Pluralist Economics

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edited by
EDWARD FULLBROOK

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Peter Earl (University of Queensland, Australia) has been doing behavioural economics for three decades, although behavioural economics has only become popular and relatively mainstream in recent years. He is the author or editor of seventeen books and many journal articles and book chapters, a member of several editorial boards, and has been co-editor of the *Journal of Economic Psychology*.

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Introduction

EDWARD FULLBROOK

Rebellion

The year 2000, with hindsight, increasingly looks like a turning point for economics. In June of that year economics students in Paris circulated a petition calling for the reform of their curriculum. The petition described their situation as follows:

> Most of us have chosen to study economics so as to acquire a deep understanding of the economic phenomena with which the citizens of today are confronted. But the teaching that is offered, that is to say for the most part neoclassical theory or approaches derived from it, does not generally answer this expectation. Indeed, even when the theory legitimately detaches itself from contingencies in the first instance, it rarely carries out the necessary return to the facts. The empirical side (historical facts, functioning of institutions, study of the behaviors and strategies of the agents ...) is almost nonexistent. Furthermore, this gap in the teaching, this disregard for concrete realities, poses an enormous problem for those who would like to render themselves useful to economic and social actors. (Fullbrook 2003, p. 13)

The students asked instead for a pluralism of analytical viewpoints.

> Out of all the approaches to economic questions that exist, generally only one is presented to us. This approach is supposed to explain everything by means of a purely axiomatic process, as if this were THE economic truth. We do not accept this dogmatism. We want a pluralism of approaches adapted to the complexity of the objects and to the uncertainty surrounding most of the big questions in economics (unemployment, inequalities, the place of financial markets, the advantages and disadvantages of free-trade, globalization, economic development, etc.). (Fullbrook 2003, p. 13)

The closed-mindedness identified by the French students struck a chord not only with thousands of economics students around the world, but also with economists in many countries. So too did the students' emphasis on economics' disconnection from concrete realities and thereby its failure to empower people wishing to deal effectively with the economic dimensions of the material world.

A year after the French intervention, twenty-seven PhD candidates at Cambridge University in the UK issued their own petition, which further clarified the nature of the intellectual decadence. It emphasized four points:

1. It is harmful to teach students an economics based on neoclassical theory without also teaching them to recognize its limited range of applicability.
2. As currently practiced, economics disadvantages society by depriving it of all knowledge and understanding of economic phenomena not generated by the one approach.
3. It holds back the growth of that knowledge by restricting research to the traditionalist parameters.
4. It systematically denies career opportunities to economists who do not keep to 'the prescribed way'.

In other countries more petitions calling for pluralism in economics followed, including one from Harvard in 2003. Increasingly these interventions focused on how to remedy the situation, in short, how to restore academic freedom to economics teaching and research. A widely circulated manifesto issued by an international gathering of economists and students in Kansas City, USA, identified seven steps that the economics profession needed to take if it were to make a full break with dogmatism and to move to an open and pluralistic approach to knowledge that, as in the natural sciences, engaged with the real world:

1. A broader conception of human behaviour than that admitted by the nineteenth-century notion of 'economic man'.
2. The recognition that economic activities are embedded in culture.
3. The recognition that economic reality is always historically situated.
4. The need for all economists to be truthful about the fact that in the socio-economic realm the distinction between fact and value is not clear-cut.
5. Acceptance of the fact that economic theories, no less than physical ones, need to be empirically grounded.
6. To bring about that empirical grounding, economists must expand their methods of observation, including the use of multiple vantage points and data-gathering techniques.
7. Economists must learn to dialogue both with other disciplines and with fellow economists who approach economic reality from different vantage points.

These petitions, far from being momentary disruptions to the tranquillity of the status quo, have led to an outpouring of articles and papers diagnosing the origins of the dogmatism problem, identifying the characteristics of pluralism in science and its advantages for economics, and considering the concrete practices that a pluralist economics might entail. This book brings together some of the best writing from this new body of work as well as one

of the essays (Chapter 8) that inspired the beginning of what is now a broad, fast-growing, worldwide and – though it will take a long time – hopefully unstoppable movement for the radical reform of economics.

Pluralism and Philosophy

In the context of science the doctrine of pluralism, as opposed to monism, is surprisingly simple. Its justification rests on the fact that all empirical knowledge is fundamentally conditioned by the conceptual frameworks through which it is gathered. Each and every theory has its categories, internal relations and hierarchies of importance through which it filters and shapes its picture of the world-out-there, yielding a particular simplification and characterization of reality. It is the intrinsic nature of theories, scientific or otherwise, to function in this way. Sometimes theories pertaining to the same or overlapping realms contradict each other, suggesting that one theory is partly or wholly false and the other perhaps true. But more often their different stories merely reflect the complexity of reality in all its parts and the need to observe them from different points of view, as one sizes up a room by viewing it from different angles. It is this complexity of reality *vis-à-vis* the simplicity of theories that necessitates pluralism if science is to realize anything like its full potential.

In recent decades, however, the centrality of pluralism to scientific advancement has tended to be insufficiently appreciated, or in extreme cases even denied, by people outside the natural sciences. This failing arose in consequence of the philosophical spin put on the astounding events in early twentieth-century physics. Einstein's speculations and the discoveries that followed contradicted Newtonian physics which had been accepted for centuries. The subsequent transformation of belief begged an explanation. Philosophers stepped forward to portray science as sometimes a battle between competing theories, with one winning out over the other. This narrative came in a number of varieties and attained, not undeservedly, great popularity among the intellectual classes. But this popularization encouraged, in most cases unintentionally, an acute simplification of belief, namely that all important science shares this 'Cowboys and Indians' plot. Among economists especially this caricature of scientific practice gained ascendancy. This carries implications for practice because if all science were like this then there would be no point to pluralism. Therefore, before turning to the strange case of economics it seems prudent to restate some observations that celebrated physicists – not economists or philosophers – have made regarding pluralism and scientific advancement. The next section draws on four luminaries from modern physics whose working lives span the period 1880 to near the present.

Pluralism and Physics

We begin with Heinrich Hertz (1857–1894). The first to detect the electromagnetic waves predicted by James Clerk Maxwell's unification of electricity and magnetism, Hertz subsequently – in *The Principles of Mechanics Presented in a New Form* – turned his attention to developing a theoretical framework congenial to the new developments. In the book's long introduction, intended for advanced physics students, he sets out what he understands to be the prevailing epistemological ethos in his profession.

> In endeavouring thus to draw inferences as to the future from the past, we always adopt the following process. We form for ourselves images or symbols of external objects. ...The images [of] which we here speak are *our conceptions of things*. (Hertz [1899] 2000, p. 1; emphasis added)

Then, after noting that '[V]arious images of the same objects are possible, and these images may differ in various respects', Hertz spells out three criteria for choosing among different conceptions of the same phenomena. First a conception must be 'permissible' in the sense of being logically consistent. Second it should be 'correct' in the empirical sense that, in so far as 'the range of our experience up to the present' permits us to know, 'all those characteristics of our image, which claim to represent observable relations of things, do really and correctly correspond to them' (Hertz [1899] 2007, p. 9).[1] The third criterion Hertz calls 'appropriateness'. It differs from the first two in that it depends on the use to which the system of analysis is to be put. Hertz explains it as follows:

> To the question whether an image is permissible or not, we can without ambiguity answer yes or no; and our decision will hold good for all time. And equally without ambiguity we can decide whether an image is correct or not; but only according to the state of our present experience, and permitting an appeal to later and riper experience. But we cannot decide without ambiguity whether an image is appropriate or not; as to this, differences of opinion may arise. One image may be more suitable for one purpose, another for another; only by gradually testing many images can we finally succeed in obtaining the most appropriate. (Hertz [1899] 2007, p. 3)

Hertz was not making a case for pluralism here but instead merely describing to the physics student the basis of the pluralist ethos that he saw as characterizing his profession and as being the context into which his book was introducing a new 'system of mechanical principles', a new 'mode of conception', a new 'mode of treatment', a new 'mode of thought' (Hertz [1899] 2007, pp. 14–15).

A second account of the operation of pluralism in physics is provided by Louis de Broglie (1892–1987), one of the founders of particle physics. He writes:

> the quantum of action compels us today to employ 'complementary' descriptions to account for the phenomena on the atomic scale. By this term we are to understand descriptions which are certainly complementary but at the same time, taken strictly, incompatible. … According to Bohr, still further, each of these complementary descriptions is an 'idealization' *permitting us to represent certain aspects of the phenomena under consideration, but not all the aspects*.
>
> The best known instance of such complementary descriptions is supplied by the two descriptions of Matter and Light by means of waves on the one hand and of corpuscles on the other. The employment of each idea, as we have abundantly seen, *has proved essential for the interpretation of some phenomenon or other*; but the two ideas still remain, despite every effort, incapable of being reduced to terms one of the other, and the only connection that can be established between them is of a statistical nature. (Broglie 1939, p. 276; emphasis added)

This is an even more robust pluralism than the one Hertz describes, as it identifies the necessity of deploying theories that are incompatible.

Werner Heisenberg's (1901–1976) understanding of the need for an ongoing pluralism is perhaps even more radical:

> it was found that already in the theory of electricity an analysis using these concepts was no longer possible, and therefore in the investigation of this new domain of experience there emerged *new systems of concepts* leading to a final mathematical formulation of the laws of electricity.
>
> Accordingly, in the exact sciences the word 'final' obviously means that there are always self-contained, mathematically representable, systems of concepts and laws *applicable to certain realms of experience*, in which realms they are always valid for the entire cosmos and cannot be changed or improved. Obviously, however, we cannot expect these concepts and laws to be suitable for the subsequent description of *new realms of experience*. It is only in this limited sense that quantum-theoretical concepts and laws can be considered as final, and only in this limited sense can it ever happen that scientific knowledge is finally formulated in mathematical or, for that matter, in any other language. (Heisenberg 1962, p. 27; emphasis added)

The leaders of the next generation of physicists continued to emphasize the importance of pluralist practice for the advancement of the science. For example, Richard Feynman (1918–1988), celebrated for expanding the theory of quantum electrodynamics and particle theory, was also renowned for his teaching in which he emphasized the importance of describing nature in a range of ways. Here are two illustrations from one of his lectures in the series *The Character of Physical Law*:

> As long as physics is incomplete, and we are trying to understand the other laws, then the different possible formulations may give clues about what might happen in other circumstances. (Feynman 1967, p. 53)

and

> We must always keep all the alternative ways of looking at a thing in our heads, so physicists . . . pay but little attention to the precise reasoning from fixed axioms.

> One of the amazing characteristics of nature is the variety of interpretational schemes which is possible. (Feynman 1967, p. 54)

Pluralism and Economics

The history of economics is diverse, but nonetheless the idea of pluralism has generally been anathema to economists. Beginning with the French Physiocrates in the mid-eighteenth century, economists of all varieties have been inclined to believe that their approach to economic phenomena reveals, if not the whole truth, at least all of it that is worth knowing. It is with these broad conceptualizations, which are called 'schools', rather than subject areas, that economists form their primary professional identity. The assorted teachings and members of these schools are labelled orthodox or heterodox depending on whether their school is currently the dominant one or not. Until recently economists of all varieties have been comfortable with this quasi-theological scheme of things.

The traditional contempt on the part of economics for the pluralism of physics is ironic. Before other disciplines concerned with human society had even begun seriously to put themselves forward as sciences, economics was touting itself as a science on a par or near par with physics. This conceit, which in the twentieth century gradually pervaded most of the profession, originated in the 1870s with the founders of neoclassical economics. They took seventeenth-century Newtonian mechanics as their model, thereby conceiving of economic systems, be they individual markets or whole economies, as self-regulating determinate mechanisms composed of atomistic elements and tending towards equilibrium. As the celebrated Kenneth Arrow notes, two Newtonian principles have continued to be fundamental to this tradition, namely: '(1) the simple notion of determinateness . . . and (2) the specific notion that each relation represents a balance of forces . . . [meaning] that a violation of any one relation sets in motion forces tending to restore the balance . . .' (Arrow 1983, p. 107). Today economics stemming from the neoclassical tradition is called mainstream or orthodox, it being the only kind permitted in most university economics departments. All the other approaches are lumped together as 'heterodox'.

The problem is not neoclassical economics itself, but its monopoly position. Economic reality, like the natural world, is extremely complex, and so understanding it requires examining it from numerous points of view. Neoclassic economics is but one of those points, and one with a narrow sightline. When neoclassicism-based analysis is offered and accepted not as a partial truth but as the whole truth, all the other economic knowledge that could be gained and that should be part of political debate and decision-making is left out. Instead the political process takes place on the basis of a

small fraction of the economic knowledge potentially available. It is this enforced ignorance in matters economic that has resulted in the ascendancy of neoliberalism.

Neoclassical economics' monopoly in the classroom and its prohibition on critical thinking means that it brainwashes successive generations of students into viewing economic reality exclusively through its concepts, which more often than not misrepresent or veil the world, especially today's world. Nearly all of these neoclassical notions have a bearing on judgements about social, cultural and economic policy. Consequently, if society were to learn to think about economic matters outside the neoclassical conceptual system, it would almost certainly choose different policies.

This Book

The notorious orthodox/heterodox divide within economics is the mirror image of its anti-pluralism and so has no parallel in the natural sciences. Any serious move by economics towards pluralism will, therefore, inevitably result in a fundamental restructuring of both the discipline itself and the ethos in which it is practised. This book, in response to the mounting calls for reform, is a collective effort of economists from eight countries and from diverse backgrounds to come to terms with the conceptualizations and practicalities of doing and teaching economics in a pluralist manner consistent with the principles described by Hertz, de Broglie, Heisenberg and Feynman.

Part I traces the history and nature of opposition between pluralism and monism in economics. In Chapter 1 Christian Arnsperger and Yanis Varoufakis identify three meta-axioms – methodological individualism, methodological instrumentalism and methodological equilibration – 'on which *all* neoclassical analyses have been founded since the second quarter of the nineteenth century'. The authors show how these axioms not only severely limit the ability of neoclassical economics to illuminate economic phenomena but also exert a 'stranglehold over alternative modes of economic reasoning'. The authors also show how neoclassicalism's 'status within the social sciences, and its capacity to draw research funding and institutional prominence, is explained largely by its success in keeping these three meta-axioms well hidden'.

In Chapter 2 David Colander and Harry Landreth trace the ascendancy of pure formalism in economics. They argue that a pluralist methodology in economics in which individuals are actively committed to pluralism has seldom prevailed except as a state in the evolutionary process in which competing sides are of relatively equal strength. The case is made that the formalism hegemony that emerged in the latter half of the twentieth century was a temporary state and that it is already changing.

Chapter 3, by Kyle Siler, explores the interplay between the social and the intellectual underpinnings of economics and how this interplay explains both why mainstream economics is so powerful and how it is able to remain so in the face of often inconsistent empirical evidence from the 'real world'. Knowledge of the social construction of science and economics is a prerequisite for people seeking to establish a pluralist ethos in economics. As opposition to mainstream anti-pluralism burgeons, reformers should be mindful of the fact that scientific change is not entirely a scientific endeavour.

In Chapter 4 Robert F. Garnett examines, evaluates and reformulates contrasting approaches to heterodoxy. The central argument is twofold: (1) leading non-mainstream economists (including some who profess to be pluralists) are still deeply invested in oppositional paradigm building, viewing heterodox economics as primarily a search for demarcation criteria (conceptual, ontological, methodological or epistemological) that would render heterodox economics distinct from and superior to orthodox (mainstream) economics; and (2) heterodox economists would be better served by a principled pluralism – an 'egalitarian pluralism' that is committed to intellectual diversity.

Part II elaborates an array of arguments for establishing pluralism in economics. Chapter 5, mine, argues that because every narrative (including theories, paradigms and research programmes) views its domain through a particularized conceptual framework, just as one's view of a sculpture is always from a particular angle, it 'frequently happens that in a field of empirical enquiry there emerge several narratives which rather than being contradictory or incompatible are complementary in the sense of offering different windows for observation of the same or overlapping domains of phenomena'. The chapter explores the ways in which narratives, including scientific theories, differ structurally, and concludes with the identification of four kinds of epistemological pathology or 'anti-knowledge' common to the human sciences which result as a consequence of anti-pluralism.

Is there a single correct alternative to neoclassical economics? J. E. King in Chapter 6 shows that there is not and that this fact is increasingly recognized by eminent practitioners of several varieties of heterodox economic theory.

Peter Söderbaum's Chapter 7 argues that neoclassical economics is not only science but also ideology. While classical economists such as Adam Smith and David Ricardo always referred to 'political economy' in a broad sense, the neoclassical project starting around 1860 can be seen as an attempt to separate 'economics' from 'politics' and present a 'pure' economics. Here it is argued that the belief in a value-free economics is an illusion and that it is time to return to the 'political economy' vocabulary.

Geoffrey Hodgson begins Chapter 8 by discussing the breakdown of reductionism, with particular attention to economics and biology. He then asserts the need for the self-conscious use of metaphor in science and argues

that metaphor has always, in fact, been constitutive for science and has a subterranean presence in economic theory. Finally, Hodgson discusses the implications the presence of metaphors has for theoretical pluralism.

In Chapter 9 Jeroen van Bouwel scrutinizes the pluralist qualities in the work of one of the major voices of heterodoxy, Tony Lawson. Bouwel focuses on the alternatives that Lawson offers to mainstream's ontology and mode of explanation. The chapter raises some doubts about Lawson's pluralism, and identifies questions that need to be addressed by heterodox economists claiming to back pluralism.

Part III addresses the practical problems encountered in implementing pluralism in economics research and teaching. In Chapter 10 Alan Freeman and Andrew Kliman argue that economics requires formal rules of conduct to guarantee pluralism in research. The most important of these rules is that empirical reality must be tested against a multiplicity of theoretical explanations of that reality.

In Chapter 11 Peter Earl, drawing on his teaching experience, focuses on how students may be expected to behave if actually presented with a pluralist and non-deterministic approach to the teaching of economics. He explores tensions between three things: the construction and debate of rival scenarios, studies of the stereotypes that people use for coping with uncertainty, and the teaching of economics in a way that encourages students to develop for themselves effective ways of thinking about economic problems.

Thomas Mayer's Chapter 12 presents a version of epistemic pluralism that is not grounded in postmodernism and that is not subject to the objection that in its strong version it amounts to an 'anything goes' relativism, while in its weak version it amounts to no more than the platitudinous mandate 'be open to ideas that differ from yours'. Mayer counters the objection to the weak version of pluralism by presenting a version of epistemic pluralism that focuses on our limited knowledge and our uncertainty about many important aspects of the economy. He first discusses probabilistic pluralism with respect to economic policy, and then discusses theory, methodology and value judgements. He also responds to the argument that pluralism on the level of the individual economist is not needed.

In the concluding Chapter 13, Mohamed Aslam Haneef relates his experience in the Department of Economics of the International Islamic University, Malaysia, in introducing a course called Foundations of Islamic Economics. There follows a discussion of issues related to the relationship between economics and religion and also of various arguments on why a religion-based economics is possible, at least in the case of Islamic economics.

The pluralism being sought for economics by the contributors to this volume is but a mild version (closer to Hertz than to Feynman) of the one that has long ruled the roost in physics. Yet, bizarrely, the neoclassical formalist mainstream tradition is generally perceived as more akin to the spirit and

ethos of physics than the new emerging pluralist tradition. And sadly it seems that heterodox economists are almost as likely to hold this view as orthodox ones. If ever the falsity of this perception were to be made generally manifest, it is my view that the end of the monist reign would be near. From John Stuart Mill (1806–73) onwards, and never more so than in the last fifty years, economics has imitated what it misperceived to be the ways of physics, convinced itself that it was of close relation and sold itself to the public as the physics of the social sciences. If we could break this illusion, both the spirit and the public standing of economics' status quo would crumble. Economics would then find itself free to explore economic reality in all its dimensions and from many vantage points. A better world would inevitably follow.

Note

When it came to falsification, Heinrich Hertz was a Popperian long before Karl Popper (Popper 1959). In his book's introduction Hertz wrote as follows:

> No one will deny that within the whole range of our experience up to the present the correctness is perfect: that all those characteristics of our image, which claim to represent observable relations of things, do really and correctly correspond to them. Our assurance, of course, is restricted to the range of previous experience: as far as future experience is concerned, there will yet be occasion to return to the question of correctness. To many this will seem to be excessive and absurd caution: to many physicists it appears simply inconceivable that any further experience whatever should find anything to alter in the firm foundations of mechanics. Nevertheless, that which is derived from experience can again be annulled by experience. (Hertz [1899] 2007, p. 9)

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PART I
What Is Pluralism?

1
Neoclassical Economics
Three Identifying Features

CHRISTIAN ARNSPERGER AND YANIS VAROUFAKIS

There is nothing more frustrating for critics of neoclassical economics than the argument that neoclassical economics is a figment of their imagination; that, simply, there is scientific economics and there is speculative hand-waving (by those who have never really grasped the finer points of mainstream economic theory). In this sense, neoclassicism resembles racism: while ever present and dominant, no one claims to be guided by it. Critics must find a clear definition of neoclassicism if only in order to liberate neoclassical economists from the temptation to barricade themselves behind infantile arguments, namely the non-existence of their school of thought. Then, the good debate may begin.

In this chapter, we offer a definition of neoclassical economics that turns on three crucial axioms and which, in conjunction with one another, as we shall claim, underpin *all* (and *only*) neoclassical theory.[1] Later, we argue that these very axioms are simultaneously responsible for: (a) the difficulty mainstream economics faces when it comes to illuminating economic and social reality, and (b) the discursive success of neoclassical economics which gives it an effective (politically driven) stranglehold over alternative modes of economic reasoning.

We think our definition of neoclassical economics is important because critics are often caught off guard by sophisticated neoclassicists (see Dasgupta 2002) who take advantage of gaps in existing definitions in order to turn criticisms on their head. In short, the critique of neoclassical economics is bound to be as effective as its definition of the opposition is sophisticated. For instance, criticism that neoclassical economics necessarily posits hyper-rational bargain-hunters, never able to resist an act that brings them the tiniest increase in expected net returns, is apt but not telling. There are plenty of neoclassical models featuring boundedly rational agents; even utterly irrational agents (for example, evolutionary game theory; for a critical review in the spirit of this chapter, see Hargreaves-Heap and Varoufakis 2004). Similarly ineffective is criticism focused on 'neoclassical features' like market-clearing, *selfish* individualism or Pareto optimality. None of these cut

ice because, though these features are usually present in neoclassical modelling, they are not *necessary* features of some neoclassical model.

Thus, so long as critics' slings and arrows are directed against features of neoclassical economics that the latter can shed strategically, like a threatened lizard 'loses' its tail, they shall miss their target. Nevertheless, we do believe that there are at least three features of neoclassical economics that cannot be so shed; and, therefore, if the critics concentrate on them these critics shall, at the very least, force neoclassicists to engage in a fruitful dialogue. The single most promising prize from such a development ought to be the clarification of the origin and nature of the greatest paradox in social science: that *mainstream economics is as dominant as it is unappetizing* (even to some of its own practitioners).

In this sense, our axiomatic definition of neoclassicism, rather than being an idle methodological exercise, aims at exposing the root cause of mainstream economics' failure to say much that is helpful about the contemporary economic world. And it throws useful light on the reasons why such failure, instead of weakening neoclassicism, has reinforced its hold over the imagination of both the elites and the public at large. However, this is a longer argument which we shall only touch upon here (see Arnsperger and Varoufakis 2005 for more).

