, it is difficult to interpret such projects as being concerned with social illumination at all<sup>2</sup>. The goal posts have been changed so much that we appear to be involved in a different activity entirely. In particular, if it is shown that X implies Y, and if X is regarded not as a real possibility, it is difficult to understand the point of the exercise interpreted as one of (causal) logic, especially as Hahn

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<sup>2</sup> There are some that claim that although theory and derivations are not empirically grounded; nevertheless, the results do somehow provide insight into reality. Perhaps the best known, and most developed example of this is formulated by Menger, with his notion of exact laws. This is a theme I consider in Lawson, 1997 (chapter 9) and one that cannot really be examined here. Suffice it to say, however, that Menger does not succeed in making a case that illumination can in this way be achieved. Rather his argument amounts to little more than a matter of hope and faith conditioned only by a misunderstanding of certain aspects of natural science.

explicitly denies that Y occurs, or that the realisticness of Y is even relevant. In any case, I have considered the relevance of this approach elsewhere (Lawson 1997, chapter 9). Because it is not obvious that the concern is with illuminating a world such as that in which we live, I do not further consider this particular project here.

#### **2.4. Instrumentalism to the Rescue?**

So how does, or how might, the adopting of an instrumentalist stance provide a way of dealing with the problems of the modern discipline noted earlier? As I have already said, if we adopt the sort of interpretation described by Hahn we change the nature of the project entirely. The goal is no longer social illumination of any kind. Thus, I do not consider this project further here<sup>3</sup>.

What about the range of stances I have collected under the head of predictive instrumentalism? How is, or how might, a turn to instrumentalism, interpreted as a concern with providing an instrument for accommodating data on observables, be seen as a response to the widely recognised disarray of the modern discipline? For current purposes, I think we can treat together those who view theories as neither true or false and those who believe that the falsity or unrealisticness of theories is not of primary, if any, significance. Both are concerned with theories to the extent that they cover the 'facts about observables of interest' irrespective of the nature of any claims, whether or not about unobservables, that figure as parts of the assumptions, axioms, hypotheses, etc.

Now the basic point is that, by adopting such an instrumentalist stance, the tensions within modern economics, and worries about lack of realisticness or arbitrariness of theories, etc., are not so much resolved as dissolved. The puzzles and tensions and so on, are simply accommodated. Instead of overcoming the problems, the decision is made to live with them in a relaxed way.

Most obviously, worries about the realisticness of the claims expressed within theories are rendered unproblematic more or less by assumption or definition. How about the worry that econometric theory and practice are mutually inconsistent? This mostly turns on the fact that empirical models are generated in ways at

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<sup>3</sup> Instead of attempting to consider the world in which we live the theory project in question basically examine the logical possibilities of "worlds" we have no reason to suppose could ever come about. In other words, the problems of the discipline are solved by changing the goal of the project entirely. It does not matter if our theories are known to be false in this world, the aim is merely to see what would follow in worlds where they did hold. On this conception, the disarray of the discipline might be said to be in the eye only of the beholder concerned with accommodating the facts as recorded, with covering actual observations. However, as I have already said, because such approaches are not concerned with illuminating our social reality, because they change the goal of the enterprise entirely, and because I am here concerned only with projects oriented to being useful in our world, I shall not concern myself further with these purely logical exercises here.

odds with the methodological theory. But, as such methodological theory is typically premised on the idea that the aim is to assess any theory or hypothesis to see if it is false, and as the truth status of any theory is now interpreted as, at best, irrelevant, it does not really matter that the methodological injunctions are bypassed. All that is required is a model that accommodates the data.

Furthermore, if theories are merely instruments, it does not matter either that they appear from some perspective as arbitrary, or that in their construction there is no obvious direction to the scientific enterprise, at least concerning the nature of the theoretical assumptions elaborated. Indeed, most of the apparent problems identified at the outset are seen to be rendered ultimately non-perturbing. In the end, a predictive instrumentalist might claim, any idea of a crisis arises only amongst those who do not recognise (or who momentarily forget) the instrumentalist nature of the modern mainstream project. Such an attitude, for example, seems to be well expressed by Schumpeter:

“the crucial point, upon which all depends, lies in the distinction between two different aspects of the matter: on the one hand, we have the fundamental arbitrariness of theories, on which their system, rigor, and exactness are based; on the other hand, we have the conformity of theories to, and their dependence on, phenomena, and this alone gives content and significance to theories. If one distinguishes between these moments and places them in a proper relation with each other, a clear interpretation will arise and thus the difficulties and doubts which we come across in the usual discussions of these questions will be effectively overcome” (Schumpeter, quoted in Shionoya, 1990, p. 204)

