Part I

THE CURRENT ORIENTATION OF THE DISCIPLINE AND THE PROPOSED ALTERNATIVE

This first part of the book is concerned with setting out my basic position and providing a framework that is drawn upon in Parts II–IV. It contains three chapters which to a significant extent systematise arguments that I have made elsewhere. A central aim here is clarification and consolidation. But there is also some development of my previous argument.

The first of the three chapters is the most critical. Here I note the less than satisfactory state of modern economics. I concentrate on those features of the discipline which I regard as its most problematic, and which can be shown to contribute significantly to its current unfortunate situation.

In the second chapter I urge a particular reorientation of the discipline as a way forward. Here the focus is on ontology. In particular I outline an approach to ontological theorising, discuss the sorts of results that are achieved, and also indicate very briefly something of the consequences of these results (a more detailed account of the latter is provided in the rest of the book).

The third chapter, a relatively brief note previously published in Economics and Philosophy*, addresses the specific question as to why it is appropriate to identify my project as realist.*

1

FOUR THESES ON THE STATE OF MODERN ECONOMICS

How might we characterise the state of modern economics? In this opening chapter I advance four basic 'theses' which bear quite fundamentally on this question. Because I have defended each one to some degree before I will not go into very great detail here. My purpose in reconsidering them side-by-side at this point is to systematise and clarify relevant background preconceptions. For the picture they collectively convey is taken as given (if further developed) in most of the chapters which follow. These four theses are quickly stated:

1 Academic economics is currently dominated to a very significant degree by a mainstream tradition or orthodoxy, the essence of which is an insistence on methods of mathematical-deductivist modelling.
2 This mainstream project is not in too healthy a condition.
3 A major reason why the mainstream project performs so poorly is that mathematical-deductivist methods are being applied in conditions for which they are not appropriate.
4 Despite ambitions to the contrary, the modern mainstream project mostly serves to constrain economics from realising its (nevertheless real) potential to be not only explanatorily powerful, but scientific in the sense of natural science.

Let me consider each of these assessments in turn.

Thesis 1: Academic economics is currently dominated to a very significant degree by a mainstream tradition or orthodoxy, the essence of which is an insistence on methods of mathematical-deductivist modelling

There can be little doubt that modern economics is dominated by a project that attempts to apply mathematical methods to all areas of study. Currently, graduate programmes in university faculties of economics concentrate on the use of mathematical methods[1] and often consist in little more than micro (mathematical) modelling, macro (mathematical) modelling and econometric modelling.[2] And most journals regarded as core or prestigious publish almost only articles formulated in mathematical terms.[3]

So dominant is this mathematising project in economics, in fact, that many of its modern perpetrators (unlike their predecessors[4]) hardly (or are not willing to) recognise that there are alternative ways of proceeding. For most members of the project, indeed, categories like 'economic theory' or even just plain 'theory' have become synonymous with mathematical modelling.[5] For a contribution even to be counted as economics (or to gain an audience) in mainstream circles, it is requisite that the author takes a mathematical approach and ultimately produces a formal model. Consider Richard Lipsey's observation:

> to get an article published in most of today's top rank economic journals, you must provide a mathematical model, even if it adds nothing to your verbal analysis. I have been at seminars where the presenter was asked after a few minutes, 'Where is your model?'. When he answered 'I have not got one as I do not need one, or cannot yet develop one, to consider my problem' the response was to turn off and figuratively, if not literally, to walk out.
>
> (Lipsey 2001: 184)

To recognise this situation is not to deny that the project in question is always, in some way, also concerned with social phenomena, or at least with social categories. Economists do not usually deal abstractly just with the properties of (mathematical) operators and elements of sets, but concern themselves with variables labelled 'consumption', 'income' and so forth.[6] Although some, like Debreu (1959), profess attachment to the Bourbaki ideal of a framework free of any interpretation (see Chapter 10), this ideal seems never to be realised in its entirety. It does serve the function of loosening up the project from achieving immediate contact with reality (as again we shall see in Chapter 10). But practitioners of modern economics appear never to abandon all concern with social categories, or the hope of illuminating social reality sooner or later. Ultimately the aim, it seems, is to render aspects of the social world intelligible. There is a sense, then, in which the project always remains in essence an explanatory endeavour.

The point to emphasise here, though, is that this project's conception, or mode, of explanation is necessarily one that facilitates the widespread usage of mathematical formalism including formalistic modelling.[7] That mode of explanation called into play is *deductivism*.

Deductivism

By deductivism I mean a type of explanation in which regularities of the form 'whenever event x then event y' (or stochastic near equivalents) are a necessary condition. Such regularities are held to persist, and are often treated, in effect, as laws, allowing the deductive generation of consequences, or predictions, when accompanied with the specification of initial conditions. Systems in which such regularities occur are said to be *closed*.[8] Of course, a closure is not restricted to the case of a correlation between just two events or 'variables'; there can be as many of the latter as you like. Nor is a closed system avoided by assuming a non-linear functional relationship or by pointing out, as in chaos theory or some such, that what happens may be extremely sensitive to initial conditions. If, given the exact same conditions, the same outcome does (or would) follow (or follows on average, etc., in a probabilistic formulation) the system is closed in the sense I am using the term.

Notice that it is the *structure* of explanation that is at issue here. The possibility that either many of the entities which economists interpret as outcomes, including events or states of affairs, are fictitious, or claimed correlations do not actually hold, does not undermine the thesis that deductivism is the explanatory mode of this project. In other words, by deductivism I refer only to forms of explanation for which closed systems are an essential component; no commitment to the realisticness of any closures or regularities posited is presupposed.