Once upon a time, it could be argued that neoclassical economics is typified by a familiar *mélange* of theoretical practices: positing an equilibrium in the labour market, the habitual recourse to Say's Law, the assumption that the interest rate will adjust automatically so as to equalize investment and savings, the depiction of capitalist growth *à la* Robert Solow and company, the imposition of Cobb-Doublas or CES production and utility functions, etc. Nowadays, any attempt to define neoclassicism by reference to these practices is music to the neoclassical ear; for there is a seemingly endless list of mainstream models that distance themselves from some, if not all, of the above. One of two conclusions appear in front of us: either the mainstream has moved on from neoclassicism (as neoclassical economists claim) or the definition of neoclassicism needs to be rethought and abstracted from a list of neoclassical practices like the one above. We choose the latter. So, the remainder of this chapter concentrates primarily on the three axioms that we think lie at the heart of neoclassical economic theory, old and new alike.

The First Axiom of Neoclassical Economics: Methodological Individualism

Unsophisticated critics often identify economic neoclassicism with models in which all agents are perfectly informed. Or fully instrumentally rational. Or

excruciatingly selfish. Defining neoclassicism in this manner would perhaps be apt in the 1950s but, nowadays, this definition leaves out almost all of modern neoclassical theory therefore strengthening the mainstream's rejoinders. Indeed, the past thirty years of neoclassical economics have been marked by an explosion of models in which economic actors are imperfectly informed, sometimes other-regarding, frequently irrational (or boundedly rational, as the current jargon would have it), etc. In short, *homo economicus* has evolved to resemble us more.

None of these brilliant theoretical advances have, however, dislodged the neoclassical vessel from its methodological anchorage. Neoclassical theory retains its roots firmly within liberal individualist social science. The method is still unbendingly of the analytic-synthetic type: the socio-economic phenomenon under scrutiny is to be analysed by focusing on the individuals whose actions brought it about; understanding *fully* their 'workings' *at the individual level*; and, finally, synthesizing the knowledge derived at the individual level in order to understand the complex social phenomenon at hand. In short, neoclassical theory follows the method of the watchmaker who, faced with a strange watch, studies its function by focusing on understanding, initially, the function of each of its cogs and wheels. To the neoclassical economist, the latter are the individual agents who are to be studied, like the watchmaker' cogs and wheels, independently of the social whole their actions help bring about.

So, the first feature of the 'body of theory' we think of as neoclassical is its *methodological individualism*: the idea that socio-economic explanation must be sought at the level of the individual agent. Note two things: First, this was not the method of classical economists like Adam Smith and David Ricardo, or, indeed, of Keynes or Hayek. Second, this proclivity is fully in tune with the mid-nineteenth century angloceltic liberal individualism (though the opposite does not hold) as it imposes axiomatically a strict separation of structure from agency, insisting that socio-economic explanation, at any point in time, must move from agency to structure, with the latter being understood as the crystallization of agents' past acts. We shall argue later that this strict separation is central in not only defining but also undermining the most recent claims of neoclassicism.

It is, we think, indisputable that all the new manifestations of what we term neoclassicism still subscribe to methodological individualism. While it is true that mainstream economists have, during the past few decades, acknowledged that the agent is a creature of her social context, and thus that social structure and individual agency are messily intertwined, their models retain the distinction and place the burden of explanation on the individual. Individual worker effort is nowadays often modelled as a function of sectoral unemployment (for example, efficiency wage models),

and the firms' micro-strategies reflect the macroeconomic environment. Nevertheless, and despite these interesting linkages between the micro-agent and the macro-phenomenon, the explanatory trajectory remains one that begins from the agent and maps, unidirectionally, onto the social structure.

The Second Axiom of Neoclassical Economics: Methodological Instrumentalism

We label the second feature of neoclassical economics *methodological instrumentalism*: all behaviour is preference-driven or, more precisely, it is to be understood as a means for maximizing preference satisfaction.[2] Preference is given, current, fully determining and strictly separate both from belief (which simply helps the agent predict uncertain future outcomes) and from the means employed. Everything we do and say is instrumental to preference satisfaction: so much so that there is no longer any philosophical room for questioning whether the agent will act on her preferences. In effect, neoclassical theory is a narrow version of consequentialism in which the only consequence that matters is the extent to which a homogeneous index of preference satisfaction is maximized.[3]

Methodological instrumentalism's roots are traceable in David Hume's *Treatise of Human Nature* ([1739/40] 1888) in which the Scottish philosopher famously divided the human decision-making process in three *distinct* modules: Passions, Belief and Reason. Passions provide the destination, Reason slavishly steers a course that attempts to get us there, drawing upon a given set of Beliefs regarding the external constraints and the likely consequences of alternative actions. It is not difficult to see the lineage with standard microeconomics: the person is defined as a bundle of preferences, her Beliefs reduce to a set of subjective probability density functions, which help convert her preferences into expected utilities, and, lastly, her Reason is the cold-hearted optimizer whose authority does not extend beyond maximizing these utilities. However, it is a mistake to think that Hume would have approved. For his Passions are too unruly to fit neatly in some ordinal or expected utility function. It took the combined efforts of Jeremy Bentham and the late nineteenth-century neoclassicists to tame the Passions sufficiently that they could initially be reduced to a unidimensional index of pleasure before turning into smooth, double differentiable utility functions.

During the tumultuous twentieth century, neoclassicists invested greatly in bleaching all psychology out of the rational agent's decision-making process. All hints of a philosophical discussion regarding the rationality of *homo*

economicus were thus removed. People could, and 'should', be modelled *as if* they possess consistent preferences which guide their behaviour automatically. The question of whether all rational women and men are condemned to maximize some utility function all the time became … nonsensical. Thus, instrumentalism lost its connection to the philosophies of Hume, Bentham or Mill and became a technical move that economists made instinctively with the same nonchalance as that of an accomplished artist preparing his oils and canvass before getting down to business.

However, it is false to claim that this state of affairs, even though ubiquitous in economics departments the world over, is *essential* for neoclassical economics. The first signs that it need not be came with the literature on endogenous preferences. Neoclassical economists increasingly sought to distance themselves from the assumption that preferences are fixed and exogenous. During the past twenty-five years or so, *homo economicus* has developed a capacity to adapt his preferences in response to past outcomes (see Bowles 1998). However, while the assumption that current preferences are exogenous was dropped, they remained fully determining. Thus, instrumentalism was preserved albeit in a dynamic context.

A more recent development has taken neoclassicism, and *homo economicus*, onto higher levels of sophistication. The advent of psychological game theory (see Rabin 1993, and Hargreaves-Heap and Varoufakis 2004, Chapter 7) has brought on a reconsideration of the standard assumption that agents' current preferences are separate from the structure of the interaction in which they are involved. Suddenly, what an agent wants hinged on what she thought others expected she would do. And when these second-order beliefs (her beliefs about the expectations of others) came to depend on the social structure in which the decision is embedded, the agent's very preferences could not be linked just with outcomes: they depended on the structure and history of the interaction as well.

In view of the above, there is no future in criticisms of neoclassicism based on the charge that the latter must take for granted preferences that are either exogenous or independent of the agents' socio-economic relationships. Critics toeing that line will be met with the scornful rejoinder that they criticize out of ignorance. However, our point that neoclassicism is still rooted in methodological instrumentalism cannot be so dismissed. For even in the latest reincarnation provided by endogenous preferences and psychological game theory, *homo economicus* is still exclusively motivated by a fierce means–ends instrumentalism. He may have difficulty defining his ends, without firm beliefs of what means others expect him to deploy, but he remains irreversibly ends-driven.

The Third Axiom of Neoclassical Economics: Methodological Equilibration

The third feature of neoclassical economics is, on our account, the *axiomatic imposition of equilibrium*. The point here is that, even after methodological individualism turned into methodological instrumentalism, prediction at the macro (or social) level was seldom forthcoming. Determinacy required something more: it required that agents' instrumental behaviour is coordinated in such a manner that aggregate behaviour becomes sufficiently regular to give rise to solid predictions. Thus, neoclassical theoretical exercises begin by postulating the agents' utility functions, specifying their constraints, and stating their 'information' or 'belief'. Then, and here is the crux, they pose the standard question: 'What behaviour should we expect *in equilibrium*?' The question of whether an equilibrium is likely, let alone probable, or how it might materialize, is treated as an optional extra, one that is never central to the neoclassical project.

The reason for the axiomatic imposition of equilibrium is simple: *it could not be otherwise*! By this we mean that neoclassicism cannot demonstrate that equilibrium would emerge as a natural consequence of agents' instrumentally rational choices. Thus, the second-best methodological alternative for the neoclassical theorist is to *presume* that behaviour hovers around some analytically discovered equilibrium and then ask questions on the likelihood that, once at that equilibrium, the 'system' has a propensity to stick around or drift away (what is known as 'stability analysis').

It is remarkable that the above has been with us since the very beginning. When A. A. Cournot constructed the first model of (oligopolistic) competition in 1838, he immediately noticed a lacuna in his explanation regarding the emergence of an equilibrium (Cournot [1838] 1960). Rather cunningly, instead of discussing this difficulty, he studied what happens when we begin from that equilibrium. Would the system have a tendency to move away from it or was the equilibrium stable? The proof of its stability secured his place in the pantheon of economic theory. Moreover, it established this interesting practice: first, one discovers an equilibrium; second, one assumes (axiomatically) that agents (or their behaviour) will find themselves at that equilibrium; lastly, one demonstrates that, once at that equilibrium, any small perturbations are incapable of creating centrifugal forces able to dislodge self-interested behaviour from the discovered equilibrium. This three-step theoretical move is tantamount to what we, here, describe as *methodological equilibration*.

Note that *methodological equilibration* is equivalent to avoiding (axiomatically) what ought to be the behaviourist's central question: *Will* rational agents behave according to the theory's equilibrium prediction? Instead, the question becomes: *If* rational agents are behaving according to the theory's

equilibrium prediction, will they have cause to stop doing so? Note also that *methodological equilibration* has remained intact since 1838 and Cournot's first use of it. To see this, consider the two great success stories to have come out of neoclassical economics since World War Two: general equilibrium theory and game theory. In neither case does the equilibrium solution spring naturally from the models' assumptions.

In general equilibrium theory its best practitioners state it categorically: convergence to some general equilibrium can only be proven in highly restrictive special cases. More generally, it is not just *difficult* to demonstrate that a system of theoretical markets will generate an equilibrium in each market, on the basis of rational acts on behalf of buyers and sellers; rather, it is *impossible*! (See Mantel 1974, and Sonnenschein 1973, 1974.) In game theory the same result obtains: in the most interesting socio-economic interactions (or games), common knowledge that all players are instrumentally rational seldom yields one of the interaction's Nash equilibria. Something more is required to bring on an equilibrium. That something comes in the form of an axiom that the beliefs of all players are *consistently aligned at each stage of every game* (see Hargreaves-Heap and Varoufakis 2004, chapters 2 and 3). This assumption is, of course, yet another reincarnation of *methodological equilibration*: for once we *assume* that agents' beliefs are systematically and consistently aligned, they are *assumed* to be in a state of (Nash) equilibrium. Yet again, equilibrium is imposed axiomatically before stability analysis can test its susceptibility to perturbations. Cournot's spirit lives on …

Three Axioms, One Neoclassical Economics

It is hard to imagine how any standardly trained economist could deny that her theoretical practices digress from the three methodological moves mentioned above: *methodological individualism*, *methodological instrumentalism* and *methodological equilibration*. For simplicity we shall henceforth refer to them as the *neoclassical meta-axioms*. Whether it is general equilibrium theory, evolutionary game theory, non-Walrasian equilibrium theory, social choice theory, industrial economics, economic geography, new political economy, analytical Marxism, public choice economics etc., all mainstream approaches in these fields remain loyal to the three meta-axioms above.

In fact, the meta-axioms are beginning to develop much closer, almost symbiotic, links with one another than was the case until fairly recently. Take, for instance, the attempts by psychological game theorists to create a sophisticated model of men and women, capable of drawing utility not only from socio-economic outcomes but also from the means that bring them about. When *homo economicus* learns that the ends do not necessarily justify

the means, he develops a welcome capacity to ponder, prior to acting, what others expect of him so that he can decide how much he values the various alternative outcomes.

For example, when deciding on whether to act bravely in defence of someone in need, his second-order beliefs (that is, his beliefs regarding what others expect of him) influence his estimate of the (psychological) cost of acting selfishly. To put it simply, his utility function cannot be defined independently of (a) the *structure* of the strategic interaction and (b) the beliefs that all participants would have *in equilibrium*. In this sense, *methodological equilibration* is no longer *prior* to *methodological instrumentalism* (as is the case in standard consumer or game theory): the axiomatic imposition of equilibrium is not only necessary in order to predict the interaction's outcome but it is also essential in order to define the instrumentally rational agents' preferences! (See Hargreaves-Heap and Varoufakis 2004, Chapter 7 and Fehr and Gächter 2000.)

It is, therefore, uncontroversial to state that every aggregate phenomenon scrutinized by neoclassical minds is explained increasingly and exclusively as some axiomatically imposed equilibrium emerging from the interaction of instrumentally rational individuals who either are optimizing consciously (as in rational choice or game theory) or are drawn to such behaviour through a process of 'natural selection' (as in, for instance, evolutionary game theory). The bottom line, then, is clear: despite all denials, there *is* such a thing as a body of social theory that subscribes to the three meta-axioms above and that we can legitimately, for want of a better term, label *neoclassical*.

At this juncture, there is one move open to neoclassical economists who still insist that what they are doing ought not be labelled as anything other than scientific economics: they need to persuade us that the neoclassical method, that is, models based on the three meta-axioms, is the only proper method; which obviously implies that there is no distinctly neoclassical method after all, even once that method has been characterized as above.

Effectively, they would have to adopt a rather extremist defensive posture: to claim that the combination of the three meta-axioms above is indispensable to any economic theory worth its salt; that the neoclassical method, as founded on the triptych of individualism, instrumentalism and equilibration, is not just one possible analytical strategy but that it is somehow *uniquely and ontologically grounded in social reality*. Their defence would amount to a claim to the effect that all other economic approaches, including for instance Adam Smith's, are not in the same scientific league as their own. Undoubtedly, many neoclassical economists think that (though few would state it in polite conversation).

Nonetheless, the truth status of that defence must be an empirical matter rather than a methodological one, and the defender of neoclassisism has to

provide hard evidence concerning the actual, material processes of (a) how preference orderings determine actions uniquely, and (b) how individuals' reasoning skills, or social/natural selection, slice through indeterminacy to bring about an equilibrium. Needless to say, such extreme naturalism has no chance of being empirically supported. Even sophisticated empiricists like Karl Popper rejected the idea that the joint hypothesis of individualism and equilibrium can be tested empirically; they are, he rightly claimed, *preconditions for* knowledge rather than *objects of* knowledge. Hence there is no such thing as a 'natural method'. The very thrust of the Enlightenment project rules it out of court.

The last resort of the mainstream economist, who wants to defend the presumption that the three neoclassical meta-axioms are essential to any scientific analysis of the social economy, is to argue that the neoclassical method of explanation, while not being a 'natural method', has nevertheless *evolved* historically as the *most adequate* method for studying a society of free, enlightened individuals. That it is, in short, the only non-contradictory embodiment of the Enlightenment project itself. That, just as representative liberal democracy is a bad system of government but remains the best one available, neoclassicism has evolved as the best economic analysis that is consistent with the liberal human condition.

However, such a rhetorical strategy can only work if it is accompanied by a sound evolutionary argument depicting the three meta-axioms as the unique 'attractor' of liberal social science. Unfortunately, no such argument seems to be forthcoming. Instead, mainstream economics is perpetually reproducing itself through a series of metamorphoses that Ovid would have been jealous of. The resulting models gain in complexity, expand in scope, and move into areas hitherto untainted by the economist's inquiring gaze. Nonetheless, all these models, in all their multiplying guises, share a well-hidden, and almost completely unspoken of, foundation: the three meta-axioms above. The radical absence of a debate about them is, we shall argue below, essential to the discursive power of neoclassical economics. As for the latter's aversion to pluralism, it is a natural by-product of this dance of veils whose purpose is to maintain neoclassicism's discursive edge by keeping our eyes off the theory's meta-axioms.

Some Thoughts on Neoclassicism's Discursive Power and its Aversion to Pluralism

What does an intelligently dispassionate observer of neoclassical economics see? She sees an ever-expanding technical literature, most of which she cannot comprehend. She sees an almost infinite series of mathematical

models that explain diverse socio-economic phenomena as part of some equilibrium scenario that posits autonomous actors bringing on the phenomenon under study, often supra-intentionally, through choices that are rational given everyone's beliefs (even when the actions are self-defeating). She sees a series of career paths that are made generously available to those who participate in this global research project. She sees economists the world over being taken seriously only to the extent that they speak this particular 'language'. She sees that the powers-that-be speak this very 'language'. Finally, she sees enterprising academics in other social sciences adopting this 'language', in a transparent bid to share into neoclassicism's discursive success. In short, the onlooker sees, correctly, power oozing out of the mainstream economists' theoretical practices. There is only one thing she does *not* see: the three meta-axioms, none of which are visible to the naked eye.

Note how instrumental to the discursive power of neoclassicism is the fact that its three foundational axioms are hidden from our onlooker's view. For if they were evident, she might start asking difficult questions for which, as we argued above, neoclassicism has no real answers (except to rephrase its axioms). This helps explain, in more than one way, the authoritarian dynamics and the disdain shown towards pluralism of university economics departments that have either managed to rank highly within mainstream economics or are striving to do so.

We suggest that there are two equally important types of explanation of neoclassicism's evolution into an authoritarian research project that discourages pluralism: One is a type of *intentional explanation* while the second is a *functional explanation*. The intentional explanation is simple enough and runs as follows: when an inquisitive graduate student, or academic, who has mastered neoclassical technique but has started developing doubts, starts questioning the meta-axioms, she is effectively questioning the hegemony of her profession. At best, her queries and arguments are met with sympathetic nods, at worst with a great wall of dogmatic put-down lines and an avalanche of advice to the effect that these are matters she ought to worry about after retirement. Publishing in the 'good' journals is hard enough. Publishing articles that question the meta-axioms is even harder. Indeed, it takes a foolhardy young soul to jeopardize a hard-earned career path in pursuit of the truth status of one or more of the meta-axioms which allow the profession to flood the journals with mathematical models that are so highly regarded and so little discussed. And as is so often the case with dominant paradigms, self-censorship is the predominant vehicle for neoclassicism's unimpeded march.

The functional explanation adds an interesting twist to the same tale of intellectual authoritarianism. If phenomenon X is functionally to explain the occurrence of phenomenon Y, this explanation has merit if and only if the following four conditions are met (see Elster 1982): (1) Y must be beneficial

for some group of agents Z. (2) Members of group Z must be responsible for the practices that cause X but must not intend to bring Y about through practices that result in X; indeed, Z members must remain innocent of the causal link between X and Y. Lastly, (3) phenomenon Y, which is caused by X, must be shown to reinforce X through a feedback mechanism involving, unintentionally, members of group Z.

In our case, Y is the discursive power of neoclassical economics, X are the practices that keep neoclassicism's meta-axioms hidden, and Z is the set of neoclassical economists. Can a convincing functionalist explanation of how X causes Y be built along the lines sketched above? If it can, then we shall have an interesting (and possibly correct) explanation of why pluralism is absent from economics departments: its radical absence, which is guaranteed when an eerie silence engulfs the three neoclassical meta-axioms, emerges as a *prerequisite* for neoclassicism's dominance. Let us now put together the basic elements of such an explanation.

Before we proceed further, it is important to note that the merit of this functional explanation is that it is entirely consistent with a distaste for conspiracy theories. As it will transpire shortly, the offered explanation does not presume neoclassical economists in cynical pursuit of discursive power; no theorists are imagined who silence subversive voices within the profession so as to preserve the power vested in them by their models (see part 2 of the argument above which rules out such intentional cynicism). In fact, our explanation works better when most neoclassical economists would have been (honestly) appalled at the thought that we suspect their practices as driven by anything other than scientific rigour. From experience, we can confirm that most neoclassicists believe strongly in the theoretical superiority of their models and may even have a moral commitment to pluralism. Nevertheless, even if we accept that these fine sentiments are all-pervasive in the economics profession, our argument still stands.

To render coherent the functional explanation of neoclassicism's discursive power as the result of a general 'silence' regarding the three meta-axioms at the bottom of all neoclassical theory, we needed three arguments: The first (see [1] above) is that neoclassicism's power is beneficial for neoclassical economists (this is self-evident). The second (see [2]) is that neoclassical economists are innocent of the charge that they are keeping quiet on the three meta-axioms intentionally, so as to enhance their method's discursive power (we accept, therefore, their own denials that they would have conceivably done such a thing). The third piece of the jigsaw (see [3]) is the crucial one: we must now demonstrate that 'phenomenon Y, which is caused by X, reinforces X through a feedback mechanism involving, unintentionally, members of group Z'.

In other words, it must be argued convincingly that the enhancement of neoclassicism's discursive power, which is largely due to the hidden nature of

its three meta-axioms, makes it *even less likely* that neoclassical economists will be open to a pluralist debate on their meta-axioms. Anyone who has worked in an economics department has surely experienced such a feedback mechanism. Research funding in economics is vast compared to the trickle that finds its way to the 'other' social sciences. It would not be forthcoming if economists regularly experienced philosophical angst regarding the axiomatic foundations of their wares. Naturally, the bulk of the profession's funding goes to practitioners who do not indulge in methodological debates, who simply 'get on with the job'. No one wants to keep quiet on the meta-axioms. They are just too busy building magnificent edifices on top of them, and being magnificently rewarded for it.

Nobel laureate Vernon Smith almost apologized, in a recent article (see Smith 2002), for entering into a methodological discussion of the work he devoted an extremely productive life to. This is typical of the fear of methodological discussion instilled in the best and even the most liberal minds in the economics profession. By whom? By no one is the honest answer. The death of pluralism in economics is a crime without a criminal. It died long ago as a result of a particular dynamic within the profession which, operating behind the backs of even neoclassical economists, encourages them to produce all sorts of models (even of altruism and revolution, see Roemer 1985) but surreptitiously penalizes any deviation from, or even explicit discussion of, the three meta-axioms.

Of course, the pressing question is: *Why are public and private funds so uncritically lavished upon what turns out to be no more than a religion with equations?* Alas, this is a question that the present chapter cannot answer within a purely methodological context. For such an explanation we need to venture into political economy (see Arnsperger and Varoufakis 2005, for an attempt).

Epilogue

Neoclassical economics, despite its incessant metamorphoses, is well defined in terms of the same three meta-axioms on which *all* neoclassical analyses have been founded since the second quarter of the nineteenth century. Moreover, its status within the social sciences, and its capacity to obtain research funding and institutional prominence, is explained largely by its success in keeping these three meta-axioms well hidden. The radical lack of pluralism in mainstream economics is, on this account, not to be blamed on illiberally minded practitioners. Rather, it is to be explained in evolutionary terms, as the result of practices that reinforce the profession's considerable success through diverting attention from the models' axiomatic foundations to their technical complexity and diverse predictions. A pluralist economics

will remain impossible so long as the social economy rewards economists in proportion to their success in keeping their models' foundations opaque.