## **2.5. The Question of Empirical Adequacy**

The problem with all this, however, is that even the acceptance of an instrumentalist stance fails, in the end, to get around one central problem—that of the dearth of empirical success—that the theories or models of modern mainstream economists continue to perform badly at the empirical level. Even if running thousands of regressions facilitates a model specification which accommodates the available sample data, each model so obtained is almost always found to perform badly out of sample, and is thereby effectively rendered useless. Predictive instrumentalism is a response that neutralises all the usual seeming puzzles or tensions so long as economic theories can be interpreted as instruments successful at accommodating the data. Unfortunately, it is not a response that in itself dissolves or renders unproblematic the failure of theory to fit the data in practice. Rather, it is a response usually accepted in conditions where this problem has already been solved.

At this point, in the face of empirical failure, the instrumentalist has little recourse

but to carry on searching for a theory or model that is empirically more adequate. The methodological injunction can only be, and is always, to try a little harder.

Moreover, if consistent, the instrumentalist cannot really suggest in any systematic way how further searching might proceed. It is true, as already noted, that some instrumentalists emphasise certain pragmatic criteria, such as simplicity or parsimony, or whatever (see Shionoya 1990, p. 212). But these serve not as substitutes to the criterion of empirical adequacy, but as supplements. The presumption remains that theories must fit those facts for which they are predictive instruments. Additional pragmatic criteria such as simplicity, parsimony, etc., are relevant not to this endeavour but to that of selecting amongst hypotheses already considered to be empirically successful with respect to the relevant data. Their purposes is with ranking potential instruments not their identification<sup>4</sup>.

It must be admitted that there are various economists who, though proclaiming either that models cannot be true, or that issues of truth and falsity are irrelevant, do nevertheless suggest ways of proceeding in the face of empirical failure. Usually, their recommended strategies are found to involve one or both of i) adding more variables to the model, and/or ii) digging deeper, searching for regularities at a more disaggregated, micro or atomistic level (see discussion in Lawson 1997, chapter 7). However, such responses tend to be poorly articulated, and, as I say, from the instrumentalist perspective, difficult to render coherent anyway.

To sum up on the instrumentalist response, it is clear that instrumentalism does offer a strategy for handling most of the tensions, difficulties or puzzles of modern economics. But it does not provide much help with overcoming the problem of poor empirical fit. Moreover, the methodological consequences it does bear in such, or indeed any, circumstances are inherently conservative; the demand can only be for more of the same. The chief stipulation is to carry on looking, to search always that little bit harder. The interesting question, clearly, is whether a realist orientation can take us any further.

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<sup>4</sup> It is illuminating to consider Shionoya's assessment of Schumpeter's strategy in this respect. Shionoya writes:

"In *Wesen* Schumpeter repeatedly speaks of the 'results' of hypotheses and theories; hypotheses and theories are to be assessed by their usefulness in producing 'results.' What, then, are 'results'? He asserts that fitness to reality is a necessary condition for a theory to be useful. The fitness of theories to reality refers to the ability of theories to describe, predict, and explain phenomena...The condition that theories fit or be consistent with observable facts, however, cannot be the exclusive criterion for ascertaining the usefulness of theories; it is also important to see how consistent they are with the facts, because...facts allow an infinite number of mutually incompatible theories. Other criteria must be invoked, therefore to chose among theories which are all more or less consistent with facts. One may bring in ad hoc criteria such as simplicity, generality and the like. But Schumpeter is consisting in defining the usefulness of hypotheses and theories by applying Mach's principle of economy of thought to the appraisal of theories that are all equally regarded as fitting the facts" (Shionoya 1990, p. 212)

### 3. Realism in Modern Economics

I turn, then, to examine the sort of response facilitated by the orientation I shall designate realist. Let me first briefly indicate what I understand by the term. In fact, any position might be said to be a philosophical realism that asserts the existence of some disputed kind of entity, such as black holes, class relations, economic equilibria, gravitons, tanon-neutrinos, utilities. Clearly on this definition we are all realists of a kind, and there are very many conceivable realisms.

In science, in particular, a realist position, i.e., a scientific realism, asserts that there are ultimate objects of scientific investigation, and that these exist for the most part quite independent of, or at least prior to, our investigation of them.