Observe, too, that it does not make any difference whether an inductive or *a priori* deductive emphasis is taken. If mathematical methods of the sort economists mostly fall back on are to be employed, closures are required (or presupposed), whether they are sought-after in observation reports or 'data' or are purely invented. Deductivism is an explanatory form that posits or requires such closures whether or not any are actually found. And deductivism, so understood, clearly encompasses the greater part of modern economics, including most of modern microeconomics, macroeconomics and econometrics.[9]

So characterised, the modern mainstream project might be labelled in various ways. In the sections which follow I refer to its activities interchangeably as mathematical-deductivist modelling, formalistic closed-system modelling, or just as formal (or mathematical) modelling, amongst other things. Such descriptions amount to the same thing and can be loosely systematised under the head of (modern) mathematical economics.[10] It is this approach, however we label it, that now pervades the discipline. And it is an insistence on this approach, I am suggesting, that characterises the highly dominant modern mainstream component within it (see also Dow 1997; Setterfield 1997).

If the mainstream mathematising endeavour is so dominant that its contributors often take it to be the whole of the discipline, this nevertheless is a mistake. Though marginalised, there are not only dissenting individuals but also various highly productive heterodox traditions that pursue understanding in economics whilst rejecting the mainstream insistence on mathematical modelling methods. Amongst the more prominent of the latter traditions we find, for example, Austrianism, feminist economics, (old) institutionalism, post Keynesianism, Marxian economics and social economics. Although sub-groupings or individuals within these projects do sometimes turn to formalistic modelling, there is not a reduction of economic method to techniques of formalistic modelling. Let me quote Diana Strassmann, the editor of *Feminist Economics*, who very well captures the orientation of the modern mainstream project as viewed from a heterodox perspective:

> 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: 153–4)

Although Strassmann here recognises the close association of mathematical modelling with the current mainstream project, there are some economists who have sought instead to characterise the modern mainstream in terms of features of its substantive theorising. Such endeavour, though, has not proven successful. Most typically, it has associated mainstream economics with theories of human rationality or conceptions of equilibrium, or some such. The problem here is that such features as are identified are found not to survive across the numerous (and consequential) flits in fads and fashion that the project in question repeatedly experiences at the level of its substantive interests.[11]

On recognising this situation, critical observers conclude that the current mainstream is just too slippery a project to pin down. Some even wonder if there is any continuity to, or commonality to the various strands of, the mainstream project at all. In Mirowski's view,

> the historian is forced to concede that, in fact, it is best described as a sequence of distinct orthodoxies, surrounded by a penumbra of quasi-rivals; and that it is this, more than any deductive or inductive 'successes', which accounts for its longevity.
>
> (1994: 68)

Overlooking the mainstream project's continuous reliance on methods of mathematical-deductivist reasoning, Mirowski feels we must question whether this project can be said to 'consist of anything more than a bold assertion of continuity in the face of repeated ruptures every two or three generations?' (*ibid.*: 69).

Those who reason in this sort of manner take the mathematisation of modern economics (if not necessarily any specific form of mathematics[12]) for granted. No doubt the common tendency to do so is reinforced by the widespread failure of most within the mainstream itself to defend or even comment on the mathematical emphasis. In consequence, I think it is worth recalling Whitehead's warning when considering philosophy more generally:

> When you are criticising the philosophy of an epoch, do not chiefly direct your attention to those intellectual positions which its exponents feel it necessary explicitly to defend. There will be some fundamental assumptions which adherents of all the variant systems within the epoch unconsciously presuppose.

> Such assumptions appear so obvious that people do not know what they are assuming because no other way of putting things has ever occurred to them. With these assumptions a certain limited number of types of philosophic systems are possible, and this group of systems constitutes the philosophy of the epoch.
>
> (1926: 61)

In any case, in the face of the seemingly unquestioned acceptance of the reliance of modern economics on methods of mathematical deductivist reasoning, it is worth emphasising over and again that mathematical modelling is certainly not essential to social theorising and understanding. This is a point I establish under the heading of thesis 4 below, where I argue, in fact, that the current formalistic emphasis is likely often debilitating of explanatorily insightful social analysis. My concern at this stage, though, is to emphasise that with mathematical methods being insisted upon by the mainstream but regarded as *in*essential by heterodox traditions and others, we can see that the various strands of orthodoxy have not only a common, but also a distinguishing, feature after all. This, as I say, just is the *insistence* that mathematical-deductivist methods be used in just about all endeavour to advance knowledge of phenomena regarded as economic (for further discussion see Lawson 1997c; 2002).

Thesis 2: This mainstream project is not in too healthy a condition

A second assessment of the current situation I want to advance is that this mainstream project, when viewed as an endeavour concerned with social explanation (as opposed to being considered under its aspect of seeking to maintain its dominant position within the academy), is actually not too successful.

In fact the problems of the modern mainstream project are sufficiently widely recognised (and recorded) by those who reflect on the issue that I need say very little indeed here. Heterodox economists have for a long time pointed to the failings of the project (see, for example, Ferber and Nelson 1993; Fine 2001; Hodgson 1988; 1993; Kanth 1997; Srassmann 1993a) as have close observers of the discipline (see e.g. Parker[13] 1993; *The Economist*[14] 1997; Howell 2000[15]). But even some proponents of the mainstream project themselves are showing signs of increased concern. Certainly some contributors to this project acknowledge that it performs rather poorly according to its own (explanatory/predictive) criteria of success (Kay 1995; Rubinstein 1991; 1995) and is plagued by tension and inconsistency between how it claims to proceed and actually does so (Leamer 1978; Hendry *et al.* 1990). Basically, the project is recognised as being in a state of some disarray and unclear even as to its own rationale (see e.g. Bell and Kristol 1981; Blaug 1997; Kirman 1989; Leamer 1978; 1983; Leontief 1982; Parker 1993; Rubinstein 1991; 1995; Wiles and Routh 1984). Consider for example Rubinstein's reflections:

> The issue of interpreting economic theory is … 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 utilizes mathematical 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 offered by the natural sciences, and the link between economic theory and practical problems … is tenuous at best.
>
> (Rubinstein 1995: 12)

This mainstream 'theorist' continues:

> Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question 'where does it lead?'.
>
> (Rubinstein 1995: 12)