Notes

1. See Aspromourgos (1986), for a history of the term 'neoclassical economics'.
2. Not to be confused with actual, psychological satisfaction. In this sense, *homo economicus* may maximize his preference satisfaction while feeling suicidal.
3. Once upon a time, we could have instead talked of *methodological rationalism* as the dominant narrative centred on agents acting rationally. But since ordinal utilitarianism took over, there is no sense in narrating behaviour in terms of agents acting rationally. Instead, rationality is reduced to the consistency of one's preference ordering which, by definition, determines what agents will do.

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2
Pluralism, Formalism and American Economics[1]

DAVID COLANDER AND HARRY LANDRETH

Economics evolves in fits and starts as it struggles to come to an understanding of the economy and to provide some guidance for policy. In this evolution there has been an ongoing debate between 'formalists', those economists who believe that the study of economics should consist of a highly formal analysis of the economy, and 'nonformalists', who believe that a less formal, process-oriented analysis of the economy, including relevant historical and institutional elements, is the more appropriate model for economic analysis. Although Leland Yeager falls into the nonformalist category, he is unusual in that he also falls into the committed pluralist category, and he is always considering and integrating subtle ideas developed from formalist models into his work. His wide-ranging scholarship has enabled him to integrate a sense of history and institutions into his analysis, and while he has consistently avoided any mathematical presentation of his ideas, the ideas he addresses are those addressed more by formalists than nonformalists.

Although Yeager's analysis is nonformal, it is, nevertheless, highly rigorous; his views are always well thought out and supported by impeccable logic. But, except among his ardent admirers, his work has not had the impact that its cogency deserves. The reason lies in part in the very attributes of his work that give it its strengths. It is iconoclastic – logical unto itself but unbending in its dedication to the exposition of the institutional realities of the time. Be it in his interest in Interlingua, his theory of money, his consideration of the role of ethics, or in his consideration of what Austrian economics is all about, one can be sure that Leland's work will provide enormous insight but also that it likely will be out of step with the mainstream profession's thinking. He could have expressed his ideas in a formalistic manner, but he found that approach a less than optimal way of expressing them, because it would not allow him to point out the subtleties of the argument that went beyond the math. Thus, his work was rich in institutional detail that was impossible to include within a formalist

presentation of those ideas, but at the same time was concerned with the ideas that the formalists were concerned with, not the ideas that the non-formalists focused on.

Recently there have been a number of considerations of formalism, pluralism, and their relationship to the evolution of economic thought over the past hundred years.[2] In Morgan and Rutherford (1998) there seems to be a sense that formalism is bad and that nonformalism is inherently pluralistic and good, and that, in an ideal pluralistically committed world, being out of step with the mainstream should be a strength. In a profession devoted to a pluralist methodology, researchers would turn to those who are out of step for applicable solutions, because the insights one might gain from them would likely be higher than from other sources. Leland Yeager certainly would be considered pluralistic and open; in his work he has demonstrated a willingness to give every view consideration, and he has always dealt seriously with those that he felt met his standard of insightfulness, regardless of whether they advanced an 'in' theory or not. He follows a self-described libertine approach to methodology.[3] His argumentation demands rigour but is almost impervious to ideological positions – he criticizes mainstream, Austrian, and radical economists with equal vigor.

Yeager is in a small minority in following this pluralistic approach on either side of the formalist/nonformalist divide. Commitment to a pluralistic approach is not a characteristic of the profession – and, in our view, his commitment to pluralism has played an important role in reducing his work's influence.[4] Our argument is that a pluralist methodology, such as that practised by Leland, and that supported by Morgan and Rutherford, is not a systemically stable methodology. This presents a problem for researchers committed to a pluralist methodology: How does one exist in a world that is not committed to pluralism? We see this question as a Yeageresque question; for Leland there is no ideal world, there is only the world we live in. And in this world the periods of pluralism that we observe generally have not come about because researchers have made a commitment to pluralism, but instead because various opposing methodological groups have found themselves of roughly equal strength. The reality is that if you fall outside the methodological mainstream of your time, your work will get less consideration than it otherwise would. It follows that, other things being equal, methodological libertines such as Yeager will have less success than methodologically committed individuals. Our argument is not that this situation is good – in this chapter we take Yeager's commitment to what is, rather than to what should be, seriously, and simply say that this is the way it is: a commitment to pluralism is not an evolutionarily stable strategy.

We raise these issues because they relate to how one might understand the history of the profession over the past century. Specifically, Morgan and

Rutherford, having considered that history, have described how the formalist revolution wiped out the pluralism that existed in the early 1900s. In their story what they term neoclassical economics overcame a pluralistic institutionalist approach in the US, with the result that modern economics is far less pluralistic than it was earlier.[5] They seem to lament both the formalization of economics and the loss of pluralism that occurred in the period between World War I and World War II. We find that story unsatisfying. We see the pluralism that existed then as a byproduct of other forces. It was simply a temporary part of a dynamic process in which the formalist and nonformalist methodological positions were of somewhat equal strength. None of the players in the interwar period was so dominant that others were excluded from academic appointments at important graduate programmes, from space in the major journals, from representation in the power structure of the American Economics Association, or from research support. As we will show, our approach provides a different view of the formalist revolution over the past hundred years from that found in previous studies. Ours is a process-oriented view of the profession in which ideas compete given the institutional realities of the profession. Those that succeed are those that meet the institutional requirements of survival. The 'truth' or 'appropriateness' of the idea is only one of many factors deciding the success of an idea.[6]

The alternative story we tell is one in which pluralism has occurred by default, as the profession has swung from a nonformalist to a formalist methodology, as one side or the other gained prominence while holding an unpluralistic methodology. Formalism and nonformalism are both disequilibrium situations to which, over the broad course of the history of economics, have swung like a pendulum, from one side to another, and will likely continue to swing indefinitely in the future. Given this pendular swing, our argument is that, when viewed in its historical context, the past century is best seen not as a movement away from pluralism, but simply as part of the swing of the pendulum.

In our 'process' view, a pluralist methodology in which individuals are actively committed to pluralism has seldom been the nature of the equilibrium; it is simply a state in the evolutionary process in which competing sides are of relatively equal strength. Thus, in our view the unpluralistic formalism that emerged in the latter half of the twentieth century was a temporary state, one that, in our view, is already changing. Today the formalism of that period is combining with the informal work of earlier times, creating a new type of economics that is inductive, highly mathematical, and institutional.[7] This chapter, however, is concerned with the ascendancy of pure formalism, not its current demise, although we will briefly discuss that demise in our concluding comments.

The Swinging Pendulum

The ongoing debate between formalists and nonformalists can be seen in the approaches of the major economists of classical economics. Smith was a nonformalist, Ricardo a formalist. Mill moderated Ricardo's formalism, while post-Millian economists diverged as to which track to take. In the late nineteenth century the battle between the two approaches peaked in the famous *Methodenstreit* that pitted the German historical school against the newly emerging marginalists. This *Methodenstreit* set the backdrop for the rise of the American economics profession, and, with that rise, the shift of the centre of world economics from Europe to the United States.

At the beginning of the twentieth century, the debate considered by Morgan and Rutherford was between the institutionalist nonformalists and the neoclassical formalists who incorporated the newly emerging marginalist ideas as the centrepiece of their approach to economics. The initial debate, however, was nowhere near as stark as it might have been, because at the time the primary standard-bearers of the formalist views were, in large part, Marshallians. From a formalist perspective, this period was hardly pluralistic. In fact, as Blaug notes in 1930, 'it is doubtful that there were more than a half-dozen economists in the world who had ever read Walras, much less understood him' (Blaug 2003, p. 150).

Marshall's approach to economics was itself a compromise approach, using formalist techniques but then moderating them with history and institutions at every point. Marshall's approach was essentially a straddle between the German historical school and the marginalist formalists. Thus, contrary to what is implied in Morgan and Rutherford (1998), from the perspective of a formalist the 1930s were hardly pluralistic. What would at that time be called the super-formalists, such as Edgeworth and Walras, were in a small minority in the US during the interwar period.[8]

Why This History Is Important

The long history of battles between the two sides is important because of the perspective it adds to the transformation of economics that has occurred since the 1930s. It strongly suggests that whatever pluralism existed in the interwar period was a tenuous pluralism existing because neither side had eliminated the other, not a pluralism grounded in pluralistic methodological foundations. The history of the development of the economics profession in the US is one that abounds with intrigue, hostility, and warfare between advocates of the different views (Barber 1988). Given this lack of a pluralistic

methodological foundation, the transformation away from pluralism that occurred in the era after World War Two is about as surprising as the tipping over of a coin standing on its edge. The relevant question is not: Why did the coin tip? It is: Why did the coin land on the side that it did? Specifically, why did superformalism become the centre of the American economics profession?

This question is even more interesting given the starting point of the debate between the formalists and nonformalists. True formalists had a minimal presence in the US at the beginning of the interwar period. Thus, to understand the history of the profession, one must understand how this small group emerged from World War Two as the strongest group and how the institutionalists and Marshallians, who were strong at the turn of the century, eroded. In our view, two interrelated issues explain these events: the failure of the institutionalists' research and pedagogical programme to meet the institutional requirements of an ongoing research programme within the US institutional environment, and the instability of the Marshallian straddle. We will argue that the transformation was essentially bipartite. First, it was a victory of the coalition of formalists and Marshallians over the institutionalists. Second, it was a victory of the formalists over Marshallians.

The Players

Let us begin by briefly considering who the players were in the early 1900s and in the interwar period. Those players can be divided into three loose groups that represented divisions similar to those that existed in Europe at the time. The largest group was the institutionalists. This group represented the German and English historical-institutional approach to economics as a discipline and contained a number of German-trained PhDs. However, the principal intellectual force in this group came from American-trained Veblen, Commons and Mitchell.

The second group was what we will call formalists. This was the smallest group. It roots were not in Smith, but rather in Cournot, Jevons, Walras and Edgeworth. This group was influenced by contemporaries – the English economists Edgeworth, Bowley and Wicksteed, and the Swedish Wicksell. Simon Newcomb was a member of the group, but the towering American figure in the early years of the twentieth century was Irving Fisher.

The third group was a swing group between the two. It probably best goes under the name Marshallian, because its methodology and approach closely followed Alfred Marshall. Marshall had masterfully built an economic engine of analysis that tried to straddle the institutionalist and pure formalist schools. It argued for a type of pluralism in which no rigid lines were drawn

on almost any issue of scope, method or content, and all were welcome under the big tent.[9]

At this time a distinct Austrian school did not exist; it was simply part of the Marshallianism that characterized the period. By 1900, the beginning of the time frame we are mostly concerned with, the existing main contributions of those who later became called 'Austrians' in the minds of most economists of the time, had already been incorporated into the Marshallian views of the time, views that came to be called neoclassical economics.[13]

The Victory of the Coalition of Formalists and Marshallians over the Institutionalists

In the early part of the twentieth century, institutionalists were the most powerful group. Thus, the first part of the story is their loss of power. That loss was in many ways due to the institutionalists' failure to meet the institutional requirements of an ongoing research programme within the economics profession's institutional structure. To see this we need to look more closely at the three groups of institutionalists who, though never united in a coherent research programme, came to be linked to one another primarily by their opposition to theory, whether it be formalist, or Marshallian. Thus the glue that held institutionalists together was not a positive glue, but a negative glue.

To give you an idea of their opposition to Marshallian neoclassicism, consider Veblen's mockery of the assumption of rationality in his chapter 'Why Economics Is Not an Evolutionary Science':

> The psychological and anthropological preconceptions of the economists have been those that were accepted by the psychological and social sciences some generations ago. The hedonistic conception of man is that of a lightning calculator of pleasure and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. (Veblen 1919, pp. 73–4)

Wesley Claire Mitchell, in a letter to J. M. Clarke, made even more biting comments about the formalists. In explaining why he could not take neoclassical theory seriously, he compares the grand theorist to a great-aunt with whom he argued when he was young. In arguing with that great-aunt, who 'was the best of the Baptists, and knew exactly how the Lord had planned the world', he found when he presented her with logical difficulties that her simple scheme could not handle, she always 'slipped back into the logical scheme, and blinked the facts', just as the grand theorists do. For Mitchell, developing grand theories was child's play. He states, 'Give me

premises and I would spin speculations by the yard' (Mitchell as cited in Clarke 1936, pp. 410–11).

While all institutionalists agreed on the problems of neoclassical economics, they did not agree on what should replace it. This meant that institutionalism went in three disparate directions. The sons and daughters of W. C. Mitchell never became institutionalized in any academy in the sense that there was a graduate education programme in economics founded on the research philosophy of Mitchell. The National Bureau of Economic Research and other agencies initially pursued his empirical approach, but with the development of econometrics, which supposedly offered a way of integrating theory and measurement, Mitchell's empiricism died out. The reasons this change from Mitchell's empiricism to econometrics occurred – and assessments of it are complicated – are only now beginning to be understood. But it is clear that the initial belief that econometrics offered a way of integrating theory and empirical work that tested theories was an important element in the fall of Mitchell's brand of institutionalism and in the transition. In this transition Keynes's *General Theory* played a significant role, providing the needed push to both the collection of macroeconomic data and the building of macroeconomic econometric models, and thus precipitating the demise of Mitchell's approach.

The Veblenese part of institutionalism was, in large part, unique to Veblen. Mitchell rejected it, and while almost all will agree that Veblen's approach was highly insightful, it offered little that ordinary students could build upon. Veblen's approach was carried on largely in the work of Clarence Ayres and his students. In retrospect it appears to have been a non-viable research programme, with PhDs receiving training in what was wrong with Marshall and more formal economics but gaining few tools to bring to a positive research agenda. The Ayresians never were able to gain editorial control of a major economics journal, and they often squabbled with editors of journals publishing in the historical-institutional tradition. The criticism that the Ayresians had no analytical framework or research programme led Ayres to write 'The Coordinates of Institutionalism' (1951) which had little impact on the profession. Veblenian-Ayresian institutionalism was fading in post-World War Two America.[11]

While a few Austin (University of Texas) satellites were attempted, they never took hold. One important aspect of understanding the demise of Veblen-Ayres institutionalism is the recognition that over time a communication barrier developed between these economists and the rest of the profession. They and the emerging formalists did not read each other's writings, and both were like visitors in a foreign country with no language skills. The same divide existed between Austrian economists and formalists.[12]

What happened to the Commons-University of Wisconsin part of the

historical-institutional camp is complex and subtle.[13] Here was a progressive research programme with possibly an element more important than the tools: a view that government and intellectuals should work together to help solve some of the social problems created by the industrial society. The union between the state government at Madison and the academicians produced a long list of social legislation. The depression of the 1930s found a cadre of academicians ready to go to Washington DC to apply the Wisconsin model of government–academy cooperation. It is not by chance that one of the foremost advocates of Keynesianism in the United States, Alvin Hansen, was a Wisconsin PhD who brought the Wisconsin model to Harvard in its fiscal policy seminar and began a Harvard–Washington DC nexus which remains today.

The demise of Wisconsin economics is in large part explained by its failure to produce professors who would produce more professors. The chain-letter process of the modern mainstream, whereby graduate professors beget students who become professors and beget more students *ad infinitum* assures continuity and ascendancy, at least until major paradigmatic changes occur. But since the Wisconsin PhDs went primarily to government, undergraduate education and business, no major satellites producing PhDs of their philosophy were established. Part of the demise is explicable by the fact that the ideological position of Wisconsinites about the faults of society and the role of government became accepted, co-opted, and pre-empted by other graduate programmes. As that ideological position played out in the Roosevelt administration, the rest of the economics profession would not go as far as the Commons Wisconsinites in changing the institutional structure, but they were willing to go far enough to create a society in the 1960s very different from that of the 1920s.

The Institutional Cause of Institutionalists' Demise

The link between the demise of the three brands of institutionalism was the failure of each to meet the institutional requirements for survival. Institutionalist economists were seen as anti-theoretical and anti-mathematical. Neoclassical economists were seen as theoretical. Mathematical neoclassical economists portrayed economics as a predictive science that involved specifying a theory and empirically testing that theory. Such a method created large numbers of small jobs, enough to keep an academic army of neoclassical students busy. Institutionalism, however, presented economics as a policy-driven combination of the study of institutions and of empirical facts about the economy, neither of which required a formal theory or definitive – and labour intensive – empirical testing. Given those choices, it

is clear which view would succeed institutionally — and it was not the institutionalist view.

Whether one believes that a grand theory is true in some fundamental sense is irrelevant. Even if you do not *believe* a theory, it can still be useful in the metaphysical sense of organizing one's thinking. Students and, indeed, almost everyone require such an organizational scheme. Neoclassical economics offered one, but only Veblen's brand of institutional economics offered broadly inclusive theory, and it was highly nonformal and indefinite. One reason such formal theories are needed is that, while Mitchell might have been able to twist his great-aunt's arguments every which way, most students cannot perform these kinds of mental gymnastics: they need an organizing structure for their study. Most people need a simple structure to organize complex principles in their minds. Neoclassical economics offered such a simple organizing principle; institutional economics did not. The lure of neoclassical economics mimics the lure of religion in being a relatively simple way of organizing one's understanding of an otherwise almost hopeless chaos.

This need for a formal organizing theory was strengthened by the structure of US higher educational institutions which typically emphasized a broad-based educational system in which large numbers of students were enrolled in economics courses. In practical terms that necessitated the use of multiple-choice tests. The institutionalist approach to economics with no accompanying formal theory did not fit well into that system. There are only so many times that 'it depends' can be given as an answer.

In the eyes of the institutionalists, the simple neoclassical models did not come close to corresponding to reality. They recoiled at the disparity between the simple model and the observed reality. Students who shared an institutionalist sensibility typically either abandoned the study of economics or were weeded out, since they were unable to bring themselves to provide the simplistic answers to complex questions that the educational system required of them. Those who appreciated the simplicity of the neoclassical models did well in exams and went on to create more complicated versions of these models: they became modern economists.

What we are arguing is that having a branch of economics working on a formal grand theory was a requirement of survival in the US educational environment. Lacking a grand theory reducible to simple textbook models, the institutionalists' complex economic worldview was incompatible with the pedagogical institutions through which economic ideas were propagated. The institutionalists' decision simply not to discuss formal theorizing rendered them incapable of competing in the metaphysical grand theory realm; meanwhile the neoclassical worldview *succeeded* in providing a system whereby students could organize their thinking about the economy. Once

the simplicity of that worldview was built in, moreover, it was not questioned, and it soon became the norm by which economists approached their work. Little consideration was afforded the implications of the institutionalists' complexity leap of faith, while more and more elaborate theorizing was developed on the simplicity leap of faith.

An ongoing research programme needs to excite students, and provide dissertation and article topics for them to work on. These dissertations and articles must lead to jobs at other universities, producing future PhDs so that the research programme can replicate itself. All three branches of institutionalists failed to do these things, although for different reasons. Commons's students went on to government; so in a sense his branch planted no seed corn. Mitchell gave students no organizing principles. While his mind was large enough to spin out millions of theories, and organize empirical work, most students were not up to the task of following his lead. They gravitated instead to the clarity of neoclassical theory and econometrics, even if it did not fit reality. Veblen required students to be as insightful and as good an expositor as he; most weren't. Thus, institutionalism failed institutionally, and its demise was speeded up by the enormous growth of universities, requiring large numbers of new PhDs during the era that followed World War Two.

The Victory of Formalists over Marshallians

The above section explains our view of why institutionalism lost the battle with Marshallian economics. Had that been the end of the story, the pluralism of Marshallian economics would typify post-World War Two American economics. But that was not the case. Instead, soon after World War Two, Marshallian economics began to fade, and with it, the methodological pluralism that characterized it. By the early 1960s, Marshallian economics was totally overwhelmed by a formalist economics clothed in a methodological straitjacket.

To understand why this second transformation occurred, we need to look more carefully at Marshallian economics. One can view Marshall's economics as an attempt to prevent either side of the long-continuing battle between formalists and nonformalists from winning. Marshall argues that what is needed is the broadest of scopes, methods and content, with some problems and issues more satisfactorily pursued by less rigid, more historical-institutional approaches, and other problems and issues by more formal abstract analysis. It all depends, said Marshall. This 'it depends' answer irritated both of the other groups. Marshall irritated the would-be formalists in his Appendix B of his *Principles of Economics* (Marshall 1961) praising

Adam Smith as a model of method; in Appendix C, 'The Scope and Method of Economics', and in Appendix D, 'The Uses of Abstract Reasoning in Economics', where he commended the methodology of the German historical school; in his widely circulated letter to Bowley deprecating the role of mathematics and abstract reasoning in economics; in his refusal to give precise definitions of economics, factors of production, or the representative firm; and in his *Principles of Economics* in which he preaches that 'a man is likely to be a better economist if he trusts to his common sense, and practical instincts …' (Marshall 1961, 368). The institutionalists were similarly irritated by Marshall's attempt to take what he regarded as something from all sides. They saw him as essentially accepting neoclassical theory and then slightly modifying its application.

Being the pluralist he was, Marshall was extremely hesitant to draw policy conclusions from economic theory. He believed that policy issues required normative and institutional judgements that had to be added back to any logical-deductive theoretical model before policy conclusions could be drawn. Policy conclusions did not follow from theory alone.

Marshall's hesitation to associate policy arguments with economic theory has been noted by Hirsch and de Marchi. They point out that for Marshall the analysis of direct incentive effects was only a starting point of his analysis of taxes (Hirsch and de Marchi 1990, p. 161). Another example they give is Marshall's consideration of the question of import duties. In that consideration Marshall lists a variety of specific questions that need to be answered before one can come to a policy conclusion. They write:

> Marshall operates not as a theorist who sets up his assumptions and then 'reasons out' (to some general conclusions for hypothetical categories of cases), *but as one who actually has to give advice, or to make the decision in favor of one tax over another, or for no tax at all* [emphasis supplied]. He cautions frequently against making direct application of the results of simple first-round impact analysis. A prefatory note in his Memorandum, for example, points out that 'the incidence of import duties is extremely complex' and he adds: 'the indirect are often much more important than the direct effects'. … Marshall also warns that although the exposition to follow is concerned chiefly with 'proximate causes and their effects' a student should actually be 'endeavoring to probe to the causes of causes'. (Hirsch and de Marchi 1990, p. 162)

Despite the fact that Marshall worked assiduously not to fall into any particular methodological or policy position, his partial formalization gave a suggestion of scientific aura to the results of models. Marshall's concept of consumer surplus seemed to make it possible to draw policy results from analytic models. We can see this in Pigou's proposal to subsidize industries, in the development of cost–benefit analysis, and in the enormous focus of the economics profession on efficiency and waste to the exclusion of other

issues such as the inability of government to implement proposals, or the information transfer role of prices. Thus, when there was a debate about market socialism, it concerned technical issues, and the subtlety of Hayek's arguments against socialism was lost until rediscovered in the 1980s.

The Instability of Marshall's Straddle

What we are arguing is that, while Marshall's pluralist methodological approach worked for him, just as it worked for Leland, it was not transferable. In the hands of a less committed pluralist, such as Abba Lerner or Milton Friedman, the Marshallian approach provoked reactions against it that undermined pluralism in the post-World War Two era. Marshall's strength was his ability to do formal theory and simultaneously to recognize the limitations of his formal model. But many of his followers did not; they drew policy conclusions from the theory, which set up a problem for other researchers: to show how, analytically, those conclusions did not necessarily flow from theory: or that they were based on a particular assumption. Thus, Marshall's partial formalization was unstable; it set in motion a chain of formalizations, each one demonstrating that the previous formalization was incomplete – and inconclusive – with regard to policy.