Now the conception of realism for which I want to argue is closely and explicitly bound up with ontology or metaphysics, i.e., with enquiry into the nature of being, of existence, including the nature, constitution and structure of the objects of study. Indeed, it is a forthright, systematic and sustained concern with ontology, and in particular with elaborating the broad nature of aspects of natural and social reality, that explains, in what follows, the term realism being used in reference to the project and perspective distinguished.

Might it not be said that if we are all realists, no project warrants being distinguished or identified as realist (see Hausman 1998, for an argument of this sort)? Certainly, predictive instrumentalists are realists of sorts; they are realists about the phenomena recorded in the observation reports that it is the job of the theoretical instruments to cover or accommodate. And some instrumentalists, as we have seen, are realist about the theoretical claims sustained—even if prepared to acknowledge that the claims are fictitious. They accept elements of what Mäki terms semantic realism. Moreover, whether or not instrumentalists are concerned about it, their methods and criteria always carry ontological presuppositions of some sort, and some instrumentalists explicitly acknowledge that some categories of unobservables refer even if they do not represent (see for example, Nagel 1961). Are we not then all realists, with the consequence that, as Shionoya (1990, p. 195–196) suggests<sup>5</sup>, the distinction between realism and instrumentalism is blurred at best?

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<sup>5</sup> As Shionoya recognises:

“Instrumentalism claims that the theoretical (nonobservational) terms of science are not really assertions about the world but nonreferring symbolic devices for permitting derivations of statements about observable phenomena. Thus it denies ontological status to theoretical entities and structures. But some instrumentalists, still holding the thesis that true theories are neither true nor false, admit that some theoretical entities are real. This is the reason why the difference between instrumentalism and realism is sometimes viewed as blurred; moreover, this reason why we regard the response to realism as a subsidiary claim of instrumentalism” (Shionoya 1990, p. 195–6).

As I say, I think we are indeed all realists. But it does not follow that the term realist cannot be used to designate a project, or indeed that it does not make sense to distinguish realists from instrumentalists. We, most of us, occasionally sing, teach, study, clean, joke and so forth, but this does not render meaningless the act of referring to some people as singers, teachers, students, cleaners, comedians. The use of such labels indicates that the activity in question is carried out by the person or agency so designated in an explicit, systematic and sustained way; this is the distinction expressed when performers are labelled by way of reference to their activities. And in similar fashion it is precisely an explicit and sustained concern with ontology to which I refer when attributing the realist label. That is why, in tying realism to ontology above, the endeavour was referred to as explicit, sustained and so forth. This commitment is essential to any realist enterprise. And it is such a commitment that instrumentalists, amongst numerous others, clearly lack.

I now want to argue that such a realist orientation brings with it various methodological implications regarding the conduct of economics, and certainly encourages alternative responses to the current situation, and I believe more fruitful ones, than the instrumentalist emphasis on 'more of the same.'

### **3.1. Realism to the Rescue?**

Realists of the sort I am here identifying are concerned to explain the world. And in principle there are no phenomena considered necessarily outside the realist purview. To the contrary, the realist can rest easy only when all phenomena of immediate interest are explained and apparent puzzles, or tensions resolved. So far, I have mentioned various surprising or interesting phenomena in passing. And it seems beholden on any satisfactory realist project concerned with matters raised here, that an account is developed which has sufficient explanatory power to accommodate such observations as have been mentioned. Let me briefly enumerate the various observations made along the way. They can be grouped as three types:

- i) From the point of view of providing social illumination mainstream economics is none too successful. Certainly it performs badly in terms of accommodating the data. Further, that project is widely perceived as unrealistic, without clear direction and in a state of disarray.
- ii) A good number of economists are of the view that the realisticness of their theories is not a problematic issue, and proceed, in effect, as though theories can serve primarily as instruments of prediction.
- iii) Despite any clear articulation, a common response to econometric forecasting failure is to revise models by including more variables and digging deeper.

Now how can an explicit ontological orientation hope to make a difference to the situation? It can so, in essence, by putting a posteriori analysis in place of an essentially a priori statement merely of hope. Suppose that some tool has been found to be useful for some task, say a fountain pen for writing a letter. And suppose for that reason alone it is offered as a tool for a different task in a different sphere, say for the cutting of grass. We know this to be absurd just because we know something both of the nature of the grass, and so of the sort of tool which will be useful for its cutting, as well of the nature of the pen, and so of its limitations regarding the cutting of grass. But in analogous fashion, by way of ontological enquiry we can learn something of the nature of social phenomena, and thereby also of the sorts of methods that promise to be successful in illuminating the social domain as well as those which stand little chance on this. Ontology, then, or so I shall argue, can provide constructive methodological insight as well as reveal the limits of certain methods adopted merely on more a priori criteria. For the realist, then, in place of adopting methods because they are thought to have proven successful in other domains, or are considered essential to all science, or are considered proper according to some conventionalist criteria, or whatever, the aim also is to develop methods to fit the specific material of the social domain.