More than ten years earlier, Leontief, a Nobel Memorial Prize winner in economic science, was already bemoaning the project's continuing failure to advance understanding:

> 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: 104)

Recently, Blaug, perhaps the foremost methodologist of the mainstream, formulates matters at least as starkly:

> Modern economics is sick. Economics has increasingly become an intellectual game played for its own sake and not for its practical consequences for understanding the economic world. Economists have converted the subject into a sort of social mathematics in which analytical rigour is everything and practical relevance is nothing.
>
> (Blaug 1997: 3)

Friedman, also a Nobel Prize winner, adds:

> economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems.
>
> (Friedman 1999: 137)

And Coase, yet another Nobel Prize winner, further remarks that

> Existing economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world.
>
> (Coase 1999: 2)

Of course, of those who acknowledge the less than satisfactory state of the modern (mainstream) project, not all actually associate its problems with its mathematical nature. To the contrary, this will tend to be the last consideration of most mainstream economists, just because to question the widespread reliance on mathematical methods is to query the very essence of their programme. Consider the response of the (mainstream) 'economic theorist' Alan Kirman (1989). In an admirable piece entitled *The Intrinsic Limits of Modern Economic Theory: The Emperor Has No Clothes*, a title which clearly indicates the critical and reflective predispositions of the author, Kirman is concerned about aspects of 'economic theory' as currently practised. However, despite an openness to change, Kirman seemingly cannot bring himself to sanction the possibility that something other than a form of mathematics is required. In attempting to 'identify the source of the problem' of modern 'economic theory', Kirman writes:

> The argument that the root of the problem … [is] that we are confined by a mathematical strait jacket which allows us no escape, does not seem very persuasive. That the mathematical frameworks that we have used made the task of changing or at least modifying our paradigm hard, is undeniable but it is difficult to believe that had a clear well-formulated new approach been suggested then we would not have adopted the appropriate mathematical tools.
>
> (Kirman 1989: 137)

The failings of econometrics has met with the same sort of response. For example, Edward Leamer, who, like Kirman, is clearly a critical and reflective contributor to his subject (with published papers carrying titles like *Let's take the con out of econometrics*), acknowledges both that the 'opinion that econometric theory is largely irrelevant is held by an embarrassingly large share of the economics profession', and also the existence of a 'wide gap between econometric theory and econometric practice' (Leamer 1978: vi). However, after failing to resolve the noted inconsistencies Leamer writes:

> Nor do I foresee developments on the horizon that will make any mathematical theory of inference fully applicable. For better or for worse, real inference will remain a highly complicated, poorly understood phenomenon.
>
> (Leamer 1978: vi)

The idea of a non-mathematical theory of inference, though, goes unconsidered.

The central point here, however, is that all responses of the sort noted rest upon recognitions of the less-than-buoyant state of the discipline. Whatever the types of diagnoses sought or offered, there is quite widespread agreement that the modern discipline is not in too healthy a condition, and that whatever explains the fact that the formalistic mainstream project has risen to such dominance (see Chapter 10 on this), it has little to do with this project's record so far at explaining the social world in which we live.

Thesis 3: A major reason the mainstream project performs so poorly is that mathematical-deductivist methods are being applied in conditions for which they are not appropriate

I now want to suggest that the continuing poor performance of the project in question is explained precisely by the persistent application of methods of formalistic modelling just in (social) conditions for which they are mostly not appropriate.

I am aware that such a possibility is almost unthinkable to many economists. Frank Hahn probably captures widespread sentiment when he declares of any such suggestion that it is 'a view surely not worth discussing' (Hahn 1985: 18). In fact, Hahn later counsels us 'to avoid discussions of "mathematics in economics" like the plague and to give no thought at all to "methodology"' (Hahn 1992a; see also Hahn 1992b). However, given the record of the modern mathematising project in economics, I think the need for a detailed discussion and analysis of its nature and relevance grows more urgent by the day.

Behind much of the incredulity many experience in any thesis of the sort I am advancing (i.e. in the idea that the mathematising tendency may itself be at least part of the problem) is a view, sometimes stated explicitly but I suspect more widely held, that mathematics, as used in economics, is just (another) language (see e.g. Samuelson 1952). I believe this perception is, at best, misleading. We are mostly dealing here with the ways economists apply to their discipline already worked-out mathematical procedures. And the problems that arise are more easily brought into relief by drawing parallels not so much with language as with tools more generally.[16] Few people, I suspect, would attempt to use a comb to write a letter, a knife to ride to work, or a drill to clean a window. Yet all these tools have their uses in appropriate conditions. And so it is with modelling methods of the sort that economists wield. Of course, with these examples I am being somewhat less than subtle. But if bringing them to mind helps challenge the complacency involved in the idea that any tool, including formalistic mathematical reasoning, can be universally applicable, they will have served a purpose.

Ontology

These considerations lead into the topic of ontology. By ontology I mean the study (or a theory) of being or existence, a concern with the nature and structure of the 'stuff' of reality. Now, all methods have ontological presuppositions or preconditions, that is conditions under which their usage is appropriate. To use any research method is immediately to presuppose a worldview of sorts.

It seems to be the case, however, that the ontological presuppositions of the methods of mathematical modelling used by economists are rarely questioned or even acknowledged, at least not in any systematic or sustained way. As a result, the possibility of a lack of ontological fit (a mis-matching of the presuppositions of these modelling methods with [the nature of] those features of social reality being investigated) is not considered. Yet, as I say, methods of mathematical-deductivist modelling, like all methods, do have ontological presuppositions. And my assessment, simply stated (and defended below), is that these preconditions of mathematical-deductivist methods appear not to arise very often in the social realm.

Closed systems

To move towards justifying this assessment, let me first note that the sorts of formalistic methods which economists wield mostly require, for their application, the existence (or positing) of event regularities; they presuppose the occurrence of closed systems. Mainstream economics, as I say, is a form of deductivism. By deductivism, I repeat, I simply mean any form of explanatory endeavour which assumes or posits or constructs regularities (deterministic or stochastic) connecting actualities such as events or states of affairs.