Perhaps the most obvious partial formalization that Marshallian economics brought into the profession was the elevation of the partial equilibrium supply–demand diagram to centre stage. This elevation created an almost totem-like model that shaped students' vision and understanding of economics. Within this supply–demand view, economics issues weren't complex: they were simple, and could be answered in reference to the supply–demand diagram. Institutions weren't important: they were simply frictions that slowed the forces of supply and demand. The market existed: it drove the economy to a desirable equilibrium, and any restriction on the market was bad.

Marshall's vision of economics was far more complex than this, but that complexity did not come through the supply and demand diagrams. As those diagrams became institutionalized, Marshall's broader pluralism was lost. Thus, it was Friedman who picked up the mantle of Marshallian economics in the US, and he used it to push a laissez-faire policy agenda.[14] In Friedman's hands, Marshallian economics led to laissez-faire policy conclusions (see Friedman 1953), just as in Lerner's and Pigou's hands it led to activist policy conclusions.

In the 1930s the supply–demand diagram was expanded upon and expanded upon. It was in the 1930s that the standard monopoly concepts were created, and many of the geometric tools that are now standard in introductory and

intermediate microeconomics were introduced. This geometricization of economics started a shift within Marshallian economics – towards less focus on historical and institutional detail and more on formalization.

An example of Marshallianism in America is the theory of monopolistic competition of E. H. Chamberlin. Chamberlin had neither Marshall's mathematics aptitudes nor broad interests in historical materials. The theory of monopolistic competition is in Marshall, although never formalized. Chamberlin's formalization of it used a combination of words and graphs. The result was something of a muddle, but one that could be taught neatly to undergraduates. It was inconclusive, and it was unclear how it related to a theory of oligopoly, which was, observationally, much more prevalent in the economy.

The reality was that markets between pure competition and pure monopoly required a mathematics that could deal with the mutual interdependence of actors, and that was beyond the mathematics of the time. The Marshallians formalized the presentation sufficiently to make nice neat geometric models that provided excellent teaching tools for students, but doing so naturally led to more formalization. The pedagogical use of these models elevated their policy conclusions from logical games to formal policy arguments.

The formalization of economics allowed by geometricization, no matter how complicated the diagram, was highly limited – it reduced everything to two, or at most three, dimensions. This limitation invited mathematically oriented economists to correct the errors, which led to publications, advancement in the academic profession, and the propagation of further formalism to clear up the problems of the last level of formalization.

The limitations of partial equilibrium analysis were recognized early on, and in the 1930s the work of Abraham Wald and John von Neuman on equilibrium conditions of static and dynamic models turned the heads of mathematically trained economists towards general equilibrium theory. As Samuelson cogently noted, 'To a person of analytical ability, perceptive enough to realize that mathematical equipment was a powerful sword in economics, the world of economics was his or her oyster in 1935.' (Samuelson 1964, 315). Thus, beginning in the 1940s, economics began considering issues in a formal mathematical manner nicely described by Blaug.

The movement was first towards a calculus formulation of general equilibrium and then towards set theoretic formulation of general equilibrium in which the existence of equilibrium was a key issue. Our difference with Blaug is that we see this work developing because of the Marshallian straddle, which led to a combining of theory and policy that made it look as if results were being pulled from economic theory that, in fact, could not be

pulled from it. The formal work in general equilibrium theory caught on because it showed the *limitations* of theorizing, not its *strengths*. It showed the enormously strong assumptions that were necessary to draw out any actual information from the theory much more than it showed the power of the theory to explain real-world events.

As often happens when something develops in reaction to something else, it sets in motion a set of forces that swing the pendulum too far in the opposite direction, and that happened in the 1960s and 1970s. Microeconomics became the formalist game that Rosenberg (1992) has described, moving to higher and higher levels of abstraction. Initially macroeconomics was immune to this movement; but in the 1970s the push to carry out the logic of macroeconomics in the Walrasian unique equilibrium led to the new classical revolution.

Formal general equilibrium theory, as contrasted to Marshallian partial equilibrium theory, could not be studied or applied without considerable training in mathematics. When that training was added to the graduate school curriculum, the formalists' victory began to fall into place. Sometime in the 1950s, the economist's toolbox required for holy anointment began changing. The requirement for two foreign languages was replaced by mathematics-quantitative proficiency, and economic history and the history of economic thought went the way of the dodo bird. As that happened the curricula of graduate economic programmes changed, the editors and content of the major journals changed, and the types of individuals who were becoming economists changed. All of these forces finally prevailed in the 1960s, at least temporarily. By the 1970s, if you wanted to be considered a theorist, you had to play by formalist's rules: the formalist pendulum swing was at its peak.

Some Final Comments and Some Thoughts about the Future

Let us conclude by briefly summarizing our argument. The evolution of the economics profession can best be seen as a pendulum swinging between formalism and intuitive approaches. The nature of the swinging pendulum can best be understood in reference to the institutional structure of the profession and the changing analytic and computing technologies of the time. Pluralism is highly unlikely to exist at any given time because researchers favouring either an intuitive or a formal approach have a commitment to pluralism. Hence, when pluralism does exist, it will be simply as a temporary state in which various sides are at a point where neither has won out. Thus, in our view, during the 1930s there was no pluralism in the sense of a profession committed to a pluralist methodology,

there was simply a temporary position in the swing of the pendulum in which competing sides were of relatively equal strength.

Formalism started winning out in the 1930s because of the failure of nonformalist schools to meet the institutional requirements for survival. The nonformalists tried to become too policy-oriented, and seemed to be arguing that one could draw out policy conclusions from positive economics. Formalist writing delineated the problems with that position, but in the process created a set of institutions that kept the pendulum swinging towards formalism. Analytic and computing power also changed during this period, causing applied work to become more technical – and more useful. It is important not to confuse the formalism of Hilbertian general equilibrium theory that Blaug is describing as formalism with the highly technical applied mathematics and econometrics that characterize much of the modern applied work in economics. That work is technical but nonformal. This increase in the technical nature of economic analysis is not an increase in formalism; it is simply a reflection of a change in technology. Whereas Marshall had to rely on observations, today we can rely much more on technical data analysis. Vector auto regression is highly technical, but it is not formal theorizing. Similarly, much of modern applied mathematics is nonformal: researchers are not concerned with proofs but rather with pulling information out of data.

Turning to the implications of our argument for the future, we see the following: The profession is now in a period of change. The formalism described by Blaug is on the wane, as developments in computer technology have made analytic theory less useful. Today, instead of writing a general solution to an abstract problem, it is easier to provide a solution for a specific problem. As that happens, the profession is moving from pure mathematics to applied mathematics (Weintraub 2002). The same is true in statistical studies. With the development of computers, statistical patterns such as those searched for by Mitchell can now be found, and consequently cointegration and vector autoregression techniques which pull information from data with minimal theory are flourishing, and they are replacing the need for theory. Similarly, agent-based modelling is allowing economists to analyse models with heterodox agents and incomplete information that previously were beyond consideration. All these methods are mathematical but not formal. They are essentially tools of inductive rather than deductive analysis, and they are likely to characterize the economics of the future. We believe this because each of these new developments is article-laden, which will meet the institutional requirements of survival for the economists who study them. As they become entrenched in decision-making positions in the profession, the formalism of the 1950s, such as that found in general equilibrium analysis, will further fade, and that solid inductive analysis combined with a sharp

intuition and a rigour of the sort that characterized Leland Yeager's work will be making a comeback, albeit in a quite different form.

Notes

1. An earlier version of this chapter was presented at a History of Political Economy conference at Duke University, North Carolina, in April, 1997.
2. For other views of the reorientation of economics, mostly complementary and compatible with ours, see Blaug (1998, 2002, 2003); Niehans (1990); and Samuels (1998). Very different conclusions are reached by Morgan and Rutherford (1998), and Yonay (1998). While this chapter focuses on the changes that took place in American economics during the twentieth century, the importance of American graduate education in economics strongly suggests important ramifications for the development of non-American economic thought.
3. As Yeager points out, by this he does not meant that 'anything goes, or that whatever one comes up with is automatically valid'. He is simply saying that one should 'let people work with whatever method works for them, and fits with their talents and inclinations'. Yeager (1988)
4. Our argument is not that he was wrong in holding his views; only that holding those views reduced his influence.
5. There are many dimensions of pluralism. There can be pluralism in policy proposals, where the profession comes to multiple mainstream positions on policy. We have not seen a significant post-World War Two decline in policy pluralism. Where we believe there has been a decline in pluralism is in methodological pluralism. There is less diversity of approach today than there was in the 1930s. It is that aspect of pluralism that we focus on in this chapter.
6. For a further development of this idea, see Colander (1991).
7. This view of economics is developed in Colander (forthcoming) and Colander, Holt, and Rosser (forthcoming).
8. Since this debate between the formalists and the nonformalists plays such a central role in the transformation of American economics, it needs to be clarified. It is not a debate between those who favour mathematics and those who don't. It is a debate about the worldview that individuals have concerning the complexity of the economy, and the usefulness of formalizing discussions of the economy with the mathematical tools that exist at the time. Nonformalists believe that the mathematical tools available at the time are insufficient to capture the complexity of the economy, whereas formalists believe that those tools are sufficient.

   What this means is that as mathematical tools change, people's view of the usefulness of a formal approach may change. For example, with the recent developments in mathematics such as chaos and catastrophe theory, and with the increase in the ability of computers to handle difficult problems, views of whether formalism is useful can be quite different today than they were in the 1930s when the tools involved relatively simple differential calculus, and almost no developed statistical analysis.
9. We see Leland's methodology as similar to Marshall's. In many ways Leland was the consummate Marshallian straddler.
10. Austrian economics developed as a separate school only later in the 1970s as a group of economists worked hard to organize themselves into a separate school.

11. As an example, consider the path of one of the authors. He, together with three other Texas economics PhD candidates from the University of Texas at Austin, transferred to the PhD programme at Harvard during the mid-1950s. They developed into a separate group: their basic framework was so different.
12. This doesn't mean that the Austrian economists' ideas weren't correct or better than the emerging ideas; it simply means their ideas no longer were compatible with the institutional structure of the emerging shape of the economics profession's institutions.
13. Lampman's *Economists at Wisconsin 1892–1992* (1993) may trigger research that will produce clearer insights into what happened to the Wisconsin school.
14. See Colander (1995) for further discussion.

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3
The Construction of Economics

KYLE SILER

Many of the articles published in outlets friendly to heterodox approaches to economics focus on challenging and/or refuting mainstream economic theory.[1] This tacitly serves as a means of precipitating further thought about economics, and, in most cases, also functions as a means of promoting change in the discipline and beyond. However, as evidenced by history, be it the notion that the Earth revolves around the Sun, the double-helix model of DNA (see Watson 1969), or the hegemony of mainstream neoclassical economics today, merely having innovative or possibly better ideas does not necessarily equate with the ability to establish immediate scientific and societal acceptance of those ideas, or make widely accepted truth claims. Stephen Cole (1992) bridges a long-standing fissure in science studies and the sociology of science between constructivists and non-constructivists by arguing that all sciences are comprised of both socially constructed and scientific components. It follows that economics should be no different. Hence, understanding, harnessing or changing economics will be a social process, in addition to being a scientific one as well.

My account of the social and scientific construction of economics is largely derived from British sociologist Richard Whitley's (1984) seminal work *The Social and Intellectual Organization of the Sciences*. The crux of Whitley's argument is that in addition to what they study empirically, scientific fields are shaped and affected by the degrees and types of *mutual dependence* and *task uncertainty* they possess. In other words, the things economists study and the tools they examine them with influence the organization of the discipline and *vice versa*. The next two sections will explain how these characteristics exist and function in mainstream neoclassical economics.

Mutual Dependence

Whitley (1984, p. 88) broadly defined *mutual dependence* as 'the need to adhere to particular standards of competence and criteria of significance in

order to reward important reputations for contributions'. More specifically, mutual dependence is comprised of two analytically distinct agents: *functional* and *strategic* dependence. Economics has high *functional dependence*, as economists generally have to adhere to a dominant neoclassical strategic paradigm to be taken seriously in the mainstream of the discipline. Conversely, it also has low *strategic dependence*, as due to this consensus, economists generally spend little time arguing over theoretical issues. Hence, many debates about the theoretical and philosophical underpinnings of economics occur outside of mainstream economic forums (such as is the case with the *Post-Autistic Economics Review*).

Whitley (1984, p. 31) also adds that 'intellectual fields must have distinctive work procedures if they are to function as reputational work organizations'. These distinctive work procedures set the context for self-conscious and self-regulating colleague groups being based 'on their power to validate the expertise, and thus mediate the careers of, members' (p. 20). The often arcane and esoteric mathematical nature of neoclassical economics is a powerful context, contributing to a very strong, unified organizational discipline, thus influencing both the profession and 'science' of economics. Mathematics is not only an effective means of creating scholarly hierarchies, but also makes economic work difficult to comment on (at least in the mainstream economists' domain and language) for those outside the discipline and for economists with less mathematical and technical virtuosity. While gains may be realized from specialization of discourse and method in any science, specialization also fortifies professional and intellectual boundaries between scholarly communities.

However, the many theoretical quirks and idiosyncratic uses of mathematics that characterize mainstream economics also render mainstream economics difficult for other mathematically inclined scientists to fully comprehend. This places control over the discipline largely into the hands of the most advanced mathematical economists, while insulating and empowering the discipline as a whole. It is also the epitome of what Thomas Gieryn (1983) dubbed 'boundary work' as a means of demarcating 'science' from 'non-science'. Social and cultural norms that value abstractness, theoretical complexity, esoteric science and quantification also help make economics trusted, well-supported and respected. As Andrew Abbott noted (1988, p. 16):

> [P]rofessions often legitimate themselves by attaching their expertise to values with general cultural legitimacy – increasingly those of rationality, efficiency and science. Hence, having a theoretical and professional core characterized by esoteric, mathematical abstractions allows mainstream economics to derive rents from employment, scientific and intellectual closure, in addition to benefits from espousing rhetorical and theoretical devices that are commonly seen as 'legitimate' and tend to receive deference in modern societies.

As mutual dependence (which is the basis for much of the power and prestige of economics) increases, local and individual circumstances tend to become irrelevant, and become the turf of less prestigious economists, academics and fields. Espeland and Stevens (1998) offer the concept of *commensuration* to describe this cognitive (and arguably, social) process. Commensuration is defined as the transformation of different qualities into a common metric. The mathematization of social phenomena is an archetypal example of this. Commensuration has the potential of integrating disparate interest groups and phenomena. Thus, it can be seen as a form of organization, even if it is only implicit and cognitive. However, it may also underpin critiques of economics (and often, science in general) that claim it ignores and marginalizes the experiences of certain groups and places in society. Class (for example, Wolff and Resnick 1987) and gender-based (for example, Ferber and Nelson 1993) perspectives are the most common sources of these critiques. Hence, it is not surprising that economics tends to privilege abstract thought, shunning context and historically dependent work.

A number of additional factors are indicative of the high mutual dependence in economics. These include:

- the existence of a relatively small, concentrated, theoretical disciplinary core of economists;
- inimicality to cross-disciplinary, pluralistic and heterodox thought (see Klamer and Colander 1990);
- relatively agreed-upon hierarchies of competence and knowledge;
- insulation from the lay public and most other academics;
- the existence of a Nobel Prize, which serves to galvanize the discipline, and confer significant prestige upon economics as a whole in public perception, and upon the winning economists, who tend to perpetuate further the prevailing orthodoxy.

It is difficult to ascertain whether these characteristics are causes and/or effects of high mutual dependence (on each other). Regardless, this complex interweaving of social characteristics is a significant factor contributing to the power, scope, autonomy and legitimacy of neoclassical economics today.

Economics and Task Uncertainty

The social sciences are generally characterized by a greater degree of task uncertainty than most of the natural sciences. Laboratory controls and manipulation of research subjects are rarely viable options in social science research. Economists cannot manipulate the behaviour and social and historical

contexts of governments, firms and actors in various contexts in order to test and retest hypotheses about economies.[2] Whitley (1984, p. 120) observes that 'the more paradigm-bound a field is, the more predictable, visible and replicable are research results, and the more limited is permissible novelty'. Hence, the degree of task uncertainty in a field is influenced by a socially constructed component, via the social organization of a given discipline, apart from empirical, data-based, or so-called scientific considerations.

Whitley (Chapter 4) identifies three major contextual factors that influence task uncertainty:

Reputational autonomy

Reputational autonomy alludes to the degree to which a given field can adjudicate standards of quality and worthiness without influences from other interests. As per Abbott (1988), the ability of a profession to monitor and control the content and membership of its intellectual turf is vital to its empowerment in the larger, overarching competitive system of professions. Mainstream economics is empowered with a very high degree of reputational autonomy. For example, while the government and the lay public are generally unwilling (or unable) to engage in dialogue with academic economists on their own terms, they are willing to be amateur sociologists on such issues as inequality and culture. In other words, economics receives a disproportionate amount of deference from within and inside academia. Further, while some social science departments are prone to being subsumed by topical or interdisciplinary studies in universities, economists are generally immune. In addition, when economists do participate in interdisciplinary work (for governments for example), they usually do so 'on their terms', often as atheoretical data miners and crunchers with scarce quantitative skills, credentials and cultural legitimacy from their professional status. 'Peripheral' economists enjoy these benefits, despite being divorced from the abstract core of the discipline.

CONCENTRATION OVER THE MEANS OF INTELLECTUAL PRODUCTION AND DISSEMINATION

Economics has relatively high concentration in journals, paradigmatic thought, prestige and universities. This is in part a result of (or contributor to) its aforementioned high reputational autonomy. As an example of the degree of concentration of intellectual production in the USA, Pieper and Willis (1999: 86) show that 54 per cent of economics faculties at doctoral universities, and more than two-thirds of the thesis supervisors at the 47 top-ranked programmes in the United States, come from one of the 'top ten' schools. These schools include Chicago, Harvard, Stanford, and MIT, which

are of course among the strongest purveyors of highly mathematical neoclassical economics. As Devine (2002) observed, the more famous the university, journal or student, the more likely they are to adhere to the general tenets of neoclassical economics. The degree of control these schools have over economics education is well evidenced by a report done by the Commission on Graduate Education in Economics in the USA, which concluded that 'the content and structure of graduate programs is amazingly similar' (Hansen 1991, p. 1085).

Audience plurality and diversity

Economics has relatively low audience plurality and diversity, largely due to the practice of conducting esoteric, mathematical research published in academic journals kept largely away from public scrutiny. Economists seldom write books, and if they are written in a publicly accessible fashion, they are often derided as 'lacking rigour', or as 'Galbraithism' (as a slight against the late eminent Harvard economist who was (in)famous for often writing in a publicly accessible manner). Further, academic economics is also shielded by the fact that most public economic debate occurs outside of the academic sphere, far removed from the behavioural assumptions and arcane analyses couched in powerful academic economics journals, and textbooks. This will be discussed further shortly.

All of the above factors, which are all social factors to some extent, serve to reduce the perceived task uncertainty. This is despite the fact that economics operates in the often complex, contextual realms of the human sciences. This apparent contradiction will be explored in the next section.

Economics as a partitioned bureaucracy

Economics is extremely unusual in academia in that it combines the high technical task uncertainty of the social sciences with very low strategic task uncertainty. Whitley (1984, p. 181) states that this mix should be highly unstable *unless the central core of conceptual orthodoxy is partitioned away from empirical sources of uncertainty*. Hence, privileging theoretical data (informed by the central core) at the expense of empirical considerations is a necessary condition for maintaining strategic consensus in the discipline. Mainstream economics does exactly that. As in many facets of economics, there is a clear hierarchy (made possible by high mutual dependence) of subfields in economics, with the more theoretical endeavours enjoying epistemological and organizational superiority. This occurs both within and outside of economics. Within economics, econometrics, labour and health economics, and other relatively applied work remains subordinated to, and to a certain

extent derivative of, the dominant paradigm, couched in the theoretical core of the discipline. This also appears to be indicative of Abbott's (1988) thesis that professional groups use abstraction as a means of protecting and amassing intellectual turf and power, in addition to professional prestige. Doing applied or socially relevant work is acceptable to mainstream economists, provided one does not use their experiences in the applied realm to question the theoretical core. Accordingly, as would be expected in Whitley's partitioned bureaucracy, most 'applied' economics work tends to be atheoretical.

Outside of economics, much applied or context-dependent work is actually done in lower-status economics departments, in business/finance and social science departments in universities, and by businesses and governments outside of academia. In the case of business and finance departments using economic theory, there appears to be somewhat of a symbiotic relationship, where applied economists (who may, for example, work in professional schools, government agencies or business) use neoclassical economics for a methodological and moral legitimation. In return, the theoretical core of economics gets insulated from empirical concerns, uncertainties and contingencies that could undermine the strategic consensus, or at least reveal anomalies that could call the dominant orthodoxy into question. This symbiotic relationship also may help contribute to maintaining (if not reinforcing) the 'bourgeois' focus of mainstream economics (which may or may not be intentional), which tends to trumpet the virtues of capitalism more than it criticizes the economic, social and moral shortcomings it may possess.

Concluding Thoughts

John Kenneth Galbraith (1984: 3) remarked that the shortcomings of contemporary economics are not necessarily due to original error, but rather to 'uncorrected obsolescence'. Given the intricate tapestry of social, empirical, and organizational factors buttressing mainstream economics today, it is little wonder that the neoclassical paradigm and the economics profession are not opening themselves up to different viewpoints, philosophies and epistemologies. Much of the work by heterodox economists and other critics of mainstream economics illustrates many of the excellent ideas and debates that, at the very least, call into question the dominant economic paradigm both in theory and in practice. However, as Kuhn (1962) made clear, merely being right scientifically is not necessarily sufficient by itself to modify a discipline significantly, especially one as powerful and entrenched as economics. A cognizance of the interplay between the social and the intellectual

underpinnings of economics is relevant to understanding how and why ideas gain precedence in academic and public realms. In turn, knowledge of the social construction of science and economics should be a vital part of constructing any such strategy. As heterodox notions of economics burgeon, it should be kept in mind by such dissenting groups that scientific change is not entirely a scientific endeavour. This awareness could aid the construction of strategies for social and scientific change, both in academic and public realms.

Notes

1. The post-autistic economics movement appears to be an example of what Frickel and Gross (2005) dubbed scientific intellectual movements (SIMs).
2. This limitation also characterizes, in varying degrees, the natural sciences, particularly biology.

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4
Paradigms and Pluralism

ROBERT F. GARNETT JR

Introduction

Heterodox economists are in the midst of a critical transition. The long-standing goal of developing a 'single correct alternative to neoclassical economics' (King 2002) is increasingly giving way to pan-paradigmatic agendas and forums such as the Post-Autistic Economics Network, the Association for Heterodox Economics, and the International Confederation of Associations for Pluralism in Economics (Dow 2000; Lee 2002; Fullbrook 2003). This dispositional shift is stirring new controversies among heterodox economists about the future of school-of-thought organizations (Rutherford 2000), the meaning and importance of pluralism (Sent 2003; Davis forthcoming; Davidson 2004), and the mission and identity of heterodox economics itself (Ancochea 2004; Dow forthcoming). What exactly do we stand for as heterodox economists? Where do our intellectual priorities lie? Are we paradigm warriors, first and foremost? Or are we pluralists, seeking to promote intellectual tolerance and critical engagement among diverse points of view? These basic questions sit unsettled today, and understandably so, as many heterodox economists remain fiercely committed to the pluralist ideals of liberal inquiry as well as to paradigm-based struggles to establish their own project (or heterodox economics at large) as '*the* truth'.