### **3.2. Deductivism**

How might we start? Let me first examine the methods of the mainstream and determine their ontological presuppositions. I will then suggest that the latter are not particularly characteristic of the social domain, so that it is hardly surprising that the methods wielded fail. However, I shall argue that the conditions of the social realm are such that economics can be a successful science none-the-less—albeit only if we tailor our methods to insights we can obtain into these conditions.

I have already argued that the essence of the modern mainstream is the method of formalistic modelling. Now it is essential to recognise that the sorts of formalistic methods used by economists rest on the deductivist mode of explanation or its associated conception of science. By this I mean only that such methods presuppose, as a necessary condition of their application, closed systems, i.e., those which support regularities of the form ‘whenever event (or state of affairs)  $x$  then event (or state of affairs)  $y$ ’. These regularities can be as complicated as you like—they may involve numerous variables, and perhaps be associated with non-linear representations—and be deterministic or stochastic. According to deductivism, event regularities are essential to the scientific or explanatory process, however interpreted.

Now there can be no doubting that deductivism is a central, indeed characteristic, feature of modern economics. As I say, it is essential to all forms of mathematical modelling; it is explicitly posited in seemingly every econometrics text book and in most papers in this field; and reasonably often acknowledged explicitly by mainstream theorists as well. Thus, for example, we find Hahn criticising dissenters from the mainstream approach for not realising that the deductivism he recommends is indeed the universal approach:

“Opponents of [economic] theory often argue that it is tautologous because it consists of logical deductions from axioms and assumptions. If one is kind to such critics one interprets them as signalling that they do not care for these axioms or these assumptions. In any case all theory in all subjects proceeds in this manner” (Hahn 1985, p. 9).

And Allais supposes the existence of regularities is the phenomenal situation in economics, a state of affairs which explains, as he sees it, why economics is a science resting on the same general principles and methods as natural sciences such as physics:

“The essential condition of any science is the existence of regularities which can be analyzed and forecast. This is the case in celestial mechanics. But it is also true of many economic phenomena. Indeed, their thorough analysis displays the existence of regularities which are just as striking as those found in the physical sciences. This is why economics is a science, and why this science rests on the same general principles and methods of physics” (1992, p. 25).

Now a condition for the supposed universal relevance, in the social realm, of deductivism, and so of formalism, is a ubiquity of closed systems. In fact, the reason for the repeated failure of the mainstream project, I want to suggest, is simply that the social realm is everywhere open, that scientifically interesting event regularities rarely, if ever, occur.

### **3.3. A Closer Look at Real World Closures**

Now to help sustain this claim, and to indicate that, for science, openness is not an insuperable problem, let me briefly consider rather more closely than economists usually do the nature of the natural realm and of its science. I do not, of course, do so with the intention of arguing straight from natural science to social science. Rather, the social realm has to be considered on its own terms. But there

are two strategic advantages in turning to a consideration of the natural realm and its science at this point. First, the desire amongst economists to be scientists in the mode of natural scientist is so strong that it is worth showing first—as I think is easily done—that the formalistic deductivist method is actually not central to natural science after all. Second, because closed systems clearly do occur in the natural realm, though are hardly ubiquitous, indeed are rather restricted in their occurrence, we can learn by way of examining them, something of the conditions of their occurrence.

Now a significant fact of the matter to emphasise here is that, outside astronomy, natural scientific closures are mostly confined to experimental situations. Even so, it is worth taking note that experimental results are yet successfully applied in open (non-experimental) contexts, in situations in which event regularities are not to be found. In fact, these two observations are sufficiently important for us to add them to the list of phenomena for which we have to account:

- iv) Scientifically interesting closures, at least outside astronomy, are mostly confined to well controlled experimental situations.
- v) Experimental results are regularly successfully applied in open systems where event regularities do not obtain.