Of course, the fact that formalistic modelling methods require the identification or construction of event regularities is well recognised by mainstream economists. Allais (1992), taking the association of deductivist modelling and science for granted, expresses the conventional situation well:

> 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 as physics.
>
> (Allais 1992: 25)

But if Allais correctly points to the modern mainstream emphasis on identifying or formulating social event regularities, his description of the situation of modern economics is actually quite wrong in two of its aspects. Econometricians repeatedly find that correlations of the sort formulated are no sooner reported than found to break down. Social event regularities of the requisite kind are hard to come by (see Lawson 1997a: ch. 7). And it is just not the case that 'striking' event regularities of the sort Allais appears to reference, and which modern mainstream economists pursue, are essential to science. Their prevalence is a precondition for the mathematical-deductivist methods that economists emphasise having relevance, but the application of these methods cannot be equated to science. This latter claim I will defend below. For the time being I merely note that any presumption of the universal relevance of mathematical-modelling methods in economics ultimately presupposes a ubiquity of (strict) event regularities.

Atomism and isolationism

But this is not the end to the ontological preconditions of methods of mathematical-deductivist modelling as employed in modern economics. A further important feature, which is less often recognised (or at least rarely explicitly acknowledged), is that the dependency of mathematical-deductivist methods on closed systems in turn more or less necessitates, and certainly encourages, formulations couched in terms of (i) isolated (ii) atoms. The metaphorical reference to atoms here is not intended to convey anything about size. Rather the reference is to items which exercise their own separate, independent and invariable (and so predictable) effects (relative to, or as a function of, initial conditions).

Deductivist theorising of the sort pursued in modern economics ultimately has to be couched in terms of such 'atoms' just to ensure that under given conditions x the same (predictable or deducible) outcome y always follows. If any agent in the theory could do other than some given y in specific conditions x – either because the agent is intrinsically structured and can just act differently each time x occurs, or because the agent's action possibilities are affected by whatever else is going on – the individuals of the analysis could not be said to be atomic, and deductive inference could never be guaranteed.

Why do I qualify the inferences drawn, insisting the modern emphasis only *encourages* (and does not fully necessitate) atomism? And why do I refer only to deductivism 'of the sort pursued in modern economics'?

When I refer to deductivism as pursued in modern economics, I have in mind those closures in which the connected events might be said to stand in a relation of causal sequence. This qualification is required to the extent it might be suggested that the latter closures are not exhaustive of deductivism. Let me briefly elaborate.

By describing two events as standing in causal sequence I mean that one event, y say, happens in some sense because, or as an eventual result, of the other event, x say, which is prior. To describe two such events as standing in causal sequence carries no necessary implication that y happens (or is thought to happen) as a *direct* result of x. In the social realm events regarded as economic, and standing in causal sequence, are usually mediated at least by human agency. Thus for an increase in a person's income to result, say, in an increased expenditure on certain commodities the individual usually has to act, to exercise essential causal agency. But still the increased income is an event in the causal process or sequence resulting in additional expenditure. To say of two events that they stand in causal sequence is to assert that one is in the causal history of the other.

Why might it be retorted that deductivism does not require that events stand in a relation of causal sequence to each other? Situations may arise in which variations in two events x and y are merely concomitant, being caused, perhaps, by movements in a third (set of) factor(s). Examples of this sort abound in the economic realm as in any other. When the sterling price of US dollars (or of petrol or any imported items) rises in the east of England the price often rises in the west as well. When the striking refuse collectors fail to turn up to remove my neighbour's rubbish they also fail to turn up to remove mine. Clearly in such examples, the correlated events do not, or need not, stand in a relation of causal sequence. Neither event need be (even indirectly) a cause of the other.

Because I have defined a closed system as any in which an event regularity occurs, we might want to refer to a system where the events are correlated, but where neither causally conditions the other, as a *closure of concomitance* to differentiate it from a *closure of causal sequence*, where some event (the consequent event or dependent variable) is causally conditioned by the other(s) (the antecedent event(s) or independent variable(s)). The former type of closure can be extremely useful in social life, including (non-deductivist) explanatory work, as we shall see in Chapter 4. I focus upon this form of closure here, just to acknowledge that it does not presuppose an ontology of atomism. If x and y move together because they are both related to a third (set of) factor(s), there is no necessary presumption about how movements in the latter are related to movements in either (or both) of the former.

Having noted these qualificatory considerations, they need not detain us here. Although they will prove useful to my own project in due course, they have little bearing on the practices of modern mainstream economics. For, as a rule, mainstream economists, though committed to positing or detecting closures, are simultaneously concerned with theoretical formulations or explanations of a causal sequence sort. When, in constructing their models, modern economists relate consumption to disposable income, wages to consumer prices, imports to total final expenditure, investment to interest rates, and so forth, they are hypothesising that the posited relations arise because, in each event pair, movements in the former are somehow ultimately brought about because of, or in response to, changes in the latter.

Actually, I have to acknowledge that even faith in closures of the causal sequence sort ultimately, or formally, does not necessitate atomism. That is why, above, I acknowledge that the latter is only (albeit strongly) *encouraged*. I make the qualification just because an event regularity even of this (causal sequence) kind could come about by chance, with a different causal complex connecting the *a posteriori* associated events on each occasion. Such a possibility, however improbable, cannot be ruled out in principle. Of course, economists need more than this; they need to construct their theories in a way that event regularities are guaranteed, allowing deductive reasoning, etc. Thus although there is strictly no formal necessity for it, if economists are to theorise general connections between given events, if they are to persist in their micro- and macro- and econometric modelling endeavour, an atomistic ontology will be involved.

Atomism, then, is essential, if closures of the sort economists usually require are to be assured. However, even in the noted scenarios the assumption of atomism is not yet sufficient to ensure closure and facilitate deductivist explanation including prediction. For even with an atomistic ontology, the total effect on an outcome of interest may be changed to almost any extent if all the other accompanying causes are different. That is why, in concrete economic analyses, the (atomistic) individuals tend to be treated as part of an assumed-to-be isolated and self-contained set or system.