This chapter seeks to examine, evaluate, and reformulate these contrasting approaches to heterodoxy. The central argument is twofold: (1) leading non-mainstream economists (including some who profess to be pluralists) are still deeply invested in oppositional paradigm building, viewing heterodox economics as primarily a search for demarcation criteria (conceptual, ontological, methodological, or epistemological) that would render heterodox economics distinct from and superior to orthodox (mainstream) economics; and (2) heterodox economists would be better served by a principled pluralism – an 'egalitarian pluralism' that is committed to intellectual diversity as well as to capabilities-enhancing reforms 'in the character of

scientific debate, in the range of contributions in [professional economic] journals, and in the training and hiring of economists' (Hodgson, Mäki and McCloskey 1992) so that 'alternative economic work is not simply tolerated, but … the material and social conditions for its flourishing are met, to the same extent as is currently the case for mainstream economics' (the Cambridge 27, see p. 57). To the latter end, I propose a philosophical framework in which the paradigmist and pluralist impulses of contemporary heterodoxy might be rethought and rejoined, combining Deirdre McCloskey's vision of science as a pluralistic conversation (McCloskey [1985] 1998) with Amartya Sen's capability-centred view of human development (Sen 1999). From a Sen/McCloskey standpoint, the principal goal and tool of economic inquiry is intellectual freedom, defined in a dual Smithian sense: negative freedom from the tyranny of a prescribed Method for the production and evaluation of all economic knowledge, and positive freedom to live a choiceworthy intellectual life.[1] This is an approach to our disciplinary enterprise – grounded in an Aristotelian/liberal vision of economic discourse as a 'civilized conversation among equals' (McCloskey 2001, p. 107) – that all academic economists, especially heterodox economists, can and should embrace.

Radical Paradigmism

The 'my paradigm, right or wrong' strain of heterodox economics emerged in the 1960s (Backhouse 2000, pp. 150–4), a period of unparalleled hubris in US economic theory (Lee 2004; Bernstein 1999; Morgan and Rutherford 1998; Stein 1996). Mainstream microeconomic theorists had been inspired by Debreu's *Theory of Value* (1959) to imagine that 'the model of Walrasian equilibrium was the root structure from which all further work in economics would eventuate' (Weintraub 2002, p. 121). And leading neoclassical Keynesians had declared business cycles passé and were beginning to envision a macroeconomic 'end of history':

> Most economists [now] feel that short-run macroeconomic theory is pretty well in hand. … The basic outlines of the dominant theory have not changed in years. All that is left is the trivial job of filling in the empty boxes, and that will not take more than 50 years of concentrated effort at maximum. (Solow, cited in Hahn and Brechling 1965, p. 146)

Dissenting economists responded in kind. From the early 1970s through the late 1980s, leading Austrian, Marxian, Sraffian, post-Keynesian, social, and institutionalist economists sought to establish their particular mode of economic theorizing as the 'single correct alternative to neoclassical

economics' (King 2002). Philosophically, these radical critics were emboldened by the writings of Thomas Kuhn (1970). Kuhn never wrote a philosophy of social science *per se*; yet his emphasis on theoretical conflicts and paradigm shifts in the natural sciences gave hope and legitimacy to non-mainstream economists, making it possible for them to envision mainstream neoclassicism (*circa* 1975) as a dominant paradigm in crisis, ripe for overthrow by an emerging revolutionary science (Gutting 1980).[2]

The intellectual agendas of these 'revolutionary scientists' shared three common goals: (1) to develop a rigorous critique of mainstream economic theory; (2) to develop a logically complete and compelling alternative theory; and (3) to codify the unique premises and methods of their alternative approach.

As one example, consider the paradigmist efforts of Austrian economists in the 1960s and 1970s. Their goal was to level 'a radical paradigmatic challenge against the core of neoclassicism', on the assumption that 'the possession of a distinct paradigm' was 'necessary for a successful scientific revolution' (Boettke and Prychitko 1994, pp. 6, 13). To this end, Austrian economists invested years of careful work to formulate a consensus framework. The drive for paradigm unity led Austrians to emphasize (among themselves, and occasionally in published work) their shared normative objections to mainstream economics. At the same time, Israel Kirzner and other leading theorists strove to bar these value-laden statements from the formal discourse of Austrian economics. For Kirzner, a commitment to value-free science was essential to the scientific image and integrity of Austrian economics, to demonstrate Austrians' commitment to the pursuit of objective truth and their willingness 'to exercise the restraint necessary to prevent that truth from being dismissed in the eyes of the public as mere propaganda' (Kirzner 1976, p. 87).[3]

Similar stories can be told of institutionalist, social, post-Keynesian, Sraffian, and Marxian economists in the 1970s and 1980s, or even of pan-paradigmatic radical economists like Howard Sherman (*Foundations of Radical Political Economy*, 1987) and Malcolm Sawyer (*The Challenge of Radical Political Economy*, 1989), both of whom sought to fuse multiple strands of radical-left economics into a single oppositional paradigm.[4] All of these projects entailed more than just paradigm building. Leading theorists within each of these groups aspired for their approach to become the new master framework, the new 'general theory', to which other theories would be subsumed as special cases. This is paradigm*ism*: paradigm building infused with the modernist ethos of Solow, Samuelson, and Debreu. Driven to emulate the intellectual imperialism of their mainstream rivals (and in some cases former teachers), many non-mainstream economists became committed to a fundamentalism of sorts. Much as Lucas, Sargent, and other new classical economists in the

1970s and 1980s endeavored to rewrite Keynesian macroeconomics from the ground up by returning to the first principles of individual self-interest maximization and logical-mathematical precision (Lucas 1975, 1976; Sargent and Wallace 1975),[5] many radical left- and right-wing economists returned to the first principles of their dissident traditions in search of alternatives to the mainstream orthodoxy (Kregel 1975; Eichner 1979; Dolan 1976; Steedman 1977; Desai 1979).

Cold War connections

Recent studies in the history and philosophy of postwar economics have highlighted the impact of the Cold War on US economic theory and policy (Bernstein 1999; Mirowski 2002; Mirowski and Sent 2002; Fusfeld 1998; Hodgson 2002). Morgan and Rutherford (1998) and Sent (forthcoming) argue that pluralism was regnant in American economics prior to World War II but then was displaced during the Cold War years by a methodological and epistemological monism – a 'technical turn' wherein 'the possibilities for pluralism persistently waned as the language, form, and tools of economics continued to narrow … [and] objectivity came to be associated with a particular set of methods (mathematics and statistics)' (Sent forthcoming, p. 6).

In the present context, this new scholarship raises an important question: Is there any meaningful link between the Cold War and the paradigmist approach to heterodox economics?

The provocative thesis of Fullbrook (2001) is that heterodox economists have been shaped profoundly (albeit indirectly) by the Cold War, via the rhetoric and reception of Kuhn's landmark text *The Structure of Scientific Revolutions* (1962; 1970). Fullbrook's view coincides with the arguments of Fuller (2000), a philosopher of science who urges scholars to re-read Kuhn's text 'as an exemplary document of the Cold War era' (Fuller 2000, p. 5). Fuller details the personal and intellectual history of *The Structure of Scientific Revolutions*, particularly the role of James Bryant Conant, the man to whom Kuhn dedicated the book. Conant was president of Harvard University from 1933 to 1953. During World War Two, Conant also served as director of the National Defense Research Committee (which supervised the construction of the first atomic bomb) and later he served as chairman of the anti-Communist Committee on the Present Danger. According to Fuller, Conant helped Kuhn to secure his first teaching position and introduced him to the historical study of science. Fuller claims that 'Kuhn simply took Conant's [Cold War] politics of science as uncontroversial – indeed, as a taken-for-granted worldview. *Structure* does not so much transcend the Cold War mentality as express it in a more abstract, and hence more portable, form' (Fuller 2000, p. 6). For example, Fuller points to Kuhn's 'incommensurability

thesis' as a 'Cold War worldview' (Fuller 2000, p. 175) in which competing paradigms are cast as (ideo)logically opposed systems of thought.

Fullbrook (2001) extends Fuller's argument into the realm of economic theory, underscoring the impact of the Cold War on Kuhn's arguments and on pop-academic images of science that emerged in the wake of Kuhn's famous book. Most provocatively, Fullbrook cites Kuhn's explicit analogy between scientific paradigms and rival political systems: 'Like the choice between competing political institutions, the choice between competing paradigms proves to be a choice between incompatible modes of community life' (Kuhn 1962, p. 94, cited in Fullbrook 2001).

> Kuhn's book methodically transposes the Cold War narrative onto the competing-theories narrative of science. This transposition extends even to his vocabulary, with a heavy use of Cold-War buzz words and expressions like 'subversive', 'polarization', 'crisis' and 'crisis provoking', 'techniques of mass persuasion', 'allegiance', 'commitment', 'conversions', total 'destruction' and 'total victory', and of course 'revolution'. (Fullbrook 2001)

In Fullbrook's view, heterodox economics today is still laden with Cold War conceptions of science, leading many non-mainstream thinkers to erect their own separate citadels rather than engaging in critical, pluralistic exchanges as 'real scientists' would do. 'Kuhn's narrative makes the defense of one's paradigm community, through the elimination or marginalization of rival ones, the scientist's overriding goal.' 'It is this emotionally-charged us-or-them, all-or-nothing mentality which Kuhn's book seems to legitimate as the ethos of science' (Fullbrook 2001).

Emerging objections to paradigmism

The paradigmist quest for a 'single correct alternative to neoclassical economics' (King 2002) has inspired tremendous intellectual energy and progress among dissenting economists, making it possible for numerous individuals and schools of thought to survive under adverse professional circumstances (Lee 2004). It has enabled heterodox economists to (re)claim valuable space within academic economics by publishing articles, teaching graduate and undergraduate students, acquiring senior faculty positions, organizing conferences, creating new scholarly journals, and so on. In so doing, the paradigmist movement has afforded later generations of economists the opportunity to choose among a wider range of intellectual options than otherwise would have been available in a professional culture dominated by the assimilationist dogma that (to paraphrase Milton Friedman) 'There is no heterodox economics – just good economics and bad economics.'[6]

These virtues notwithstanding, I am inclined to agree with Fullbrook that

the 'paradigm warfare' approach has become increasingly anachronistic and self-defeating for heterodox economists, on several counts.

First, it presupposes a monolithic enemy ('neoclassical economics') that arguably has ceased to exist.[7] As Davis notes, 'mainstream economics is no longer "neoclassical" in the way that many of us are accustomed to thinking of it' (Davis forthcoming, p. 6; see also Colander 2000, pp. 129–30 and Dow 2000, p. 159). From this standpoint, many heterodox criticisms of neoclassicism are 'off the mark' today 'because mainstream economic thinking has changed. ... Economics is moving away from a strict adherence to the holy trinity – rationality, selfishness, and equilibrium – to a more eclectic position of purposeful behavior, enlightened self-interest, and sustainability' (Colander, Holt and Rosser 2004, pp. 1–2).

Second, radical paradigmism encourages an obsessive concern with the uniqueness and separateness of one's own theoretical approach *vis-à-vis* others. It fuels a bunker mentality of Us versus Them, and an autarkic tendency to see one's own paradigm community as a self-sufficient intellectual universe. As Malcolm Rutherford observes in regard to AFEE (the 'old institutionalist' Association for Evolutionary Economics):

> [H]eterodox groups often think that they know the truth. This can make such groups (and particularly those groups that have been under sustained attack and that feel themselves embattled) very inward looking, defensive, and not very open to new ideas. A mentality of defending the true faith can come to dominate, and, in my view this has been a serious problem in AFEE and in Marxian and post Keynesian groups. (Rutherford 2000, p. 186)

This isolationist tendency creates particular difficulty for young heterodox economists inasmuch as it encourages them to disengage from other traditions of thought, and perhaps from economics itself, inasmuch as they are taught to see little point (other than careerism) in trying to connect their ideas to the larger economic conversation – a costly decision, as '[e]ven for self-confessed heterodox economists, this rugged aspect of the landscape carries with it in many cases an unwanted and unnecessary sense of isolation' (Potts 2000, p. x).

Third, the Kuhnian notion of a single dominant paradigm (and the correlative notion of a single dominant rival) encourages an all-or-nothing view of intellectual change: it is never enough for radical paradigmists to oppose the prevailing orthodoxy – they must provide a complete alternative. Much as revolutionary Marxists have long borne the burden of having to devise a socialist or communist utopia that would preserve all of capitalism's virtues but none of its problems (Gibson-Graham 1996), heterodox economists of all persuasions continue to saddle themselves with the burden of offering a new master framework that would fully supersede neoclassicism. Besides

inviting feelings of resignation, burnout, and despair as we 'wait for the revolution' (Gibson-Graham 1996, pp. 256, 259), this perspective fuels unduly intense rivalries over which insurgent paradigm is best equipped to do battle with 'the enemy,' inhibiting exchange and collaboration among intellectual leaders whose creative energies might otherwise be joined to larger ends.[8]

Fourth, a paradigmist approach undercuts heterodox economists' commitments to pluralism. Every heterodox economist embraces pluralism to a degree. But for a radical Kuhnian, pluralism can never be more than a secondary priority, something to be honoured only insofar as it does not conflict with the first-order imperatives of scientific or ideological victory.

The Pluralist Turn

The recent trajectories of heterodox economics show a marked shift – philosophically, analytically, organizationally, and culturally – from school-of-thought paradigmism to pluralism,[9] that is, to a view of knowledge in which there is no possibility, even in principle, that 'any school could possess final or total solutions' (Fullbrook 2003, pp. 8–9). Pluralism has increasingly become a normative commitment among heterodox economists, a 'positive valuing of a diversity of views in the minimal sense that one who is so committed would not want to reduce the number of available narratives or views' (Hargreaves-Heap 2001, p. 356).[10] Heinz Kurz and Neri Salvadori, two veteran Sraffian economists, offer a revealing glimpse of this pluralist ethic:

> [T]o seek dominance for one theory over all the others with the possible result that all the rival theories are extinguished amounts to advocating scientific regress. To paraphrase Voltaire: in a subject as difficult as economics, a state of doubt may not be very comfortable, but a state of certainty would be ridiculous. (Kurz and Salvadori 2000, p. 237)

The most rigorous justifications offered for this heterodox pluralism to date have come from 'open-system realists' (Lawson 1997, 2003; Dow 1997, 2000, 2004a, 2004b, and forthcoming; Fleetwood 1999; Fullbrook 2001; Herrmann-Pillath 2001). These thinkers envision the object of economic inquiry as an open-ended network of institutions and processes so complex and heterogeneous that no single idiom can adequately represent it, and any attempt to do so will only diminish our knowledge of it. On these ontological and epistemological premises, open-system realists '[reject] the ideas of theoretical monism and theoretical universalism' (Herrmann-Pillath 2001, p. 91) and insist that a plurality of theories and methods is scientifically

essential. In their view, the best way forward for non-mainstream economists is to abandon the paradigmist dream of a unified general theory and to cultivate instead a pluralist (heterodox) community of inquiry, united by a shared commitment to open-system realism.

Open-system realism figures prominently in the wave of recent petitions from young economists in France, the UK, the USA, and Italy that sparked the formation of the international Post-Autistic Economics (PAE) movement (Fullbrook 2003).[11] This student-led movement is calling for 'a total overhaul of economics and economics teaching' based on open-system alternatives to the closed-system (deductivist, rationalist, scientistic) conceptions of social science that still pervade mainstream economics. The PAE movement is far from monolithic on these issues. Yet their campaign is suffused with open-system ontologies, as in the French students' original call for 'a pluralism of approaches in economics [that is] adapted to the complexity of the objects and to the uncertainty surrounding most of the big questions in economics (unemployment, inequalities, the place of financial markets, the advantages and disadvantages of free trade, globalization, economic development, etc.)' (cited in Fullbrook 2003, p. 13).

These student petitioners are among a growing number of non-mainstream economists who are turning to open-system pluralism as a philosophical platform for the future of heterodox economics. Lawson argues that such an ontological reorientation would help heterodoxy to become a more vibrant scientific community, 'a pluralistic forum where explicitly prosecuted ontology and critical reflection can take their place amongst all of the conceivable components of economics as social theorizing' (Lawson 2003, p. 27). He believes that various schools of heterodox economics 'can benefit at this juncture from making their ontological theorizing or commitments more explicit, systematic, and sustained, from reformulating themselves explicitly as contributions to what I am calling realist social theorizing' (Lawson 2003, p. xxiii). Lawson sees this as the best way to overcome the isolationist tendencies of paradigmism while strengthening the social-scientific contributions of heterodox economics at large.

Dow offers a parallel set of arguments, paying particular attention to the role of paradigms within a pluralist, open-system economics (Dow 2004a, 2004b, and forthcoming). In this regard, open-system realism plays a critical role, as a 'shared [ontological] vision' that both enables and constrains the play of pluralistic difference among and within paradigm communities. Dow describes this as a 'qualified pluralism': a pluralism that promotes difference *and* unity by giving individuals (or individual schools of thought) 'the capacity to follow different routes simultaneously – unified by a common goal' (Dow 2000, p. 166).[12]

Programmatically, Dow, Fullbrook and others see open-system realism as

an invaluable weapon in heterodox economists' ongoing war of position with mainstream economics. Inasmuch as it helps partisans of various heterodox paradigms to see themselves as part of a larger community of overlapping and complementary approaches, open-system realism could serve as a catalyst for solidarity and collaboration among 'non-orthodox schools of thought such as post Keynesian economics, institutionalist economics, neo-Austrian economics, behavioral economics, social economics, feminist economics, and Marxian/radical economics, all of which employ some kind of open-system approach' (Dow 2000, p. 158; also Fullbrook 2003, pp. 2–3).[13] Dow and Fullbrook are hopeful that such a broad-based heterodoxy could eventually become large and powerful enough to challenge the mainstream. In addition, open-system realism also provides a compelling way to demonstrate the superiority of heterodox economics *vis-à-vis* orthodox economics by highlighting the latter's reliance upon indefensible closed-system assumptions and methods. In this way, open-system realist critics hope to 'ensure that the [gap] between orthodox theory and reality is recognized widely enough to support a scientific revolution' (Dow forthcoming, p. 4).

Emerging Objections to the Pluralist Turn

The recent ascendance of pluralism among heterodox economists has elicited a series of pointed criticisms. Esther-Mirjam Sent doubts that 'heterodox economists are serious about their advocacy of pluralism' (Sent 2003), based on what she sees as a heterodox tendency to treat mainstream economic theories in a non-pluralistic manner. 'Though their advocacy of pluralism may be couched in metaphysical or epistemological terms, it is primarily inspired by efforts to achieve professional power and dominance' (Sent forthcoming, p. 20). Sent concludes that heterodox economists either do not understand the requirements for a philosophically consistent pluralism or are unwilling to fulfil them. She challenges heterodox economists to practise what they preach: to pay more attention to the types of pluralism they advocate; to avoid inadvertently 'sliding into monism' (for example, embracing a pluralism whose rationale is transitory, such as a temporary state of incomplete or uncertain knowledge);[14] to avoid disingenuous invocations of pluralism ('[employing] appeals to pluralism strategically, in an effort to achieve monism'); to confront the charge that pluralism leads inevitably to an 'anything goes' epistemology; and to 'work to ensure that the material and social conditions for the flourishing of pluralism are met' (Sent 2003).

Paul Davidson levels a more fundamental objection, questioning the value of pluralism itself. Davidson flatly rejects the Fullbrook/Dow assertion that a

widespread commitment to pluralism can help non-mainstream economists to effect significant changes in 'the development of their discipline as taught in major universities and economic journals' (Davidson 2004). Davidson regards this sort of pluralist optimism as dangerously misguided, and likely to reinforce the marginal status of non-mainstream ideas within the economics profession.

> Encouraging pluralism in economics without a common general theory foundation merely encourages heterodox economists to erect a modern Tower of Babel, thereby making it easier for mainstream economists to ignore the resulting incomprehensible babble coming from this heterodox structure. (Davidson 2004)

> [T]he mainstream sees heterodox [economists] as … people who do not deserve to be heard in proper academic circles because they clearly possess fundamental logical inconsistencies in their approaches. Until they can get their house in order, why pay any attention? (Davidson 2002)

With reference to the recent removal of heterodox faculty from the economics PhD programme at the University of Notre Dame on the grounds that their scholarly work 'did not meet the minimum standards of quality' (Ancochea 2004; Donovan 2004; McCloskey 2003), Davidson contends that the only way non-mainstream economists can improve their professional standing is by developing a superior paradigm, 'a single axiomatic foundation that provides the most general theory case' (Davidson 2002). 'Until heterodox economists unite behind a single "general theory", they are going to be losers' (Davidson 2003a).

> You cannot beat a rigid orthodoxy who despise non-pure bred Aryans (heterodox economists) with a 'let's all share the tent guys and gals' philosophy. As the Allies found out when dealing with Hitler, it takes an 'unconditional surrender' approach and stronger [in this case, stronger logical] forces to win what – whether you like it or not – the other side has declared to be a war of annihilation. (Davidson 2003b)

Finally, John Davis (forthcoming) offers a third major line of critique. Davis joins Sent in applauding the ideal of a more pluralist economics. Of the recent resurgence of pluralism among heterodox and mainstream economists, Davis writes approvingly: 'We appear to have entered upon a new period of pluralism in economics, structurally speaking perhaps not unlike the past interwar pluralism' (Davis 2002, p. 149). At the same time, Davis is wary of the growing tendency to *identify* heterodox economics with pluralism, in part because the philosophical foundations of pluralism derive from the same liberal arts traditions that underlie mainstream economics. Davis therefore regards heterodox arguments for pluralism as well-intentioned but philosophically ad hoc:

> Of course it is all fair and good for [heterodox economists] to press on a non-

> theoretical, purely practical basis for openness, non-discrimination, and for a 'free market' in ideas … These are ideals that ought to be defended across all of the humanities and sciences … But this sort of program does not stem directly from the particular content of heterodox economics. It stems from a commitment to social values of long standing that operate across the humanities and sciences and indeed in society generally. Only, it seems, were these ideals and values to become shared across heterodoxy and the mainstream, would there then be hope for a wider pluralism in economics. (Davis forthcoming, p. 17)

Davis concludes that heterodox economists should drop (or de-emphasize) their 'politics of pluralism' in favour of a 'politics of ontology' – a focused effort to exploit the ontological weaknesses of the 'rationality – individualism – equilibrium' nexus of mainstream economics (particularly the inadequacies of its 'now arguably defunct … atomistic individual conception' [Davis forthcoming, p. 18; also Davis 2003]) and the corresponding strengths of the 'institutions–history–social structure' nexus of heterodox economics. In this regard, Davis is willing to excuse, indeed to encourage, the non-pluralist ('intolerant') treatment of mainstream economic theories inasmuch as it is rooted in a legitimate ontological critique.