Let me immediately provide an explanation for observations iv) and v), the only sustainable explanation, indeed, of which I am aware (and indeed can imagine). Just before I do so, however, notice how problematic these observations are for the deductivist, including the predictive-instrumentalist, who considers the elaboration of event regularities to be essential to science. First, the confinement of event regularities to controlled experiments vastly reduces the scope of application of science; far from having near universal relevance, science, on such an understanding, is found to be hardly of relevance at all. Second, because any laws of nature are thought to depend on event regularities, and given these mostly are located in well-controlled experimental situations, it follows that laws of nature depend on us, on our establishment of the experimental situation, which is at the very least counter-intuitive. Third, the fact that science is regularly successfully applied outside the experiment, in situations in which event regularities are not at all in evidence, is quite unintelligible on the deductivist interpretation.

How then can we explain observations iv) and v)? In order to do so it is necessary to accept an alternative ontology and conception of science to that underpinning deductivism. The ontology required is one of structures, powers, mechanisms and tendencies, etc., that are irreducible to, but which underpin the

actual course of events and states of affairs. Once this ontology is established it supports a conception of science as moving from phenomena at one level to its conditions or causes at a different, deeper, one. Let me start with ontology.

Consider an aspirin. In virtue of its intrinsic chemical structure it has certain powers, most obviously to relieve a headache (or pain in general). Or consider a bicycle. Because of its physical structure it facilitates rides. Now the powers of aspirins, bicycles, and anything else, can exist unexercised; the aspirin may remain in the bottle, the bicycle in the garden shed. When powers are exercised they work by way of mechanisms or processes. For example, the aspirin and human body work together in such a way as to contribute to the relief of the headache. The category of tendency is reserved to capture the idea that something can be continually active even if its effect is not completely actualised in the outcome. If I have a headache the aspirin will have a tendency to make it better. And this remains the case even if the headache actually ends up worse because I take the aspirin with a bottle or two of sake, and while listening to a loud noise. Here the workings of the sake and the noise produce countervailing tendencies, dominating the effects of the tendency of the aspirin to relieve the headache.

It should be emphasised that the operation of a tendency in an open system, i.e., in the face of countervailing tendencies, licences claims that are transfactual as opposed to being merely counterfactual. They do not inform us of what would happen if things were different but of what is happening in reality whatever the actual outcome. They tell us, for example, that the gravitational tendency acts on the pen in my hand whatever I do with it. The gravitational tendency is not merely something that would, counterfactually, have an impact if I dropped the pen in an experimentally created vacuum; it is something that is acting on the pen and continues to do so whether I drop it in a vacuum, throw it in the air, continue to write with it, or merely place it in my pocket.

Armed with this ontology, and I believe only if so, observations iv) and v) are easily explained. According to it science is no longer restricted to, or primarily concerned with, correlating actualities; rather it aims to uncover the mechanisms, etc., that govern surface phenomena. And the experiment can be recognised as an attempt to intervene in reality to isolate a stable mechanism or tendency from countervailing ones so that it is more easily empirically identified. The production of an experimental event regularity—between triggering conditions and the effects of the mechanism—just is an aspect of this process.

From this perspective none of the problems with the deductivist conception arise. First, science is not restricted to the experimental situation; mechanisms are operative and identifiable all over the place, whether or not event regularities are in evidence. The experiment, with its possibility of event regularities, is just one

contribution to the scientific process. Second, there is no longer anything counter-intuitive about the confinement of event regularities to the experimental situation. For laws of nature no longer are considered to express event regularities, but rather the workings of underlying mechanisms and tendencies. From this perspective the well controlled experiment can be seen not as a situation in which a law of nature is produced, but merely as one wherein, as I say, it is empirically identified. Third, the fact that the results of experimental research are applied in open systems is also no longer surprising. For the objects of this research are now recognised as being mechanisms or tendencies, etc., many of which, if triggered, operate transfactorially, i.e., inside and outside the experimental situation alike. The gravitational tendency, as noted, is operative not only in the experimental vacuum, but equally, if transfactorially, on the autumn leaf even as it flies over roof tops and chimneys.

Given the explanatory power of this perspective—a component of a broader view that has been systematised as critical realism in economics—I think it can be accepted. But it follows from it that economists have, in their abductions, seriously mischaracterised natural science. Formalistic modelling activities, or the event regularities which condition them, are highly restricted even in the natural sphere, and in the end inessential to the natural scientific process. Rather, the essence of natural science is seen to be the move from a phenomenon of interest at any one level to a cause lying at a different ‘deeper’ one. And although the controlled experiment can aid this process, there is, as I say, no reason to suppose it to be a necessary aspect.