The ontological presuppositions of (or encouraged by) the insistence on mathematical modelling, then, are of subsets of the social domain constituted by isolated sets of atoms. Most typically, such deductivist modelling endeavour encourages a view of atomistic human agents (social atomism) where these are the sole explanatory units of social analysis (methodological individualism).[17]

I have not yet indicated precisely why I am suggesting the modern mainstream tradition fares so poorly as an explanatory endeavour. I have merely indicated that if the methods of mathematical deductivist modelling (as employed in modern economics) are insisted upon as universally valid for the social realm, a tacit presupposition is that the social realm everywhere comprises (closed) systems of isolated atoms.

Now it is immediately clear, I think, that these latter conditions need not characterise the social realm.[18] I want to suggest, in fact, that the noted conditions for closure may actually be rather rare in the social realm. I draw this conclusion on the basis of the (*a posteriori* derived) theory of social ontology, a conception of the nature of the material of social reality, defended in the chapters below (especially Chapter 2) and elsewhere (especially Lawson 1997a), and often systematised as critical realism. To avoid excessive repetition, let me postpone a defence of this ontology until the following chapter, and at this point turn and give a brief overview of aspects of the ontological conception in question.

A theory of social ontology

By *social reality* or the social realm I mean that domain of all phenomena whose existence depends at least in part on us. Thus it includes items like social relations, which depend on us entirely, but also others like technological objects, where I take technology to be that domain of phenomena with a material content but social form.

Now if social reality depends on transformative human agency, its state of being must be intrinsically dynamic or *processual*. Think of a language system. Its existence is a condition of our communicating via speech acts, etc. And through the sum total of these speech acts the language system is continuously being reproduced and, under some of its aspects at least, transformed. A language system, then, is intrinsically dynamic, its mode of being is a process of transformation. It exists in a continual process of becoming. But this is ultimately true of all aspects of social reality, including many aspects of ourselves including our personal and social identities. The social world turns on human practice.

The social realm is also highly *internally related*. Aspects or items are said to be internally related when they are what they are, or can do what they do, by virtue of the relation to others in which they stand. Obvious examples are employer and employee, teacher and student, landlord/lady and tenant or parent and offspring. In each case you cannot have the one without the other.

In fact, in the social realm it is found that it is social *positions* that are significantly internally related. It is the position I hold as a university lecturer that is internally related to the positions of students. Each year different individuals slot into the positions of students and accept the obligations, privileges and tasks determined by the relation. Ultimately we all slot into a very large number of different and changing positions, each making a difference to what we can do. The social realm, then, is highly internally related or 'organic'.

The social realm is also found to be *structured* (it does not reduce to human practices and other actualities but includes underlying structures and processes of the sort just noted and [their] powers and tendencies). And the stuff of the social realm is found, in addition, to include *value* and *meaning* and to be *polyvalent* (for example absences are real), and so forth.

This broad perspective, as I say, is elaborated and defended in the chapters below (especially Chapter 2). But I doubt that, once reflected upon, the conception is especially contentious. Nor in its basic emphasis on organicism or internal-relationality is it especially novel (see Part III below). However, it should be clear that if the perspective defended is at all correct, it is *prima facie* quite conceivable that the atomistic and closure preconceptions of mainstream economics may hold not very often at all.

That said, I repeat that the possibility of closures of the causal sequence kind, i.e. of the sort pursued by modern mainstream economists, cannot be ruled out *a priori*. Certainly, there is nothing in the ontological conception sketched above and defended in the following chapters which rules out entirely the possibility of regularities of events standing in causal sequence in the social realm. But the conception sustained does render the practice of universalising *a priori* the sorts of mathematical-deductivist methods economists wield somewhat risky if not foolhardy, requiring or presupposing, as it does, that social event regularities of the relevant sort are ubiquitous. And to the point, if the social ontology systematised in the following chapters (and sketched above) does not altogether rule out the possibility of social event regularities of the sort in question occurring here and there, it does provide a rather compelling explanation of the *a posteriori* rather generalised lack of (or at best limited) successes with mathematical-deductivist or closed-systems explanatory methods to date.

Actually the ontological conception I defend is more explanatorily powerful still. For not only does it ground a likely explanation of the widespread continued explanatory failures of much of modern economics over the last fifty years or so, but also it can account for both

(i) the *prima facie* puzzling phenomenon that mainstream economists everywhere, in a manner quite unlike researchers in other disciplines, suppose that (acknowledged) fictionalising is always necessary, and
(ii) the types of conditions that prevail when mathematical methods in economics achieve such (limited) successes as are experienced.

Let me briefly consider the latter two claims in turn.

Fictions

It is not only the case that modern economics mostly fails as a predictive and explanatory endeavour. It is also evident, and equally remarkable, that the mainstream project's theories are everywhere couched in terms of constructs that are absurd fictions, and acknowledged as such. Assumptions abound even to the effect that individuals possess perfect foresight (or, only slightly weaker, have rational expectations), or are selfish without limit, or are omniscient, or live for ever. Moreover, these sorts of assumptions are not a recent innovation but have always been thrown up by those who would mathematise the discipline. They are found, for example, in the contributions of Walras and his predecessors (see Chapter 10 below) just as they are found today.

Rather than invidiously pick on examples to illustrate the point that fictions abound (for any mainstream contribution would suffice), let me instead consider a commentary on the ways of the mainstream project by a prominent and reflective contributor. Admitting that modern mainstream economics rests on fictitious claims, the mainstream theorist Hahn (1994) writes:

> there is … a lesson which has only gradually been borne in on me which perhaps inclines me a little more favourably to the 'anti-mathematics' group.
>
> The great virtue of mathematical reasoning in economics is that by its precise account of assumptions it becomes crystal clear that applications to the 'real' world could at best be provisional. 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.
>
> (Hahn 1994: 246)

This passage captures well the sorts of assumptions that abound in modern economics, and various aspects are worth emphasising.