Which Way Forward?

Heterodox economics as we know it today emerged alongside a dominant neoclassicism in the high modernist, Cold War environment of the 1960s. Dissenters aspired to defeat an arrogant orthodoxy at its own game. Paul Davidson and other paradigm warriors are still trying. Others have moved in more pluralistic directions, seeking to build broader communities of dissent. Yet in many cases these pluralists are still paradigmers too, holding tightly to an oppositional conception of heterodox economics. Their main philosophical strategy is to formulate rules – demarcation criteria – whereby economic science is (re)defined to include Us but not Them. Davis, for example, agrees with Davidson that a principal aim of non-mainstream economics should be the establishment of a logical or ontological fault line between heterodox and orthodox approaches so that 'heterodoxy … is differentiated and also identified as a single discourse in terms of its … differences from mainstream economics' (Davis forthcoming, p. 23; also Davis 1999). Even the open-system pluralisms of Dow and Lawson carry residual traces of this paradigmist vision, insisting that heterodox economics define itself as the Other of orthodox economics. This is Cold War paradigmism in a different guise but still the same oppositional project, with the same truncated pluralism: offering intellectual openness and respect to persons and arguments within our own paradigm communities but not to

outsiders. To define heterodox economics in this way is to warrant the charge that heterodox economics has no positive identity, that it defines itself only 'in terms of what it is not, rather than what it is' (Colander, Holt and Rosser 2004, p. 11). This puts us in the reactive position of 'permitting the mainstream to set the agenda for heterodox economics … to define its structure and content' (King 2004). It also demonstrates that our professed commitments to pluralism are fundamentally ill-conceived, insincere, or both.

Which way forward, then, for heterodox economics?

Towards an Egalitarian Pluralist Economics

I would like to propose a different approach to these issues, one that seeks to reconcile the competing priorities of paradigmism and pluralism via a rethinking of science, grounding the case for economic pluralism not in an open-system ontology but in the ethics and epistemology of classical liberalism. The approach I envision would combine Sen's capabilities approach to human development (modified to focus on intellectual capabilities) with McCloskey's notion of economics as a pluralistic conversation (Sen 1999; McCloskey 1998, 2001). Among other things, this approach offers a compelling rationale for school-of-thought paradigms as tools for enhancing the intellectual capabilities of economists and their stakeholders, including our graduate and undergraduate students. This, I think, is the kind of pluralism envisioned by the student petitioners: an epistemologically consistent pluralism that seeks to enhance the substantive intellectual freedoms of economics scholars, teachers and students alike (Fullbrook 2003; McIntyre 2003).

Philosophical foundations: McCloskey and Sen

McCloskey's contributions to heterodox economics are often overlooked, due to her strong self-identification as an 'old Chicago' economist. Yet her trenchant criticisms of modernist methodologies and epistemologies – the official grounds on which mainstream economists ritually dismiss non-mainstream work as 'not serious' or 'not economics' – make McCloskey a natural ally of the heterodox economics movement (Garnett 2004). Her writings deserve attention in the present discussion because they offer the strongest arguments to date for the scientific virtues of a pluralist economics.

In barest essentials, McCloskey's pluralism begins with a Scottish Enlightenment conception of science as a marketplace of ideas, an 'economy of intellect' (McCloskey 1998, p. 28). Like Hayek and other classical liberals, she sees science as a marketlike conversation in which the 'interaction of

individuals, [each] possessing different knowledge and different views … constitutes the life of thought' (Hayek 1944, p. 165).[15] McCloskey's unique contribution is her rigorous unpacking of the assumptions, logic, and implications of this free-market philosophy via her rhetorical theory of intellectual exchange. In McCloskey's view, the best way to facilitate scholarly exchange is not by attempting to enforce a uniform methodology or ontology but by cultivating a set of liberal arts virtues – virtues she calls 'rhetoric' but might also be called 'pluralism'. Just as Adam Smith argues that a nation's wealth depends on the extent of its markets, McCloskey argues that an academic community's knowledge depends on the willingness and ability of its members to listen and speak to one another, that is, 'our ability to engage in continuous conversation, testing one another, discovering our hidden presuppositions, changing our minds because we have listened to the voices of our fellows' (A. O. Rorty 1983, cited in McCloskey 1998, p. 163).

McCloskey stresses the intellectual responsibilities entailed by a serious pluralism. As individual knowledge producers, we are free to use whatever tools are available in our efforts to make intelligent contributions to scholarly conversations. At the same time, one's own enjoyment of this freedom entails a pluralist duty to foster this same liberty in the academic lives of one's colleagues, not just by tolerating their ideas but by actually reading, listening, engaging, and learning in a spirit of intellectual humility, refusing to discriminate against propositions on the basis of intellectual pedigree. Again McCloskey:

> What distinguishes good from bad in learned discourse … is not the adoption of a particular methodology, but the earnest and intelligent attempt to contribute to a conversation … You can tell whether [an argument] is persuasive only by thinking about it and talking about it with other thoughtful people. Not all regression analyses are more persuasive than all moral arguments; not all controlled experiments are more persuasive than all introspections. (McCloskey 1998, pp. 162, 177)

McCloskey believes that academic pluralism, when practised in earnest, allows our disciplines and subdisciplines to progress in terms of 'understanding, self-understanding, and mutual understanding or agreement' (Madison 1994, p. 206) despite the absence of a common ontology or analytic framework.[16]

McCloskey's pluralism thus promotes critical engagement and accountability, not 'anything goes'. This is a steady refrain in McCloskey's writings. She does not assume that all knowledge claims are (or should be treated as) equally valid. The only universal precept is the 'demand that we persuade each other' (McCloskey 1994, p. 310). Over time, she argues, this ethical/scientific requirement gives rise to intellectual standards: 'communally accepted criteria as to what counts as a rational argument' (Madison 1994,

p. 205). These standards emerge not from the armchairs of economic philosophers but from 'open, reasonable, fair, patient, *sprachethiklich* conversation' (McCloskey 1994, p. 304) and 'informed judgment, guided by broad and evolving principles of assessment' (Hausman and McPherson 1988, p. 6).

McCloskey's pluralism is mostly silent, however, on the controversial question of how (or whether) to reform the institutional hierarchies within academic economics to create a more competitive marketplace of ideas:

> The openness of [rhetorical pluralism] gives voice to minority opinions. To this extent [rhetorical pluralism] is hostile to the mainstream … But [rhetorical pluralism] is not intrinsically hostile to the mainstream. [Rhetorical pluralism] can be used to force the dominant groups to face up to institutionalism or Marxism or feminism or Austrianism, as they should. But nothing inside [rhetorical pluralism] itself implies one or the other view. (McCloskey 1994, p. 394)

McCloskey urges heterodox groups to challenge the arrogance of the mainstream head on, not by erecting rigid paradigm barriers of their own but by embracing the ethos of intellectual free trade, 'a catholic rhetoric that encourages neoclassicals, Marxists, institutionalists, Austrians, and the other students of mankind in the ordinary business of life to gain more persuasive knowledge' (1994, p. 178). On occasion, McCloskey has been critical of the labour market discrimination and other exclusionary practices that unjustly limit the professional opportunities of non-mainstream economists (Klamer and McCloskey 1989; McCloskey 2003). In the main, however, she believes that the current structure of the discipline satisfies the institutional requirements for free speech, and that a greater rhetorical self-awareness among economists will suffice to actualize her normative vision of economics as a 'civilized conversation among equals' (McCloskey 2001, p. 107).

Precisely here, on the question of how to promote substantive equality in the intellectual marketplace, Sen's capabilities approach becomes a vital complement to McCloskey's pluralism.[17] Sen's work speaks mostly to human and economic rather than intellectual development. But the central assumptions of his argument – 'the central (intrinsic) value of freedom itself' and the notion that wealth is valuable not in itself but as a 'general-purpose means for having more freedom to lead the kind of lives we have reason to value' (Sen 1999, pp. 28, 14) – are easily extended to the intellectual realm. The key premises of an academic/scientific capabilities approach would be threefold:

1. intellectual freedom is 'the primary end as well as the principal means' of intellectual progress;
2. knowledge is not valuable in itself but as a 'general-purpose means for having more freedom to lead the kind of lives we have reason to value'; and

3. intellectual development consists of 'the removal of various types of unfreedoms that leave people with little choice and little opportunity of exercising their reasoned agency'.

From Sen's 'development as freedom' approach, then, we derive a notion of 'learning as freedom' in which 'the removal of substantial unfreedoms' is both intrinsically and instrumentally linked to intellectual development, as an important part of what intellectual development *is* and, equally, an important part of what creates and sustains intellectual development (Sen 1999, p. xii).

A Sen-based pluralism would therefore take us beyond the 'marketplace of ideas' to the constellation of institutional circumstances that affect a person's ability to exercise his or her academic freedoms. Indeed, Sen's approach suggests a broad normative criterion by which the efficacy of any academic discipline should be judged: namely, the effective freedom of its members to achieve such vital ends as literacy (the ability to read, think, and speak effectively within their discipline), the ability to choose and move freely among alternative theoretical traditions, or the ability to participate with dignity in the public life of their profession.[18] Do prevailing institutional arrangements enable all members of the discipline to achieve these ends – hence to lead good intellectual lives – if they so choose? If not, then the disciplinary community (broadly defined to include the schools, colleges, and universities within which economics departments reside) has an obligation, in the name of science and academic freedom, to alter its educational, scholarly, and professional norms to enhance the capability of individuals to achieve these essential intellectual functions (Burczak forthcoming).[19]

Of course, the ultimate purpose of academic economics is not just to enhance the welfare of economists but to promote human betterment in a larger sense (Hutchison 1992; Klein 1999). It is beyond the scope of this chapter to consider the degree to which the goals of academic economists align with those of their various stakeholders, hence the degree to which 'what's good for economists is good for society'. My working assumption, however, is that the social value of economic scholarship and teaching would increase substantially if capabilities enhancements such as those proposed by the Post-Autistic Economics movement were to be enacted. This coincides with Sen's argument that increases in substantive freedom typically generate indirect (spillover) benefits for the communities in which the individual participates. 'Expanding the freedoms we have reason to value not only makes our lives richer and more unfettered, but also allows us to be fuller social persons, exercising our own volitions and interacting with – and influencing – the world in which we live' (Sen 1999, pp. 14–15).

To summarize, the goal of economic science from a Sen/McCloskey perspective is to increase the intellectual capabilities of economists and the

various stakeholders who rely on economic scholarship and teaching as important resources (students, policymakers, citizens, civic leaders, business leaders, and so on). Further, the achievement of this objective requires no common ontological, methodological, or analytical foundation. The key, rather, is pluralism, both in the actions and attitudes of individual economists and in the norms and practices of our academic communities. The latter requires an active commitment to economic pluralism on the part of professional associations, academic departments, and university administrators, as well as the non-academic institutions from which economists receive financial support (governments, foundations, and think tanks) 'to ensure that the material and social conditions for the flourishing of pluralism are met'.

Consequences: rethinking pluralism, paradigms, and economic science

A Sen/McCloskey perspective makes it possible to recast the nature and importance of paradigms and pluralism in economics. Most orthodox and heterodox economists still subscribe to modernist, Cold War epistemologies in which 'science' is primarily a tool of demarcation and exclusion: a way of separating valid from invalid ways of knowing (McCloskey 1994, pp. 55–70; Fullbrook 2001). A Sen/McCloskey approach, in contrast, highlights the liberal face of science. It shifts the goal of science from intellectual victory (Us over Them) to the expansion of intellectual freedom for Us *and* Them, namely, our fellow economists, students, and other stakeholders. From this standpoint, scientific progress is about advancing the conversations of our various learning communities through critical engagement among diverse ways of thinking. This leads to a different view of how paradigms and pluralism each contribute to learning and discovery in the continuing conversations – the science – of economics.

With regard to pluralism, a Sen/McCloskey approach helps to clarify the conceptual meaning and requirements of a pluralist economics. As Backhouse observes, academic pluralism has both individual and social aspects (Backhouse 2001). It requires methodological awareness and tolerance on the part of individual scholars as well as a set of institutional conditions embodied in 'the organization of the academic community, how individuals operate within it, [and] its relationships to other communities and to the society at large' (Backhouse 2001, p. 166).

A Sen/McCloskey approach would also help to demonstrate the scientific importance of pluralism by highlighting its central role in intellectual development. As Sen would say, pluralism (*qua* intellectual freedom) is an intrinsic part of what intellectual development is. One mark of progress in a scientific community, he would argue, is the degree to which its members understand their rights and duties as members of a larger discourse com-

munity. The widespread acceptance of these pluralistic norms would allow individuals to enjoy more intellectual freedom and, in Hayek's words, to 'work with people whose moral values differ from our own … [in order to permit] the coexistence of different sets of values … [and] to build a peaceful society with a minimum of force' (Hayek 1960, p. 398).[20] Pluralism also fuels scientific creativity over time, both by preserving methodological diversity (Samuels 1997; Dow 2001)[21] and by helping to curb the excesses of paradigmism – the recurring tendency of paradigm communities to become so enamoured of their particular theory or methodology that they begin to see it as the new general theory, for example, the current enthusiasm for evolutionary frameworks that looks at the economy as a 'complex adaptive system' (Potts 2000; Colander, Holt and Rosser 2004). As Fullbrook reminds us:

> Accepting pluralism on epistemological grounds requires from everyone taking part a sacrifice. One must surrender the dream … which I suspect has infected each of us from time to time, that of believing that our particular approach to or tradition of economics is or could become THE economic truth. Once people sacrifice this conceit and put their heads and good-wills together, many things become possible. (Fullbrook 2005)

With regard to paradigms, a Sen/McCloskey view of science casts new light on the value of paradigm communities as well, especially those outside the mainstream. It sees paradigm communities as generators of intellectual capabilities that otherwise would not be available to certain individuals or groups. Further, it sees the empowerment of these voices as a vital prerequisite for a scientifically productive pluralism. A group that is able to establish an identity as a distinct school of thought is afforded a place to stand, a right to exist, a means to appear without shame in the public space of academic conversation. This is quite distinct from the paradigmist pursuit of scientific hegemony. Even McCloskey, despite her ambivalent view of 'schools,' is willing to grant that paradigm communities can be effective incubators of pluralist sensibilities and intellectual growth:

> How do you think schools [of thought] form in economics? A group talks intensively to each other, respectfully … They allow each to influence the other. They stop sneering and start listening. I've seen it happen. … Such a community comes to have few disagreements, if the talking goes on long enough. (Klamer and McCloskey 1989)

Beyond placing a positive value on paradigm communities, a Sen/McCloskey perspective would also challenge the common assumption that paradigms are or should be single-minded schools. Paradigm communities need not possess a unified framework or worldview in order to enhance the intellectual capabilities of their members. More likely, the converse is true.

To best enhance their intellectual capabilities, it may be best for paradigm groups *not* to be so single-minded. Sent argues, for example, that post-World War Two mainstream economics has derived considerable vitality and growth from its inability to forge a unified theoretical core (Sent forthcoming, p. 6). I believe this has been the experience of many heterodox school-of-thought organizations as well. According to Yngve Ramstad, this is precisely the story of the AFEE/AFIT institutionalist community:

> '[I]nstitutional economics' is actually nothing more than a summary term for analysis that originates on the same side of several 'great divides' … Thus [it] remains today what it has always been, a friendly alliance between those who proceed to build concepts, theories, and models from the same side of several or all of [these] 'divides'. (Ramstad 1989, p. 771)

> [W]e should put an end to our defensive preoccupation with articulating a precise statement of the institutionalist paradigm [and] … face squarely the fact that we are a catholic movement comprised of multifarious groups with some fundamental disagreements. (Ramstad 1995, p. 1004)

Inasmuch as it serves to decentre our paradigmist goals and identities, a Sen/McCloskey approach could help to breathe new life into heterodox paradigm communities. Many school-of-thought groups are currently undergoing difficult generational transitions, seeking to renew and reinvigorate themselves without losing their core identities (Rutherford 2000, p. 186). Rutherford claims that these groups must make greater commitments to pluralism if they wish to remain 'vital, interesting intellectual forums'. Institutionalists, for example, must be 'open to work that is broadly consistent with institutionalist themes, even if it derives from sources other than the recognized founders of American institutionalist economics' (Rutherford 2000, p, 187). Hoksbergen agrees, arguing that institutionalism will become 'a stronger, richer, and more meaningful tradition' only if its members become less inclined to see their group as 'a rival and potentially a superseding competitor to neoclassical economics as it has often done in the past' (Hoksbergen 1994, p. 707).

Across the heterodox landscape today, there are signs of pluralist rethinking within and across many school-of-thought communities. An exemplary statement of this pluralist tendency appears in the editor's introduction to the new *Journal of Institutional Economics*:

> In recent years there has been a welcome and growing dialogue of ideas from the old and new traditions of institutionalism. … These developments open up new grounds for a fruitful and exciting conversation between old and new institutionalism [as well as] several other important schools of thought – including evolutionary economics and constitutional political economy – that have similarly focused on the nature and role of institutions. … The *Journal of Institutional*

> *Economics* has been established in the belief that dialogue and diversity are important engines in the evolution of scientific inquiry. (Hodgson 2005, pp. i–ii)

Hodgson's institutionalists are not alone in their pluralist attitudes. Similar signs of internal diversity and external openness are visible in the paradigm communities of Marxian economists (Amariglio et al. 1996), Austrian economists (Boettke and Prychitko 1994), social economists (Davis 1994), and many other branches of economic heterodoxy.

Finally, a Sen/McCloskey pluralism offers a fruitful new angle on the old dilemma of how to embrace pluralism without sacrificing scholarly standards. A common fear about pluralism, often expressed as a critique of post-modernism, is that it threatens to erode scholarly standards by promoting the relativist notion that 'standards are relative to specific discourse communities, and … work should be appraised [only] from within the relevant community' (Backhouse 1998, p. 144). But pluralism need not lead to such intellectual autarky. Pluralism assumes only that 'after weeding out all of the many concepts and philosophical systems that can be rejected on one or another ground, a plurality of acceptable philosophies is likely to remain' (Robert Nozick, cited in Nelson 1991, p. 268; also Seabright 1993, p. 393). The difficult question is how, in practice, to combine intellectual standards and openness in light of the paradigmatic disagreements that exist throughout economics and its subfields, that is, how to reject ideas that fail to meet certain basic academic standards while recognizing that 'a plurality of acceptable philosophies is likely to remain' even when these standards are vigilantly enforced.

These problems emerge concretely in the process of peer review. Reflecting on their interdisciplinary co-editorship of *Economics and Philosophy* in the late 1980s and early 1990s, Hausman and McPherson (1988) confess to finding *intra*disciplinary paradigm differences much more difficult to navigate than differences across discipline. 'The sharpest divergence in terms of standards has lain … between different schools of economists. … [W]e have generally shied away from refereeing across schools, precisely because the differences are so sharp and so predictable and the results so generally unhelpful' (Hausman and McPherson 1988, p. 3). These problems would not be so severe if there were some semblance of transparadigm agreement about methodological and epistemological norms. Yet each of these areas is a contentious conversation unto itself, with no consensus rules or guidelines. Hence there is no easy way to resolve these evaluative dilemmas. Backhouse recounts similar experiences from his editorship at the *Economic Journal* (Backhouse 1994). In addressing the issue of standards, he says, we must recognize the reality of 'paradigm bias'. At the same time, one cannot resolve this problem by adopting balkanized editorial procedures (for example, Marxian articles can only be read by Marxists). It is better to mix and match,

though then one runs the risk of being criticized (and understandably so) for stacking the deck against authors whose submissions were rejected by reviewers from rival schools of thought.

The underlying problem here is that today's academic economists often lack the pluralist capability to evaluate work beyond their own narrow fields of expertise. Their economics education has not given them sufficient knowledge or intellectual humility to read effectively across methodological boundaries. This is the very conclusion reached by Backhouse. He suggests the need for capabilities-enhancing reforms in economic education in order to produce more scholars who are capable of fair-minded peer review. The ideal reviewer, he writes, would be a scholar 'whose position is different from that of the . . . author, but who [is] prepared to treat [the work] sympathetically as well as applying high critical standards' (Backhouse 1994, p. 116).

A Sen/McCloskey pluralism would help to highlight and address these deficiencies in the intellectual capabilities of PhD economists. More generally, it would aim to address the problem of relativism through the promotion of critical thinking and competition among diverse points of view. Inasmuch as relativism entails a lack of effective intellectual competition – in the mind of a single scholar or in a community of scholars – it can be addressed by creating a more capabilities-rich academic environment in which individuals have access to ample supplies of intellectual options, and are able to exercise reasoned choice among these options. The latter requires (or rather, is) critical thinking: a commitment to the empathetic yet sceptical treatment of all perspectives, including one's own (Paul and Elder 2001, pp. 1–4). A Sen/McCloskey approach would support the expansion of these essential capabilities among undergraduate and graduate economics students – tomorrow's economists – via curricular reforms such as those proposed by Hodgson (2002) to enhance social science literacy by '[restoring] the possibility of taking a more general view, while retaining specialist expertise'. 'Just as the requirement of mathematics is now virtually universal, so too should be some philosophy, and relevant parts of the history of ideas' (Hodgson 2002, p. 132).

Towards a Better (and More Heterodox) Economics

The capabilities-oriented pluralism outlined above suggests one way for heterodox economists to exercise leadership in a movement towards a genuinely pluralistic, multi-perspectival (*hetero-dox*) economics. Rather than bearing the heavy burden of an oppositional, anti-mainstream stance, heterodox advocates of a Sen/McCloskey approach would be empowered to operate from the positive, intellectually open position of liberal political

economists – committed, perhaps, to their own paradigm-based projects, but also to a pluralist vision of economics as a 'civilized conversation among equals' and to ongoing reforms in economics education and scholarship to better approximate this ideal.[22] In addition, this egalitarian/libertarian philosophy would place us on the very ground floor of modern economics, arm in arm with Smith, Marx, Keynes and other economic champions of human freedom who argue that entrenched inequalities must be ameliorated in order to reap the full benefits of a free society (in the present case, a society of scholars).[23] Such an approach would give heterodox economists a stronger foothold within our disciplinary community.

Of the challenges and opportunities before us, none is greater than the need to provide better learning environments for future generations of economics thinkers. I agree with Craufurd Goodwin that teaching is the most important social role of professional economists today (Goodwin 2000). Yet many economics educators and curricula, graduate and undergraduate, are unprepared to fulfill this vital role. 'The growth of formalization and mathematization and the high degree of uniformity in undergraduate and graduate curricula, and in the leading textbooks' has created a pedagogical crisis in which 'the narrowness of standard economic training is under attack even from within the mainstream profession' (Coats 2000, p. 145; Rutherford 2000, p. 186). There are growing demands for more real-world complexity and critical thinking in economics education (Earl 2002; Feiner 2002), especially at the graduate level where received curricula are arguably most dysfunctional in terms of the future of our discipline (Fullbrook 2003). Herein lie many opportunities for heterodox economists to exercise leadership in their classrooms and in the promotion of new economic pedagogies, degree programmes, and other initiatives.