### **3.4. The Social Domain and its Science**

Does this conception of science—entailing a movement from phenomena at one level to causes at another—carry over to the social? Notice that I am not wanting merely to assert that it does, but to question whether it is possible.

What first of all is the social realm? It is typically defined as that domain of all phenomena whose existence depends at least in part on intentional human agency. And it is clear that, for this domain, the conception of science in question does indeed carry over. Central to the possibility of science carrying over is the condition that the sphere of reality to be investigated is structured: it is irreducible to actualities such as events and states of affairs; it includes structures, powers, mechanisms and the like. And the social realm is easily seen to be so structured. Behind speech acts are rules of language; behind my cashing of checks, a banking system; behind my motoring acts, systems of highway code; and so forth.

### 3.5. The Specificity of Social Ontology

But there are differences between the natures of the social and natural realms as well. First, although, like natural structure, social structure conditions our actions—when I drive home I depend on gravity and the highway code alike—social structure depends in turn on us and our activities. Social structure, then, is both condition and consequence of human activity. The Japanese language, for example, is a (typically unacknowledged) condition of the speech acts of Japanese people, and its continued reproduction (and transformation) is equally a consequence of the sum total of people engaging in the Japanese language. Social being, then, is inherently dynamic; it is a transformative process in motion. In critical realism this is systematised as the transformational conception of social activity.

Second, the social realm seems to be very highly internally related or holistic. Two things or aspects are said to be internally related when they are what they are, or do what they, in virtue of the relation in which they each stand to the other. Thus, employers and employees, landladies or landlords and tenants, teachers and pupils, parents and children, etc., are internally related; you cannot have the one without the other. Notice that in the social realm it is positions rather than individuals that are internally related. I am a university lecturer, and as a result I have various rights, tasks, obligations and duties to perform, etc. But if I were to resign tomorrow, someone else would take over my job and inherit the same set of obligations, etc. They are attached not to me but to my position. We can see that this is true of all positions. We can also easily see that we all simultaneously occupy a very large number and range of positions as parents and children, employers or employees, teachers, students, members of religious or political groups, and so on. And each position is typically internally related to a large number of others. As a result, society emerges as a highly internally-related phenomenon. It is indeed an internally-related position-practice system into which agents essential slot, and which, through the collective actions of such agents, is continually reproduced and/or transformed through practice (for an extensive discussion see Lawson 1997, chapters 12 and 13).

These latter elements of social ontology have been developed here in a somewhat fast and sketchy manner. As a result, the aspects of social being to which I have referred, have been discussed only at an extremely high level of abstraction. However, I think this brief elaboration is sufficient for me to establish my basic claim. These are that i) the study of social phenomena can be scientific in the sense of natural science, because the social world is structured; but, ii) because of the highly internally-related and intrinsically dynamic nature of the social domain, social closures may not be possible. In the natural sciences,

closures are produced in experimental situations where isolatable stable mechanisms are effectively shielded from others by way of human manipulation. In this way they are empirically identified. The highly internally-related and dynamic nature of society suggests that it is unlikely that stable bits of it can somehow be similarly experimentally cut off. Thus, although the structured nature of society means that the latter can be studied scientifically in the sense of natural science, mainstream deductivist economists, with their near exclusive focus on closed systems, appear to have succeeded only in universalising that one limited aspect of natural scientific practice which, in the social realm, has hardly any relevance at all.

The realist approach, then, does not lead to the methodological conclusion of the instrumentalist that we need more of the same, that we persevere in our efforts to elaborate event regularities of interest. Instead, it bears the implication that economics ought really to move in a different direction entirely, to develop ways of uncovering causal mechanisms in a seemingly quintessentially open, as well as intrinsically dynamic, and highly internally-related, social reality.

#### **4. Making Sense of All the Evidence**

At this point I am in a position to be able also to explain the observations noted earlier. Let me briefly recall what they were:

- i) From the point of view of providing social illumination mainstream economics is none too successful. Certainly, it performs badly in terms of accommodating the data. Further, that project is widely perceived as unrealistic, without clear direction and in a state of disarray.
- ii) A good number of economists are of the view that the realisticness of their theories is not a problematic issue, and proceed, in effect, as though theories can serve primarily as instruments of prediction.
- iii) Despite any clear articulation, a common response to econometric forecasting failure is to revise models by including more variables and digging deeper.

Observation i) is easily explained from the realist perspective by recognising that mainstream economists, given the very nature of their project, are attempting to express the phenomena of the open social system as though they were generated in a closed one. This activity is a bit like trying to force a square plug into a round hole—it cannot possibly work, and not surprisingly generates all the signs of tension, inconsistency, and general disarray that we see.