Notice, first, that the sort of fictitious assumptions thrown up within modern mainstream economics do not involve claims that could be true in some really possible counterfactual state of our world. I interpret the latter as part of the domain of the real. Real possibilities are as real as actualities. Both can have a causal impact. Mainstream economics continually falls back on states of affairs, etc., that could not possibly come about (see also Lawson 1997a: ch. 9).

Observe, second, that the set of fictitious assumptions does not reduce to those (if any) that are *in*essential to the results of the analysis. Rather fictitious constructions are usually vital in generating the results obtained. As Hahn expressly acknowledges, these sorts of assumptions are there precisely 'to enable certain results to emerge and not because they are to be taken descriptively'.

But how, though, is this emphasis on fictions to be explained? Notice, at this point, a third feature of the mainstream formulations illustrated by Hahn's assessments. Just as a class of assumptions, such as rationality, omniscience or total greed, always appears in order to render the human agent atomistic, a further set of assumptions, like a given number of agents or (as in the above passage) three goods and two periods, are always in place serving to fix the boundaries of the analysis, to isolate the set of atoms on which the analysis focuses. In other words, in some form or other the assumptions of atomism and isolationism are ever present, resulting from the (typically unquestioned) reliance on methods of mathematical-deductive reasoning.[19]

The reason for the fictitious nature of modern economics, then, is clear. To the extent that human beings as well as society are, in reality, complex, evolving and open, a methodology which necessitates that the subject-matter addressed is everywhere atomistic and isolated is likely very often to throw up accounts of human individual and collective behaviour that are fictitious and rather superficial, to say the least.

It follows, though, that for the mainstream practitioner wishing to retain mathematical-deductivist methods for all situations, there may be no other option than putting on a brave face and insisting that accounts that, in terms of substantive claims made, are somewhat superficial may yet perform well according to some pragmatic criterion (such as elegance, simplicity, revealing of where assumptions lead, generating deductions/predictions, and so forth). Lucas provides an example of a self-conscious response of this sort:

> To observe that economics is based on a superficial view of individual and social behavior 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 behavior without knowing very much about the make up and lives of the people whose behavior we are trying to understand.
>
> (Lucas 1986: 425)

A major problem for this particular 'justification', of course, (and for the quasi-instrumentalist stance of Friedman 1953 from which it derives[20] – see Lawson 1997a: 309–10) is that economists are actually not very good at predicting human behaviour (i.e. at making relatively accurate predictions as opposed to producing countless rather inaccurate ones – see Kay 1995: 19).

Modelling successes

How about my second claim that the ontological analysis provided above throws light on the sorts of conditions under which mathematical methods in economics are likely to prove most useful, and perhaps can be said to have achieved most success? If my arguments are correct, these conditions are precisely those in which, first, the agents of analysis are found to have little scope in what they (can) do (as with atoms, their activities are highly determined by context), and, second, only a few factors are found to bear any influence on the outcome of interest, or, equivalently, wherein one set of influences is so dominant that the effects of others are rendered marginal.

Possible examples that spring to mind are the behaviour of motorists in rush hours in busy cities, or perhaps decisions of those with extremely low incomes in western societies about whether to spend or save out of income received. Most generally, as in such examples, closures will occur when very basic biological needs are being satisfied. Clive Granger has argued convincingly[21] that it is possible to use econometrics to provide relatively successful short-run forecasts of phenomena such as electricity loads and peaks in regions wherein one factor, temperature, or more specifically the extreme cold, dominates behaviour. Even here it is found that the effect of the dominant factor depends on the time of day, and whether or not it is the weekend. But notably forecasters such as Engle *et al.* (1992) who, in focusing upon a particular period of the year, have attempted to forecast each hour of the day separately (twenty-four models) treating weekdays and weekends separately (making forty-eight models altogether) appear to have achieved a degree of all-round success that seems high by standards of modern econometric research.[22]

The point remains, however, that the sorts of conditions in question appear *a posteriori* not to be typical of the social realm. Rather, as I say, social reality is found to be a quintessentially open, structured, dynamic and highly internally-related system, amongst other things, whilst the conditions for achieving a local closure are seemingly rare. Thus our best explanation of the widespread failures of economics (as well as the fictions that abound) is just that mathematical-deductivist or closed-systems modelling methods are often applied to materials for which they are unsuited. It is conceivable, indeed, that the set of social situations for which they are appropriate is not very large at all.

The nature of the argument

My argument is ontological. I do emphasise this. For my impression is that the few explicit responses to criticism of the emphasis on formalistic modelling miss this and address instead to other forms of criticism which are often less significant. Perhaps to back up this observation I should mention explicitly some apparent criticisms to which responses have been forthcoming, but which I think are largely mistaken. Certainly they do not reflect my own worries.

Krugman (1998) for example, conjectures that criticisms of the mainstream emphasis on methods of formalistic modelling arise because exercises of the latter formalistic sort are often found to refute the preferred theories of the critics (1998: 1829)[23]. It should be clear, then, that my own concern is almost the opposite. It is that these formalistic methods seem rarely able to help refute (or support) anything.

Nor do I regard the emphasis on mathematical methods as especially elitist (see Krugman 1998: 1831).[24] Even less do I wish to minimise the value of clarity, rigour and consistency.[25] I do though insist that these attributes are not enough, that ability to illuminate the social realm counts as well.

Nor, further, do I deny that modellers often use data and pronounce on issues of policy.[26] I do, though, reject the presumption that such practices *per se* are sufficient to put a project like the mainstream in touch with reality.

Let me elaborate on the latter remark. Krugman (1998) sometimes gives the impression that dealing with data or mentioning policy issues is sufficient for relevance. And I am even aware of an endeavour to establish the relevance of modern mainstream economics which proceeds by determining the proportion of all articles in core or 'flagship' journals which make reference to 'empirical facts' or 'draw' policy implications, and reporting that this proportion is reasonably high.