A Sen/McCloskey pluralism offers a powerful impetus and rationale for these reforms, especially in the USA, where an unlikely alliance has begun to emerge between critics of standard economic education and an influential student rights group, Students for Academic Freedom. The SAF agenda rose to national prominence with the publication of their Academic Bill of Rights in 2002, stressing the duty of US colleges and universities 'to secure the intellectual independence of faculty members and students, and to protect the principle of intellectual diversity'.[24] The classical liberal principles of the Academic Bill of Rights are nothing new. Its provisions closely parallel the American Association of University Professors' definitions of academic freedom (American Association of University Professors 1940; 1987). But the vigorous reassertion of these principles today across the humanities and social sciences makes this an opportune time for heterodox economists to ask, 'What does the "intellectual independence of faculty and students" and the "protection of intellectual diversity" require in graduate and under-

graduate *economics* education?' Heterodox economists should be among the leaders in this effort, working with colleagues in economics and other disciplines to revisit the meaning and requirements of academic freedom, and to make the case for capabilities-oriented reforms so that every economics degree programme is both expected and able to provide 'an environment of intellectual diversity that protects and fosters independence of thought and speech' (Students for Academic Freedom 2002).

Backhouse (2001) and Davis (forthcoming) argue that broad 'liberal arts' values such as academic freedom are weak grounds on which to challenge mainstream ideas and practices. Yet I wonder (as McCloskey, Dow and Fullbrook may also wonder) if perhaps the 'weakness' of these classical principles might also be their strength – as widely accepted ideals that could potentially inspire a reform movement broader than heterodox economists themselves could generate, as ideals that allow heterodox economists to exploit their comparative advantages in liberal arts education, and, not least, as ideals that focus not on the needs and preferences of heterodox economists themselves but on those of their students (Ancochea 2004, p. 19).

To return to the main argument, a thoroughgoing philosophy and politics of pluralism may have been a terrible mistake for heterodox economists twenty or thirty years ago, but it is not today. Thanks to our own human capital investments and the wholesale abandonment of economic history and the history of economic thought within many PhD programmes since the 1980s, heterodox economists currently possess a comparative advantage in intellectual literacy over many of their mainstream colleagues. This presents heterodox economists with a historic opportunity to re-engage a discipline that is struggling to retain its relevance in a post–Cold War world of 'necessarily mixed' economies (Hodgson 1995, 1998) where formerly dominant '-isms' (capitalism, socialism, communism) are dissolving and evolving into unexpected fragments and combinations, and the twentieth-century dream of 'reaching for heaven on earth' through the rational design and control of national economic systems has arguably come to an end (Havel 1992; Cullenberg 1992; Nelson 1991; Gibson-Graham 1996). The active embrace of a Sen/McCloskey pluralism would also encourage heterodox economists to liberate themselves from the all-encompassing priorities of Cold War oppositionalism (anti-neoclassicism, anti-capitalism, anti-socialism, and so on) that are illiberal and increasingly self-defeating. Dichotomous distinctions such as right/left, market/state, and neoclassical/non-neoclassical continue to limit our analytical range and insight. As Don Lavoie has argued, 'it is time for these more liberal elements of the left and right sides of the old political spectrum to transcend the confines of these obsolete ideologies and work together to articulate a new vision of the free society' (Lavoie 1994, p. 283).

By rethinking their oppositional identities, heterodox economists can remove important barriers to their own intellectual development and work more effectively to restore the freedom (for themselves and their stakeholders) to practise a 'genuine pluralism' that last predominated during the interwar period when '[e]conomists felt at liberty to pursue their own individual combinations of ideas' (Morgan and Rutherford 1998, p. 4). This will require us to demonstrate, again and again, that non-mainstream ideas deserve more space and respect within our departments, journals, and professional meetings, not on the grounds that 'we're right and they're wrong' but because the inclusion of a broader spectrum of economic perspectives is better for our students, our colleagues, and the future of our discipline.

Notes

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1. A Sen/McCloskey vision of economic science parallels Adam Smith's view of the moral and institutional requirements for a free society and free speech (Evensky 1993). This 'heterodox' view of Smith is corroborated by the recent work of Harpham (2000), Blaug (2001) and Wight (2002).
2. This Kuhnian sensibility is evident in 1970s titles such as 'Bourgeois and Radical Paradigms in Economics' (Zweig 1971), 'Radical Political Economy as a "Scientific Revolution"' (Worland 1972), or 'Austrian Economics as Extraordinary Science' (Dolan 1976, pp. 3-18). Dolan's essay is explicit in this regard: 'In contemporary economics, [Kuhn's notion of] normal science is represented by work within the framework of the neoclassical-Keynesian synthesis … [Austrian economists, in contrast, are] "doing extraordinary science."… They are very much concerned with methodological and philosophical fundamentals … [and] share a conviction that orthodox economics is at the point of breakdown, that it is unable to provide a coherent and intelligible analysis of the present-day economic world' (Dolan 1976, pp. 3–4).
3. For Austrians, as for Marxists, this 'rush to science' was propelled by a desire to transform Cold War political projects into academic-cum-scientific ones. 'Austrian economics, as interpreted by its handful of students in the 1950s, needed no refinement, critical reflection, nor change: it was considered free-market wisdom to be dispensed to anybody who would listen, in the hope of rebuilding a political program for laissez faire' (Boettke and Prychitko 1994, p. 7).

   Marxian economists occupied an analogous position in the anti-communist 1950s. As Paul Sweezy explains in reference to his editorship of the socialist magazine *Monthly*

Review during this period, 'We [at *MR*] never considered ourselves to be anything but orthodox Marxists … We were not in any way trying to redo Marxism. We just planned to use it' (Sweezy and Magdoff 1988, p. 91).

4. Sherman and Sawyer's pursuit of a unified framework was partially a response to their intellectual opponents. As Sherman explains: 'Some neoclassicals have denied that radicals have a fully developed paradigm, but this book is intended to present such a radical paradigm' (Sherman 1987, p. 5).
5. I owe this insight to Klamer (2001).
6. Friedman's original quote (from a 1974 conference) is 'There is no Austrian economics – only good economics and bad economics' (cited in Dolan 1976, p. 4).
7. Lee and Keen (2004) offer a compelling critique of the Colander thesis, citing numerous ways in which the logical structure of mainstream economic theory remains distinctly 'neoclassical'.
8. A further consequence of this all-or-nothing view is a conservative tendency to remain committed to received theories and textbooks, despite having serious misgivings about them. 'Too many people, even if they are not completely attached to orthodoxy, do not feel strongly enough to propose an alternative view. So the domination of orthodoxy is in the people themselves. Nobody is going to take a risk to do something really different, because they think they have no alternative theory that is as good as the standard one' (Raveaud, cited in McIntyre 2003, p. 15).
9. The meaning and requirements of pluralism remain undertheorized in economics, though they have begun to receive more attention over the past decade. Informative surveys of major approaches to these issues are provided by Salanti and Screpanti (1997) and Sent (forthcoming).
10. Cullenberg, Amariglio, and Ruccio also articulate a normative pluralism: '[O]ver a century after the marginalist revolution, economic discourse is more heterogeneous than one might expect from a supposedly 'unified' science. [But] this heterogeneity is nothing to bemoan in our view' (Cullenberg, Amariglio and Ruccio 2001, p. 5).
11. Fullbrook's recent collection (Fullbrook 2003) contains the initial 2000–2001 petitions from the French students and professors, as well as the 2001 petitions from Cambridge University PhD students and an international conference of students and faculty at the University of Missouri at Kansas City. The 2002 petition of PhD students in Siena is available at http://www.debating.it/siena2003/conf_phd_econ2003/manifesto.htm. Lee (2003) describes the 2003 petition of Harvard undergraduate students seeking a more pluralistic introduction to economics.
12. In a more recent article, Dow calls her pluralism 'structured' rather than 'qualified' (Dow 2004a). Yet her central argument remains the same: advocating a third-way notion of pluralism that avoids the extremes of modernist monism and postmodern (unqualified, unstructured) pluralism.
13. Dow's vision of interparadigmatic communication, facilitated by an open-system ontology and epistemology, resembles the postmodern Marxian perspective of Resnick and Wolff (1988).
14. As Sent (forthcoming) explains, some forms of pluralism reduce to monism by leaving open the possibility that for every phenomenon there is (or could be, in principle) a single, best account.
15. See also Foss and Loasby (1998, p. 11).
16. 'The purpose of dialogue in either the ordinary conversational sense or in the forms it assumes in various specialized disciplines … is to arrive at a common agreement on a certain issue' (Madison 1994, p. 206).

17. This discussion is indebted to the innovative extensions of Sen's capabilities approach by DeMartino (2000) and Burczak (forthcoming).
18. As noted by Davis (2002), Nussbaum (2003) and Burczak (forthcoming), Sen's framework does not resolve the vexing question of how and by whom the list (and rank ordering) of relevant capabilities and functionings is to be determined in particular situations. Sen prefers to leave his framework 'open-ended' in this way, to make it 'potentially more serviceable to a wider range of applications' (Sen 2004, p. 487). In contrast, Nussbaum (2003) argues that it is both possible and ethically crucial to define, though intercultural dialogue, a minimal set of universal human capabilities. In the present context, this suggests the possibility of establishing minimal requirements for a 'good academic life' through ongoing intra- and transdisciplinary conversations.
19. Hill (2003) proposes a similar extension of Sen, to examine 'the role of institutionalized power in causing and perpetuating inequalities in individual opportunities to achieve'.
20. 'There are many values of the conservative which appeal to me more than those of the socialists; yet for a [classical] liberal, the importance he personally attaches to specific goals is not sufficient justification for forcing others to serve them' (Hayek 1960, p. 398).
21. See also Colander, Holt, and Rosser (2004) on the pivotal role of non-mainstream 'edge' research in the process of scientific discovery and the growth of knowledge.
22. Backhouse (2001) and Lee (2005a, 2005b) suggest several avenues for capabilities-enhancing reforms, to redress the discriminatory treatment of heterodox economists and their ideas within the current social structure of academic economics in the USA and UK.
23. This is one of Sen's major themes: the Aristotelian dimensions of Smith's political economy and how they are interwoven with the negative libertarianism to which Smith's project is commonly reduced (Pressman and Summerfield 2000; Walsh 2000).
24. The Academic Bill of Rights was approved in principle by the 108th Congress, First Session, House Concurrent Resolution 318 (October 30, 2003).

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PART II
Arguments for Pluralism

5
Narrative Pluralism

EDWARD FULLBROOK

Einstein's revolution led philosophers and historians of science to abandon nineteenth-century views of scientific progress as a smooth accumulation of tested facts. Scholars came to focus instead on the processes by which one theory displaces or subsumes another. By the 1960s, obsession with competing theories became so extreme that increasingly all science was defined and interpreted relative to its infrequent revolutions. (Kuhn [1962] 1970). This narrative gestalt has spread through contemporary culture, dominating its perceptions of the advancement of knowledge.

Generally the natural sciences ignore outsider analysis, but the narrative fixation on the dialectical side of scientific development has had and continues to have a deleterious effect on the human sciences. Of course, theory displacement offers a true characterization of important chapters in science history. But there are many major advances in science, including its intervals of 'normal science', for which the narrative of scientific revolutions has no explanatory power. More to the point, in the human sciences those 'extraordinary episodes' that have 'necessitated the community's rejection of one time-honored scientific theory in favor of another incompatible with it', are virtually unknown. (Kuhn [1962] 1970, p. 6). In economics, for example, the absence of such episodes weighs so heavily on its pursuit of understanding that no sensible overview of its fundamental ideas is possible without abandoning the traditional narrative structure.

The notion of *narrative* provides this essay with its central organizing concept. The term is deployed inclusively, so as to encompass everything from the theories of microphysics to the myths of traditional societies. Narratives commonly taught in universities, 'knowledge narratives', will receive primary attention. It frequently happens that in a field of empirical inquiry there emerge several narratives which rather than being contradictory or incompatible are complementary in the sense of offering different windows for observation of the same or overlapping domains of phenomena. Every narrative – and, therefore, every theory, paradigm and research

programme – launches itself from a conceptual framework, including a set of presuppositions about the nature of reality. Inevitably, different conceptual frameworks offer different points of view on the object of inquiry. What one sees when one looks at Michelangelo's statue of David depends on the standpoint from which it is observed; similarly, what any empirical inquiry makes of its object depends on the conceptual framework through which it is viewed. Just as full appreciation of *David* requires viewing it from more than one perspective, so knowledge accumulation often depends upon investigating empirical domains through more than one narrative. I call this the doctrine of *narrative pluralism*. It is the same view of empirical understanding that the physicist David Bohm describes:

> What is called for is not an *integration* of thought, or a kind of imposed unity, for any such imposed point of view would itself be merely another fragment. Rather, all our different ways of thinking are to be considered as different ways of looking at the one reality, each with some domain in which it is clear and adequate. One may indeed compare a theory to a particular view of some object. Each view gives an appearance of the object in some aspect. The whole object is not perceived in any one view but, rather, it is grasped only *implicitly* as that single reality which is shown in all these views. When we deeply understand that our theories also work in this way, then we will not fall into the habit of seeing reality and acting toward it as if it were constituted of separately existent fragments corresponding to how it appears in our thought and in our imagination when we take our theories to be 'direct descriptions of reality as it is'. (Bohm 1983, pp. 7–8)

The details of these and related arguments will be set out in three sections. First, the narrative function of conceptual frameworks will be explained by examining their various standard elements. Second, twentieth-century physics will be surveyed as an exemplary case of narrative pluralism and its benefits. Third, narrative pathologies common to the human sciences and a consequence of anti-pluralism will be identified.

Narrative Selection

Simplification

'[E)xperience has to organize', wrote Henry James, 'some system of observation – for fear, in the immensity, of losing its way' (James 1962, p. 3). At the social level, this path finding embodies itself in various forms of representation: maps, verbal accounts, formulae, systems of equations, graphs, pictures, etc. All representations, whatever their form, proceed on the basis of a simplification of reality. There are no exceptions to this rule, not even the most sophisticated scientific theories. Jorge Luis Borges's parable 'Of

Exactitude in Science' illustrates the folly of disregarding this most fundamental of all narrative principles:

> In that Empire, the craft of Cartography attained such Perfection that the Map of a Single province covered the space of an entire City, and the Map of the Empire itself an entire Province. In the course of Time, these Extensive maps were found somehow wanting, and so the College of Cartographers evolved a Map of the Empire that was of the same Scale as the Empire and coincided with it point for point. Less attentive to the Study of Cartography, succeeding Generations came to judge a map of such Magnitude cumbersome, and, not without Irreverence, they abandoned it to the Rigours of sun and Rain. In the western Deserts, tattered Fragments of the Map are still to be found, Sheltering an occasional Beast or beggar; in the whole Nation, no other relic is left of the Discipline of Geography. (Borges 1975, p. 131)

But charming and useful as it is, Borges's parable illustrates only one aspect of any representation's need for simplification. For every empirical domain there exists an infinity of possible points of view and, therefore, also of potential observations. These plethoras of possibilities together with the dilemma posed by Borges present observer/narrators with an acute problem of choice. They must decide which features of their domains they are going to describe and which they are going to disregard. Each of their narratives can proceed only on the basis of a radical simplification of reality. To this end, and in lieu of random observations from random points of view, narrators deploy principles of selection, or what James called 'systems of observation' and today's writers usually call 'conceptual frameworks'. This process abstracts certain features of the narrative's domain while ignoring others. A narrative may make explicit its narrative framework, but more often it leaves it partly or wholly concealed, leaving it to operate outside critical awareness.

We must not forget that knowledge narratives, no less than popular and literary narratives, explore reality by simplifying it. They obscure great masses of detail, so as to highlight systematically certain aspects of that reality which a group of individuals have identified as being of special interest to themselves. Different but non-competing narratives of the same domain give prominence to different dimensions of that domain. Each narrative functions as an interpretative system, as a *special* way of perceiving some corner of existence.

Narrative selection proceeds through a set of assumptions that simplify or pre-empt many features of the narrative's domain. These assumptions include a system of classification of entities, the attribution of a limited number of properties to those entities, some metaphysic that posits a kind or kinds of connection between events, and usually the recognition of different structural levels within the domain of inquiry. A narrative also views its

domain at a certain scale, omitting details that it sees as too microscopic or too global, too short-run or too long-run. Typically it also describes its domain within some range of accuracy or approximation, ignoring effects that do not fall within that range. Finally, every knowledge narrative has its community of practitioners, people who develop and deploy the narrative in writing and teaching. As socially, economically, geopolitically and historically situated individuals, these people bring to the narrative enterprise various inclinations and sensibilities, as well as overt purposes, all of which help determine which aspects of the domain the narrative includes, emphasizes and ignores.

Classification

Wittgenstein noted that '*The limits of my language* mean the limits of my world,' and that 'what we cannot think we cannot *say* either' (Wittgenstein 1974, 5.6, 5.61; original emphasis). Our categories of thought, including our groupings of the objects of the world, pervade our descriptive use of language and organize all our experience. Even the predicates of everyday language categorize, though not always very precisely, the contents of the world. These informal classifications, with their mixtures of the personal and the cultural, are the means by which we order the perceptual fields of our daily existences. Similarly, every narrative needs to provide some classification of the objects in its domain.

In the specialized narratives of science this shaping of the facts is especially pronounced because the number of categories tends to be strictly limited. The selection of categories inevitably involves arbitrariness because there exist countless numbers of objectively grounded ways in which the contents of a domain can be categorized. Another parable from Borges illustrates this inescapable aspect of narratives. An Argentinean consults an imaginary Chinese encyclopaedia which says that 'animals are divided into: (a) belonging to the Emperor, (b) embalmed, (c) tame, (d) sucking pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in the present classification, (i) frenzied, (j) innumerable, (k) drawn with a very fine camel-hair brush, (l) et-cetera, (m) having just broken the water pitcher, (n) that from a long way off look like flies' (cited in Foucault 1971, p. 2).

The outlandishness of Borges's imaginary taxonomy of the animal kingdom, as well as the ambiguity of its selection criteria, suggests the diversity of ways in which one can, without forgoing objective grounding, categorize a sector of reality. Make-believe classifications, however, are not alone in making manifest the arbitrariness of conceptual orders and their resulting perceptual fields. Ethnological studies offer numerous examples of zoological classifications whose nonessential nature is immediately obvious to outsiders.

Consider the case of the villagers of Baan Phraan Muan in northeastern Thailand. They divide the animal kingdom on the basis of two criteria: edibility and habitat (Tambiah 1969). These generate five major primary categories: insects (inedible), birds (edible), water animals (edible), animals of the house and village (animals in the house are inedible, animals under the house are edible) and forest animals (animals of the deep forest are inedible and other forest animals are edible unless they have domesticated counterparts in the house). But these criteria leave numerous organisms known to the Muan villagers standing awkwardly alone in their own primary classes and rivalling Borges's for their apparent fancifulness. These anomalies include house rat (only small children eat), field rat (only small children eat; adults eat privately), giant lizard (medicinal food for children), monitor lizard (edible, but dangerous to mothers after childbirth), chameleon (medicinal food), snake (inedible), vulture (inedible) and crow (inedible).

The Karam people of New Guinea also use habitat as one of the two criteria by which they classify the animal world (Bulmer 1967). But their notion of habitat differs from that of the villagers of Muan in being two-dimensional. Its horizontal axis has the forest at one pole, the homestead at the other and open country and gardens in between. Its vertical axis runs from aerial through arboreal, terrestrial and aquatic, to subterranean. The Karam's second set of criteria are morphological (physiological): winged or wingless; bony or boneless; bipedal, quadrupedal, multipedal or limbless; elongated or not; and large, medium-sized or small. These two sets of criteria divide the Karam's zoological world into 94 primary categories. One of these, flying birds and bats, contains 44 per cent of the Karam's 422 named organisms, whereas another includes only tadpoles.

Cultural bias may incline us to attribute the disparateness between the Muan villagers' and the Karam's ways of dividing up the animal world as due to their common absence of a scientific basis. More especially, we might expect that modern biology, with its grounding in evolutionary theory, would provide for animals a determinate and definitive classification. But that is not the case. Science teaches us that the evolutionary process abounds with ambiguities. It is not even clear what are the units that survive or become extinct. Are they genes, fragments of genes, chromosomes, genotypes, phenotypes, groups of organisms, gene pools or species? This assortment of possible basic units has generated various formulations, offering different points of view on the selection process. It is this family of narratives which comprises modern evolutionary biology.

Nor does nature's biological ambiguity as revealed by science end here. Not one but numerous concepts of 'species' have emerged from evolutionary theory (Dupré 1993, pp. 37–59). These concepts divide into two types, the 'biological' and the 'phylogenetic'. The former defines a species as 'a group

of organisms connected to one another by actual or possible reproductive links, and reproductively isolated from other organisms.' (Dupré 1993, p. 46). Though we may find the biological species concept intuitively satisfying, it is inapplicable to asexual organisms and, therefore, to most micro-organisms and, therefore, to microbiology. Phylogenetic taxonomies, on the other hand, have as their basic principle that the organisms forming a species should descend from a common set of 'ancestors'. But in an evolutionary context this condition obviously is not sufficient. Rules are needed to identify cut-off points in the lines of descent, and to establish 'what makes a genealogically coherent set of organisms correspond to the rank of species' (Dupré, p. 48). To this end, various criteria, each leading to a different classification, have been put forward and used in modern biology.

The plurality of possible basic units of selection and the diverse concepts of 'species', however, are neither the only nor the most profound manifestation of pluralism in the classification of organisms in biology today. In ecological biology, niche, not species, is the basic classificatory unit. The idea of niche more resembles the Muan's and the Karam's implicit concept of habitat than it does any of evolutionary biology's notions of species. Frequently more than one species can perform the role required of a particular ecological niche. Consequently, ecological-based classifications of organisms differ greatly from evolutionary-based ones (Dupré 1993, pp. 43, 58).

Finally, a brief example from Thomas Kuhn will reinforce much that has just been said. It reveals two different classificatory concepts for 'molecule' concurrently and productively at work in the physical sciences. Kuhn relates the responses of a 'distinguished physicist and an eminent chemist' when asked whether a single atom of helium was or was not a molecule:

> Both answered without hesitation, but their answers were not the same. For the chemist the atom of helium was a molecule because it behaved like one with respect to the kinetic theory of gases. For the physicist, on the other hand, the helium atom was not a molecule because it displayed no molecular spectrum. Presumably both men were talking of the same particle, but they were viewing it through their own research training and practice. Undoubtedly their experiences had had much in common, but they did not, in this case, tell the two specialists the same thing. (Kuhn [1962] 1970, pp. 50-1)

The gist of this and of our other examples of classification can now be summarized. Borges's zoological fantasy, by means of what are from conventional viewpoints its glaring omissions, called our attention to how any classification of an empirical domain limits the possible descriptions, and thereby also the field of possible facts and possible questions. Similarly, without discounting their epistemological value for the cultural-geographical situations to which they are applied, the alien taxonomies of the Muan and

the Karam encourage us to recognize the indeterminateness and contingency of all classifications of empirical realms. But we also have seen from examples from contemporary biology that even when it comes to dividing up a domain on the basis of the most advanced science there exist more than one plausible and defensible way of doing so. *The best way will depend on the purposes of the narrative for which the classification is intended*. Every categorization of a set of empirical phenomena uniquely circumscribes our possible understanding of that realm of reality, rather as every position one takes up around Michelangelo's statue of David limits what one can see. Likewise, the numerous ways in which any domain can be divided up means that there exist many different bases for making a systematic inquiry of that domain.