Observation ii) is also easily explained, by the recognition that, in their formalistic

modelling, activities, economists are very much driven by a desire to appear like natural scientists (the fact that here they have been found to be acting on a misconception of the nature or essence of natural science does not obviate this point). And it has to be admitted that in the natural sciences, an instrumentalist orientation has sometimes proven useful. For example, Ptolemy's astronomy made it possible to fit and predict the motions of the planets with quantitative precision, facilitating, amongst other things, a reasonably accurate calendar. Yet the geometric constructions of Ptolemaic astronomy made little theoretical sense given other understandings of the physical world. It was impossible, for example, to fit non-decaying epicycles—as the Ptolemaic system required—into a heaven full of crystalline spheres that revolved naturally around the centre of the universe. However, many impressed with the ability of the system to predict or accommodate observations on planetary movements, came to accept Ptolemaic astronomy as an instrument of prediction. The same instrumentalist orientation is also taken by some to aspects of modern quantum mechanics, which has proven to be predictively successful, if difficult to interpret.

But the relevant point here, of course, is that all such examples refer to closed-system situation where predictive success had already been achieved. The celestial closure, for example, is just a fairly unique example of a naturally occurring closure (at least relative to our life spans). Of course it is a spectacular one. No doubt it is precisely its spectacular nature that accounts for some part of the general failure from Laplace onwards to realise that the situation is relatively uncommon, to appreciate that the celestial closure is far from being indicative of the phenomenal situation that can be expected to prevail more or less everywhere. This failure, in turn, appears to be largely responsible for the widespread, if tacit, acceptance, formerly in philosophy, and currently in the social sciences in particular, of a ubiquity of constant conjunctions of events in nature, and thus of the doctrine of the actuality of causal laws.

In any case, observation ii) is now explained. When natural scientists have run into difficulties in interpreting their theories they have often fallen back on an instrumentalist posture. Some mainstream economists, happy to act like natural scientists, have chosen to follow suit. The difference, as I say, is that natural scientists have done so in closed system contexts, where their theories had already been found to be predictively successful, to avoid any seemingly unnecessary metaphysical worry. Economists, though, are adopting an instrumentalist stance in contexts where predictive success has not been achieved, where there is cause for concern on all fronts. The borrowing of the instrumentalist orientation, then, is, in this context, if explicable, quite without a legitimate rationale.

Observation iii) is also now easily explicable. By way of examining the

well-controlled experimental situation it has been found that, in order systematically to generate an event regularity, a stable mechanism must be isolated and triggered. Mainstream economists concerned to produce empirical models must, if tacitly, be looking to mimic this experimental set-up; they must be looking to elaborate analogous conceptions. In their deductivist models they need ways of guaranteeing that under conditions  $x$  some predictable outcome  $y$  follows. Clearly, to mimic in their theorising the stable and isolatable mechanisms which are found to form the objects of the experimental natural sciences, two requirements are entailed. First, the objects of analysis must be intrinsically closed, essentially atomistic, guaranteeing that, in the same conditions, the posited objects of analysis will always act in the same way, that  $x$  always tends to produce  $y$ . Second, an extrinsic closure condition is also required ensuring that all factors that can influence  $y$  are internalised within the system, or effectively held off. Putting these two requirements together, we find that deductivist modelling effectively requires conceptions of isolated crypto-atoms. And in modern economics, this is indeed essentially all we find. The easiest, though not unique, way of achieving intrinsic closure, of course, is to assume agents always optimise in conditions in which a unique optimum occurs. Extrinsic closure is achieved by assuming that external factors are constant or orthogonal in their effects.

We can thus, now, easily interpret the two systematic responses to econometric modelling failure reported in observation iii). The resort of including ever more variables in the analysis, is a reaction to the possibility that the extrinsic closure condition has not yet been adequately satisfied, that the system has yet to be adequately isolated. The resort of “digging deeper” is a reaction to the possibility that the intrinsic closure condition has yet to be satisfied, that the analysis is not yet sufficiently atomistic. Such responses clearly lead to two regresses which result, in the limit, in a (constructed) social world so large it excludes nothing, couched in terms of human atomistic agents so small they include nothing. The realist alternative is to recognise the structured as well as open nature of social reality, and to seek to replace (or at least supplement) the mainstream concentration on correlation analysis, with a turn to causalist explanation. It is to encourage a move in a quite different direction. The supported aim is not to broaden the analysis at the level of surface phenomena but to move behind the latter to identify the conditions by which surface phenomena of interest are produced.