Let me be clear. If economic data record phenomena generated within an open and highly internally-related social system, and mainstream economists uncritically insist on analysing them using methods which presuppose they record phenomena generated in systems that are closed and atomistic, any claims by these economists to be in touch with reality just because data are involved are not well founded. Indeed they merely reveal the level of misunderstanding involved. Similarly, if the whole framework of theoretical modelling is inevitably largely false, and known to be so, it is not obvious there need be any relevance or insight in policy conclusions drawn.[27]

To return to the central theme, however, I hope it is clear that the orientation I am taking is indeed ontological. Mathematical-deductive methods have many desirable features. But 'fit' with reality matters too.[28] The problem with the mainstream stance is that the ontological preconditions of its formalistic methods appear to be not only *not* ubiquitous in the social realm, but actually rather special occurrences. If we knew both that social life was everywhere atomistic, and also that for any type of outcome a fixed isolated set of causes was always responsible so that all other causal processes serve only as a kind of stable, non-intervening or homogeneous backdrop, we would have grounds for feeling confident in the emphasis that mainstream economists place on the sorts of deductivist methods they use. However, our best ontological analysis suggests that closures are but a special configuration of social reality, whilst our *a posteriori* experience is that this special case seems not to come about very often at all.[29]

Thesis 4: Despite ambitions to the contrary, the modern mainstream project mostly serves to constrain economics from realising its (nevertheless real) potential to be not only explanatorily powerful, but scientific in the sense of natural science

It does appear that a central reason so many economists persevere with methods of mathematical-deductivist modelling, despite a dearth of successes so far, is an ingrained belief that these methods are an essential component of all science (see e.g. Lantner 1997: 58). Not all hold to such a belief (e.g. Clower 1999; Kirman 1997); but very many seem to.[30] As Mayer (1997: 21) expresses matters, 'formal analysis provides a comforting feeling of doing work that is "scientific … "'.

Even so I think it can be demonstrated that mathematics is not essential to science after all. Further, there is every reason to anticipate that the study of social phenomena can be not only explanatorily powerful but scientific in the sense of natural science, even if mathematical-deductive methods are not used. In fact, the mainstream insistence on employing mathematical modelling methods in conditions for which they are not appropriate, actually serves as a barrier to economics proceeding in the manner of natural science. Let me briefly defend this fourth and final thesis by re-examining what we might mean when talking of natural science.

Now I think we can agree that natural science is carried on in numerous contexts. My claim defended in Lawson (1997a) and elsewhere is that if there is something fundamental to scientific explanation in the natural realm it is the move from phenomena at one level to their underlying causal conditions. And this move is often possible even where stable event regularities are not uncovered and mathematical formalism not applicable.

Against this claim the best (and a common) response of the deductivist modeller is to observe that whatever the extent of natural science the one sure component of it is the successful well controlled laboratory experiment. And experimental activity of this sort supports the image of science accepted by the formalistic modellers of modern economics. For in the well controlled laboratory experiment, event regularities (or closures) of the causal sequence type are regularly sought and often achieved, and where they are achieved forms of formalistic-deductivist modelling are indeed facilitated.

I acknowledge the import of this observation. But it is useful for my purposes to consider more closely what goes on in such conditions. In fact, problems for the deductivist arise as soon as we also recognise that

(i) most event regularities of the causal sequence sort regarded as of interest to natural scientists are actually restricted to conditions of experimental control, whilst
(ii) the results of these experiments are frequently successfully applied outside the experiments *where event regularities are not in evidence*.

The key to understanding this situation is already in place in the preceding discussion of the implicit ontology of economists' methods of mathematical-deductivist modelling. For the latter methods presuppose occurrences of event regularities of the causal sequence sort. And we have seen that in order to generate relevant results taking this form, economists need to specify their theories in terms of entities which both are isolated and produce constant and invariable responses to given conditions.

This analysis bears on how we must interpret experimentally produced event regularities. For we can make sense of the confinement of these regularities to experimental conditions just by viewing experimental practitioners as intervening in a sphere of reality and experimentally manipulating it in order that

(i) the workings of a specific intrinsically stable causal mechanism are
(ii) insulated from the effects of countervailing factors.

It is just because an intrinsically stable mechanism is isolated, where it is, that an event regularity is produced between the triggering conditions of the mechanism and the effects that ensue. If a mechanism being investigated were not stable, or countervailing factors are allowed to intervene, the regularity would not be produced.[31]

Notice, then, that to make sense of the experimental process, it is essential to recognise that the event regularity produced corresponds to the empirical identification of an underlying causal mechanism. In other words, even in experimental work, i.e. even in that branch of scientific work which is most bound up with the production of event regularities of the causal sequence sort, the primary concern is not with the production of an event regularity *per se*, but with the empirical identification of an underlying mechanism (co-responsible for any regularity so produced).

Notice, too, that it is only by way of this understanding of the experimental process that we can make sense of the observation, noted above but not yet addressed, that experimental knowledge is somehow successfully applied outside the laboratory, even in conditions where event regularities do not occur. For the knowledge or insights obtained relate primarily not to the (contingent and experiment-bound) regularity that is produced, but to a (experimentally empirically identified) mechanism that, when triggered, operates independently of scientists and their experimental work. For causal mechanisms normally act not actualistically (resulting in the same actual events or outcomes in all conditions), but transfactually (having effects all the time whatever the outcome). Thus gravitational mechanisms or tendencies will be acting on the autumn leaves not just as they fall to the ground but even as they fly over rooftops and chimneys.[32]

Science

There are many implications of this discussion that could be developed (see Lawson 1997a). But the central point I want to convey here is that even in those experimental situations where event regularities are successfully brought about, the real contribution of (successful) science is not the production of the event regularity *per se*, but the identification of an underlying causal factor. The aim of experimental practice is to increase our understanding (or to 'test' theories about) underlying powers, mechanisms and/or tendencies, etc., responsible for the events we produce or otherwise observe.