There is not space here to indicate how this causalist analysis might proceed in practice. But this is something to which I have attended at length elsewhere, giving rise to a position systematised as contrastive explanation (see for example, the second half of Lawson 1997). I hope that I have here at least succeeded in showing that instrumentalism and realism bear contrasting implications. These

may not be much in evidence in closed-system situations, at least where a science is seemingly successful. But where the systems to be analysed are quintessentially open, as appears to be the case with economy and society, the two orientations offer in fact a world of difference.

#### **4.1. Final Comments and Conclusion**

Let me, by way of concluding remarks, frame my argument rather differently. Although it is common in modern economic methodology to contrast realism and instrumentalism, to do so is, at best, only part of the relevant story. In the context of modern economics, the central oppositions, dividing mainstream economists and their more heterodox opponents, are, if not always so expressed, between those who insist, or at least prioritise, closed-systems formalistic modelling methods and those who do not; between those who think that mimicking natural-scientific method is a sufficient basis for fashioning the methods of social science and those who do not; between those who neglect ontology and those who do not; and between reductionist and non-reductionist approaches to science.

Instrumentalists and realist find themselves on opposite sides of these oppositions. But if realist social theorists as conceptualised here may well figure as the second part of each described contrast, instrumentalists do not exhaust the former part of each pair. Certainly, not all who accept closed-systems formalistic modelling seem happy about adopting the instrumentalist stance. Hence the various expressions of dissatisfaction with the whole situation of modern mainstream economics referred to at the outset.

Here I have indeed allowed the label realist to be used to designate those who accept the need for ontological elaboration. Once an ontological stance is taken, I find it difficult to imagine that the conclusion will not be drawn that the social world is open and structured (including dynamic and internally related). Still it remains possible in principle that an alternative realist conception will eventually be sustained; this is why the one defended here has been systematised not as realism *per se*, but as critical realism in economics. Of course, once an ontological orientation is adopted, the nature of social reality determines whether a reductionist method is justified. I suppose it could have turned out that reality was of a nature such that reductionism was indeed an appropriate methodological stance. However, if the ontological analysis included above, and elaborated more fully under the head of critical realism, is at all correct, methodological reductionism is merely an unnecessary obstacle to social illumination.

In truth, I think both historically and in principle, instrumentalism and realism—as I am understanding these positions here—only come into clear

opposition in situations where there are difficult puzzles, tensions and other difficulties surrounding the interpretation of scientific theories and their inter relations, and connections to practice. For the realist the impulse is to critically transform the situation, to transcend as many of the difficulties as is feasible. The term critical in critical realism is there for a reason. For those who prefer to live with the situation, however, the instrumentalist orientation offers hope, or at least sanctuary. It offers, or allows, in the guise of an apparently respected philosophical tradition, a relaxed attitude to puzzles and tensions, a possibility that some may even find liberating. Perhaps this explains the relaxed attitude to the econometric 'malpractice' that Leamer and others find to be so endemic:

"The opinion that econometric theory is largely irrelevant is held by an embarrassingly large share of the economics profession. The wide gap between econometric theory and econometric practice might be expected to cause professional tension. In fact, a calm equilibrium permeates our journals and our meetings. We comfortably divide ourselves into a celibate priesthood of statistical theorists, on the one hand, and a legion of inveterate sinner-data analysts, on the other. The priests are empowered to draw up lists of sins and are revered for the special talents they display. Sinners are not expected to avoid sins; they need only confess their errors openly" (1978, p. vi).

However that may be, a problem with adopting such a relaxed attitude to tension and inconsistency in the context of modern economics is the absence of any cushion of predictive successes on which to fall back. The result so achieved, indeed, is conceivably the worst possibility of all. For not only is the subject marked by failure, the instrumentalist stance serves as an excuse to do nothing worthwhile about it.

Only if the working scientist possesses the concept of an ontological realm, distinct from her or his claims to knowledge of it, can he or she entertain the possibility of rational criticism of any such knowledge claims. To be a fallibilist about knowledge and method it is necessary to be a realist about things. Conversely, to be a sceptic about things is to be a dogmatist about knowledge and conventional method. When a subject is faring as poorly as is the modern academic discipline of economics, adopting the latter position, of which instrumentalism is a prime example, must be recognised as a debilitating orientation indeed.

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