We find, then, that even the achievements of laboratory experimentation ultimately constitute evidence supporting the view that *if* anything is essential to the scientific process it is this movement from a surface phenomenon to its underlying cause. This is causal explanation (rather than event prediction – see Chapter 4 below). Now, the identification of causes is not restricted to situations where stable event regularities are produced. As we shall see in Chapter 4 especially, causes can be successfully uncovered in situations where mathematical-deductivist reasoning is not applicable at all. The point of relevance here, though, is that even the experimental work of science is found to concern itself with the understanding of causal factors.

In short, *if* science can be characterised by any one aspect of its activities, the analysis sustained here and elsewhere (Lawson 1997a) suggests that the prime candidate is the (explanatory) move from a conception of a phenomenon of interest at one level to a conception of its cause(s) lying at a different one. Science is characterised by causal explanation if by any one aspect or process. And, if the social ontology sketched above and defended in Chapter 2 below is at all correct, and specifically if social reality is indeed structured, this is a move available as much to those who study social phenomena as to those who study natural phenomena. Economists can seek to uncover, for example, the social processes governing unemployment, poverty or whatever.[33]

It follows, that even if the practice of applying methods of mathematical -deductivist modelling in economics continues to be rather unsuccessful, there remains every reason to suppose that economists can yet, and successfully, practice science in the sense of (successful) natural science.[34]

The mainstream project and science

The form of reasoning that takes us from observations on phenomena at one level to hypotheses about their causes lying at a different one is retroduction. Causal explanation depends on it (see Chapter 4 below). Can mainstream economists also adopt this causal explanatory move? If mainstream economists construct (novel) social theories in an endeavour to ground presumed event correlations of a causal-sequence sort, a form of retroductive move must be involved, even if this remains unrecognised.

The problem for modern mainstream economists adopting explanatory goals is not so much whether, as how, retroduction can be employed. Causal explanation is concerned with identifying the powers, mechanisms, tendencies and structures responsible for phenomena at a higher level. The purpose is to uncover how the surface phenomena were produced. Although mainstream economists do not eschew all talk of powers and mechanisms (as Hoover [1997] for example reminds us),[35] it is clear that in the retroductive process the range of feasible options is severely constrained by the prior and dominating goal of achieving conceptions that are (mathematically) tractable.

Thus, in mainstream models, agents are often endowed with powers such as rationality which (unlike those, say, of perfect foresight or omniscience) seem realistic up to a point. However, in order to facilitate deductivist or closed-systems formalistic modelling, any powers attributed to agents (whether realistic or not) must be assumed always to be exercised, and exercised in given ways. In order to generate event regularities it is not enough to assume that agents merely could (i.e. merely have the power to) do this or that. Powers have to be exercised, and exercised in predictable ways. Thus agents, if endowed with the power of rationality, must always be rational in their actual behaviour. The starting point in mainstream undertakings is the desire to engage in deductivist modelling, and so the end result is a theoretical system, or set of conceptions, that facilitates this.

In an open social world, the *representations* of structures, including powers, elaborated on the requirement that they facilitate a closed system of the causal sequence sort are, then, in most cases going to be to a significant degree fictitious, as I have already discussed. Indeed, they will be of a sort that guarantees a system of isolated atoms. And ultimately all such powers as are conceptualised will, *qua* powers, be in any case largely superfluous to the outcome. For the deductivist, analysis requires that underlying powers, etc. are always reflected fully in predictable behaviour, i.e. are actualised, and in ways where events could not have turned out otherwise.

Mainly of course, the mainstream economist starts out intending to show that any outcome is a result of individuals optimising their situation. This is the easiest, or anyway (widely considered to be) most compelling, way of constructing a set-up with a predictable or deducible outcome. An isolated situation constructed so as to contain a unique optimum, coupled with the assumption that agents always optimise, meets, with relative ease, the requirements for formalistic deductivist modelling to proceed (thus explaining, of course, why so many commentators have interpreted the mainstream project as defined by its attention to the optimising individual atom).

However, this strategy, though explicable, is not essential. Assumptions, say, to the effect that (isolated) agents follow fixed rules irrespective of context, will do the job equally well. And indeed the presumption of rationality does not figure in all contributions accepted as mainstream (for actual examples see Lawson 1997a: ch. 8).[36]

The point here, though, is that in a non-atomistic world the constraint of providing theories that presuppose an atomistic ontology diverts us from uncovering (realistic accounts of) the real causes of phenomena of interest. And such a constraint is an unavoidable consequence of the insistence that the primary goal be always a conception or 'model' that is mathematically tractable. In orienting itself in this way the modern mainstream project, despite its pretensions, actually serves to undermine its clear ambition of achieving an economics which proceeds in a manner that is scientific in the sense of natural science. At the same time it serves to constrain the discipline from realising more of its explanatory potential.

Implications for the discipline of economics

So what, in brief, are the implications of all this? Most clearly a rather significant reorienting of the modern discipline of economics is warranted. Specifically, there is good reason for economists to turn to ontology, to engage more explicitly than hitherto in practices of realist social theorising. And, if the particular realist analysis outlined here is at all correct, there are also good reasons for economists to accept a more pluralistic orientation to the discipline, and in particular for economists to give up their insistence on methods of formalistic closed-system modelling for all occasions.

In so concluding I am not at all suggesting that formalistic modelling methods should not exist in the battery of options available. My aim with the discussion of this chapter is not to narrow down the range of methodological options by attempting to prohibit a particular method. Rather it is to widen the range of possibilities through criticising the fact that, and manner in which, in many quarters at least, the particular method in question is currently and often unthinkingly universalized.[37] The goal, as I say is a pluralistic forum[38] where explicitly prosecuted ontology and critical reflection can take their place amongst all the conceivable components of economics as social theorising. Only with this acheived, I believe, can we again, with, reason, be optimistic about the possibility of economics proceeding in an explanatorily fruitful fashion.