Selection of properties

Of all narrative genres, ontologies are the most elemental because they make assertions about the fundamental nature of reality – about what sorts of entities, properties and relations compose existence. But all narratives, and especially knowledge narratives, postulate a sort of proto-ontology in the sense of identifying a certain range of phenomena (a 'universe of discourse') whose existence, real or imagined, they wish to take into account. In the formation of these proto-ontologies, the classification of entities typically requires the predication of various properties, making these two processes inextricably intertwined. This conceptual interdependency is especially pronounced in the more narrowly focused physical sciences, which, from out of the welter of phenomenological possibilities emanating from some empirical domain, abstract a very limited set of phenomena for cognitive attention.

Highly specialized proto-ontologies are commonplace in the study of physical matter. Consider the case of crystallography, the scientific study of crystals. It divides solid bodies into two classes: crystals and non-crystals. This division presumes certain properties – approximately plane geometrical surfaces with straight edges that meet other such planes, thus bounding the object on all sides – that identify some materials as belonging to the crystal category. Along with six kinds of symmetry (mirroring, inversion, and twofold, threefold, fourfold and sixfold rotations) these properties – not mass and extension or chemical composition or market value – are the fundamental properties of the crystallography narrative. These selected attributes divide the class of all crystals into 32 subclasses. The result is a powerful but limited descriptive system, one of many useful frameworks of classes and properties for viewing solid objects.

A classification of objects leads to further questions about what additional characteristics of the entities classified the narrative should recognize. For

example, in regarding material substance, classical mechanics includes the properties of mass and length, but not the symmetrical properties of crystals or the colligative properties of solutions. The immensity and richness of actuality compels even the most comprehensive narratives to exclude more characteristics than they include. For this reason, the descriptions of any narrative are always stylized abstractions of reality. Nor is it only knowledge narratives which are characterized by this sort of abstraction. All narratives, even Joyce's *Ulysses* and Proust's *Remembrances of Things Past*, take shape on the basis of radical exclusion of phenomenal detail. The Nigerian philosopher and anthropologist Robin Horton illustrates this narrative principle at work both in traditional African religion and in modern science:

> [W]hen traditional thought draws upon people and their social relations as the raw material of its theoretical models, it makes use of some dimensions of human life and neglects others. The definition of a god may omit any reference to his physical appearance, his diet, his mode of lodging, his children, his relations with his wives, and so on. Asking questions about such attributes is as inappropriate as asking questions about the colour of a molecule or the temperature of an electron. It is this omission of many dimensions of human life from the definition of the gods which gives them that rarefied, attenuated aura which we call 'spirituality'. It is the result of the same process of abstraction as the one we see at work in Western theoretical models: the process whereby features of the prototype phenomena which have explanatory relevance are incorporated into a theoretical schema, while features which lack such relevance are omitted. (Horton 1971, p. 225)

This idea of 'explanatory relevance' suggests a further dimension of conceptual frameworks, namely the inclusion of some basis for conceiving connections between various categories of phenomena and their properties.

Interconnectivity: ten kinds of narrative linkage

Narratives need notions about how the things they classify and describe are connected. '[T]he most usual species of connection', said David Hume, 'among the different events which enter into any narrative composition is that of cause and effect'. (Hume [1748] 1955, p. 34). The relation of causation holds between two events when, given the occurrence of one event, it results in a second. The putative causal event may be either natural or supernatural, and the relation may be postulated either as a general rule as in the laws of chemistry and the procedures of witchcraft or as a singularity as with events in a novel. Causal linkages make phenomena fall into configurations, enabling us to apprehend various items as contributing to an interrelated system of parts or forming an intelligible pattern of events. This showing of things leading to other things distinguishes narratives from mere

listings, descriptions and chronological sequences. I wish to consider these cause-and-effect linkages with regard to five criteria: whether they explain in terms of past or future events, whether these explanations are open or closed in the sense of admitting or not admitting indeterminacies, whether they explain a property of something as due only to that something's parts or as due also to the structure by which those parts are organized, whether they explain the whole in terms of its part or vice versa, and whether between entities they postulate internal or external relations.

TELEOLOGICAL AND NONTELEOLOGICAL EXPLANATIONS

Time's linearity leads to two basic methods of framing narrative connections between events happening at separate moments. Items may be explained in terms of their consequences, as when we say Othello fell on his sword because he wanted to die. Alternately, an explanation may run in the other direction, the consequences explained in terms of some prior event, as when we say Othello died because he fell on his sword. Explanations of the former type are called teleological or functional and find frequent use with respect to human actions. Such usage arises from regarding humans as purposive beings, a view that obliges us to explain their behaviour, at least in part, as a function of wishes to bring about various future events. Consequently, the human sciences abound with narratives that explain operations in terms of their consequences. But the range of knowledge narratives that rely heavily on functional linkage is much broader than this, and it is examples from outside the human sciences that I want to emphasize here.

Functional or teleological narratives interpret processes from the perspective of 'wholes' or systems of interconnected components desiring or designed for the achievement of some end, in other words, a future event. Such narratives focus attention on culminations and consequences, and link the behaviour of each component to the end or purpose of the whole or system to which it belongs. Physiology is a well-known example of a primarily teleological knowledge narrative. It proceeds by identifying the function an organ performs for its organism and how it works to that end. Likewise, more often than not we perceive human artifacts, especially advanced technology, through functional or teleological narratives. A comb is a device for untangling hair; an automobile is a mechanism for getting about in and, sometimes, for impressing one's neighbours. Functional analysis identifies and classifies an entity's parts in terms of their subfunctions. For example, we commonly analyse an automobile into its various parts – a fuel system, an ignition system, a carburettor, some combustion chambers with pistons, a crankshaft, a transmission, a chassis, a set of wheels, a steering wheel, a braking system, and seats – and explain them in terms of their contribution to the intended function of the whole. The same kind of teleological account

pertains to a system's subcomponents and their operations. Continuing with the car example, a science dictionary tells us that the crankshaft is an 'essential component of piston engines that converts the up-and-down (reciprocating) motion of the pistons into useful rotary motion' (Lafferty and Rose 1994, p. 159). The entry then explains how the components of the crankshaft work to this end. Technological culture could not exist without narratives of this type.

Proceeding from the other direction, nonteleological explanations focus attention on the conditions and events preceding the event, process or state of affairs being explained.

> They seek to exhibit the integrated behaviours of complex systems as the resultants of more elementary factors, frequently identified as constituent parts of those systems; and they are therefore concerned with traits of complex wholes almost exclusively to the extent that these traits are dependent on assumed characteristics of the elementary factors. (Hempel 1966, p. 93)

For example, under this narrative mode the crankshaft's conversion of reciprocating motion into rotary motion is interpreted in terms of the laws of mechanics, the firing of the pistons, and the initial conditions constituted by the crank pins, the connecting rods and bearings, and the crankshaft.

CLOSED AND OPEN NARRATIVES

Turn now to another, more difficult, and more provocative aspect of narrative linkage, the distinction between determinate and indeterminate explanations. Some narratives are *closed* in the sense that they describe all their events as predetermined, whereas others are *open* in the sense that they admit indeterminacies. Narratives divide between these two categories. Those of the closed or determinate variety claim that given X, Y must follow, whereas open or indeterminate narratives explain Y in terms of X without the presumption that Y *always* follows X. If a field of inquiry is not seen as wholly determinate, meaning that chance, contingency, choice, uncertainty, randomness, or spontaneity enter into the relations between events, then the sets of events open to explanation by the determinate and indeterminate approaches are not coextensive. With these different ranges of application, the choice between the two forms of narrative linkage is one of selecting a method appropriate to the perceived subject matter. As such, this question of finding a suitable narrative form must not be conflated with the metaphysical question of whether reality in general is determinate or not. Traditionally philosophers have lavished attention on the latter question, but for us it need not be at issue. Here we want merely to consider two types of narrative linkage, two conceptual angles offering different vantage points on the field of observation. As I will illustrate, within the same domain of

inquiry both types of explanation may prove useful. Like the hammer and saw, the use of one conceptual tool does not preclude the use of the other.

To place these joint notions of open and closed narratives in a more traditionalist context, consider Popper's definition of a physically closed system:

> By a physically closed system I mean a set or system of physical entities … which interact with each other – and *only* with each other – in accordance with definite laws of interaction that do not leave any room for interaction with, or interference by, anything outside that closed set or system of physical entities. (Popper 1972, p. 219)

This definition, when modified as follows, defines a closed or determinate narrative. By a closed narrative I mean an account of a set or system of entities and their interactions with each other – and *only* with each other – in terms of definite laws of interaction that do not leave any room for interaction with, or interference by, anything outside that closed set or system of entities.

Tolerance for open or indeterminate narratives, however, is very much a modern development. Robin Horton notes that in the traditional cultures of Africa, the concept of coincidence or chance scarcely exists:

> When a rotten branch falls off a tree and kills a man walking underneath it, there has to be a definite explanation of the calamity. Perhaps the man quarrelled with a half brother over some matter of inheritance, and the latter worked the fall of the branch through a sorcerer. Or perhaps he misappropriated lineage property, and the lineage ancestors brought the branch down on his head. *The idea that the whole thing could have come about through the accidental convergence of two independent chains of events is inconceivable because it is psychologically intolerable*. To entertain it would be to admit that the episode was inexplicable and unpredictable: a glaring confession of ignorance. (Horton 1971, p. 250; emphasis added)

But Western culture also has exercised a strong bias against open narratives. This partiality, which until a century and a half ago was hegemonic, owes more than a little to Aristotle. His *Poetics* scorned narratives whose episodes 'follow each other without any probable or necessary connection', and applauded the *Odyssey* and the *Iliad* for the manner in which their events are 'connected into one event' (Aristotle 1934, Part II, sec. V). With incomparable influence, Aristotle argued that actions 'should arise from the structure of the fable itself, so as to be the natural consequences, necessary or probable, of what has preceded in the action' (Aristotle 1934, Part II, sec. VIII). Moreover, 'the fable … should be an imitation of an action that is one and entire, the parts of it being so connected that if any one of them be either transposed or taken away, the whole will be destroyed or changed' (Aristotle 1934, Part II, sec. V). Determinism as embodied in many scientific theories is but a variation of this ancient sensibility regarding narrative and

the connection of events. Newtonian mechanics, especially as reworked by Laplace, achieves perfect 'unity of action'. Given the positions and velocities of all the particles at any one moment, this narrative's system of equations determines the positions and velocities, and thereby the actions, of all particles for all moments, both future and past. With every event portrayed as part of an unbroken chain of events, if any one of them fails to take place, then the whole scientific narrative would, in effect, 'be destroyed'.

Horton's example of the falling tree branch, however, suggests that some happenings may not, at least from an epistemological point of view, always best be described and understood as emanating from a single and predetermined chain of events. Observation may repeatedly reveal gaps in such chains or chance convergences of two or more such chains, showing elements of unpredictability or randomness in reality. Historically these indeterminacies have proved no less 'psychologically intolerable' to many scientists and philosophers of science than they have to members of traditional African cultures. The willingness of the cultural elites of Western societies to engage with open narratives is an even more recent development than their willingness to engage with democratic processes. Prior to Darwin, no space existed in scientific narratives for indeterminate phenomena. This dimension of reality was barred from scientific inquiry no less than was heliocentric cosmology under the popes. 'The doctrine of scientific determinism', writes Stephen Hawking, 'remained the standard assumption of science until the early years of this century' (Hawking 1995, p. 59). As a physicist, Hawking thinks of quantum mechanics as the breakthrough narrative, but biologists have the better claim to being the first natural scientists to develop an open narrative that successfully breached the determinist hegemony. Evolutionary theory from Wallace (1858) and Darwin (1859) onwards relies heavily on indeterminacy as a narrative linkage.

Neo-Darwinism, which combines natural selection with Mendelian genetics and whose advent was roughly contemporaneous with the development of quantum mechanics, exemplifies open knowledge narratives. Neo-Darwinism admits indeterminacy at several levels. It predicates two sources of heritable variation, both conceptually conceived as indeterminate processes. First, the genes of each individual are the result of a random shuffle of existing genetic material (genetic recombination). Second, random mutational jumps occur due to accidents in replication and repair of DNA, accidents now attributed largely to cosmic rays modifying gene structures. Natural selection operates on these randomly shuffled and mutating genes within the field of a changing environment. The evolutionary narrative also treats this form of variation as indeterminate, as resulting both from random non-biological causes – for example, meteorites, volcanic eruptions, continental drift – and from the indeterminate and recursive process of natural

selection itself. Modern evolutionary biology includes these indeterminate narrative linkages as well as determinate ones from the laws of inheritance, most especially that *in every case* mixtures of characteristics inherited from the parents do not blend but remain distinct.

Despite the development in the natural sciences of hugely successful narratives embracing 'concepts which formally recognize the existence of various kinds of limitation upon the possible completeness of explanation and prediction' (Horton 1971, p. 250), there remain scientists and philosophers who retain a nostalgia for Newtonian certainties. The vision of a clockwork universe – no less than one governed by ubiquitous spiritual agency – is a dream not easily forgone. For those wedded to the metaphysics of determinism, quantum mechanics is but a halfway house to perfect knowledge, while evolutionary theory scarcely qualifies as science, it being so 'riddled' with indeterminacies. But metaphysical belief aside, the open narratives of quantum mechanics and evolutionary biology are the biggest success stories of modern science, especially as applied to the practicalities of technology. For better or worse, we live on the eve of the brave new world of genetic engineering, whereas already quantum mechanics, notes Hawking, 'governs the behaviour of transistors and integrated circuits, which are the essential components of electronic devices such as televisions and computers, and is also the basis of modern chemistry and biology' (Hawking 1995, p. 62).

EXTERNAL AND INTERNAL RELATIONS

We need to consider briefly a further aspect of causality which impacts on the distinction between closed and open narratives. This is the question of whether or not a narrative admits internal, as well as external, relations. A narrative may be mechanistic in the sense that the internal structures of its fundamental elements are independent of one another, the elements being connected only by external relationships. The classic detective novel, with its resolution worked out in terms of interactions between unchanging characters, exemplifies this type of narrative linkage. So too does Newton's mechanics, where the causal relations of collision and gravity leave the particles atomistically intact. Internal relations, on the other hand, are 'identity-affecting' (Bhaskar 1986, p. 111). Interactions between characters in a literary novel, for example, usually bring about 'character development'. The description of internally related phenomena has been even more central to the modern development of the natural sciences. This is illustrated by evolutionary theory, which is the story of how the identity of biology's primary units change through interaction.

AGGREGATIVE VERSUS STRUCTURAL PROPERTIES

There exist two primary ways of explaining properties. Some narratives

explain the properties of things as simply the function of the properties of their parts. For example, engineering treats an object's mass as merely an additive function of the masses of its parts, and the floor space of the Empire State Building as the sum of the floor spaces of its various rooms. Properties explained in this way I will call *aggregative*. Some knowledge narratives deploy only this approach in their conceptualization of properties. For example, classical mechanics is based on only three properties – mass, length and time – and with each described in terms of an additive function. Further or 'derived' properties are defined in terms of these three primary 'dimensions', as for example, velocity is length divided by time, and momentum is mass times length divided by time. Thus, although classical mechanics includes an extensive list of properties, they all reduce to some mathematical combination of the three primary aggregative properties.

There exist, however, many things possessed of properties that are not properties of their components, but instead come to exist only through the structures by which things are combined. Therefore many fields, and especially the biological sciences, include properties explained as being due to the characteristics of the *structure* by which something's components are combined, rather than as an aggregation of microproperties. The property of being able to see, for example, is explained not just in terms of the various individual cells of the eye and brain – none of which have the property of being able to see – but also in terms of the way those cells are combined. Similarly, human crowd behaviour is understood as depending on the relations holding between the individuals as well as on the individuals themselves. Although it was Newton's dream that someday all of existence could be accounted for in terms of aggregate properties, modern science has tended to involve itself ever more with structural properties. Even physics, with its various field theories, today concerns itself fundamentally with structural explanation.

DIRECTION OF CAUSATION: MICRO OR MACRO

Reality presents various levels of complexity, running from atomistic individuals to the universe. This polarity entails two possible directions of narrative explanation: accounting for the more complex in terms of the less so or vice versa. The first approach, 'micro explanation', characterizes Newtonian physics and for several centuries dominated the natural sciences. Chemistry, for example, advanced by describing the decomposition of compound substances by chemical processes into simpler compounds or into their constituent elements. But sometimes the object of inquiry begs a macro approach, as when a property of an individual thing appears mediated or determined by the whole or ensemble of which it is a part. The facts that I grew up speaking English instead of Chinese and eating with a knife and fork

instead of chopsticks, for example, seem more attributable to the family and society in which I emerged than to any aspect of my individual make-up. Likewise, when I die, although the event will fit some micro explanation such as heart failure or perforation of the intestine, the complex changes that will then befall the millions of cells out of which I am composed will be seen to be due to the regrettable change in the whole to which they belong.

Because the metaphysics that grew out of Newtonian science was for so long hegemonic, even today there persist pockets of prejudice against the use of macro linkages in knowledge narratives. Yet science has long conceived of some quantitative properties, such as angle and probability, as based on macro relations. Thus any change in the size of a deck of cards causes every card's probability of being drawn to change. Even more noteworthy is that in physics itself, quantum mechanics has forced through innovations in the use of narrative linkages, placing macro explanation on an equal footing with the older micro variety. The quantum factor, explains the physicist Paul Davies, 'denies that the world can be understood in terms of its components alone'. Davies continues:

> the reality of the subatomic particle cannot be untangled from the environment it inhabits … Evidently the macroscopic and the microscopic worlds are intimately interwoven. There is no hope of building a full understanding of matter from the constituent particles alone. Only the system *as a whole* gives concrete expression to microscopic reality. The big and the small co-exist. One does not subsume wholly the other, nor does the other wholly 'explain' the one. (Davies 1995, p. 39)

The Narrative Pluralism of Twentieth-century Physics

Until the appearance of Einstein's theory of relativity (1905, 1915), Newtonian mechanics with its theory of gravity was unrivalled as the most celebrated theory in the history of science. Its verification by countless experiments and astronomical observations supported the prevailing view of science as a smooth accumulation of facts generated by the application of well-tested theories. So inevitably the discrediting of Newton's theory dismayed and shocked the cultural psyche, traumatizing twentieth century thought about scientific advance, and fixating its attention on events structurally resembling the Einsteinean revolution.

Initially there was strong resistance to Einstein's new narratives of gravitation and cosmology, Newton's theory of absolute space and absolute time having for so long been accepted as an unquestionable truth. But following the solar eclipse of 1919, when Einstein's predictions were confirmed by two teams of astronomers, there began a cultural shift regarding the nature of scientific progress. Philosophers and historians of science

especially faced a new narrative challenge. The historical situation no longer pressed them to account for continuity in science nor permitted them to characterize science as a process whereby new certainties are endlessly added to existing ones. Instead they struggled to identify and describe the processes by which one theory could or should replace or withstand a challenge from another. The first major work to recast the narrative of scientific progress in terms of *competing theories* was Karl Popper's *The Logic of Scientific Discovery* published in German in 1934.

Popper showed that no amount of verification and inductive support can ever prove a theory. Instead every theory always remains vulnerable to refutation and replacement by another. This narrative nicely accommodated the recent astounding events in physics. Popper's account of theory replacement spelled out various methods, including degrees of falsifiability (Popper [1935] 1959, pp. 135, 112–35), empirical content (pp. 119–23), degrees of simplicity (pp. 136–45) and degrees of corroboration (pp. 251–82), for judging between competing theories. Under Popper's narrative of scientific discovery, competing theories fight it out on the basis of these criteria of scientific merit, and the 'best' one wins.

From the 1960s onwards Popper's version of the new narrative of scientific progress increasingly came under attack. Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) denied the historical efficacy of Popper's objective criteria for theory replacement, arguing instead that competing theories or 'paradigms' are often incommensurable and that sociological factors, rather than epistemological ones, often determine whether one theory is or is not replaced by another. Imre Lakatos's 'Falsification and the Methodology of Scientific Research Programmes' (1970) argued that refuted theories may continue to be used if no better theory exists. Paul Feyerabend's 'Against Method' (1970) emphasized that all observation is 'theory-laden' and contended that no set of methodological rules can account for theory-replacement and that all knowledge claims are relativistic. But these and other alternatives to Popperian falsification were variations of the basic narrative that had emerged as the natural aftermath of Einstein's revolution. Each added to the collection and interpretation of historical science data to answer questions suggested by the competing-theories narrative. Almost inevitably the decades of debate on theory replacement have had as their primary effect the deepening and widening of our culture's general perception of scientific progress as the outcome of a struggle between competing theories.

This chapter challenges not the narrative of competing theories as such, but rather the hegemony that narrative maintains over our vision of science. That that narrative fits important chapters in science, including the momentous one that inspired it, is above dispute. But there is much more to conceptual science than just the postulation of frameworks that challenge

other frameworks. Formulation of scientific narratives is also about gaining new points of view on domains of inquiry. Viewing the domain from a new conceptual perspective may yield not only additional information but also a new dimension to the understanding of it. The new viewpoint may even reveal fundamental phenomena which were but dimly observable or not observable at all when looking through a prior conceptual system. That such new knowledge may be conceptually incommensurable with that acquired through another narrative lens should be regarded not as a scandal but rather as due to the nature of conceptual thinking. Except in the special case where two narratives make conflicting predictions, incommensurability between narratives does not argue for competitiveness between them. To the contrary, observing a domain of inquiry through more than one conceptual framework is eminently desirable, as is observing Michelangelo's *David* from more than one standpoint.

Phenomena observed through different conceptual systems may eventually be reconciled through a 'deeper' level of theory (like a 'bird's-eye view'), as with Maxwell's unification of electronic and magnetic theory. But such unification can never happen except where *narrative pluralism* first prevails for that domain of inquiry.

The narrative of competing theories, especially Kuhn's version, seriously underestimates the scientific imagination, that talent John Stuart Mill characterized as the faculty for 'mentally arranging known elements into new combinations' (Mill 1893, p. 433). Kuhn's narrative assumes that the scientific mind is so deficient in agility as to be incapable of alternating freely between incommensurable conceptual systems. I would be the last to deny that examples of this stereotype exist in every discipline and that in some this intellectual ineptitude may even dominate. Nor do I deny that narrative communities sometimes exist in bondage to their conceptual system because they have failed to make explicit its primary presuppositions. But it seems a cruel travesty of the truth to portray the scientist in general, on the one hand, as an intellectual bumpkin, incapable of shifting between conceptual gestalts and, on the other, as a moral midget, committed primarily to the glorification of a particular narrative point of view rather than to the understanding of the empirical domain to which that narrative and others refer.[1]

For too long, historical data from science have been collected, selected and interpreted mainly to answer questions posed by the various versions of the competing-theories narrative of scientific progress. The case for regarding this narrative as a general explanation of scientific advance has, in its various forms, been constructed primarily on the basis of examples drawn from physics. Yet even here on its most favoured ground it is a simple matter to show that the narrative of competing theories not only fails to account for but also runs counter to most major developments.

In physics today, fundamental research is focused primarily on 'unification', as indeed it has been for a couple of generations now. Various schemes are used to characterize 'the unification process', but all describe a state of affairs incomprehensible in terms of the traditional competing-theories narrative of scientific development. Stephen Hawking, for example, explains the quest as follows:

> Today scientists describe the universe in terms of two basic partial theories – the general theory of relativity and quantum mechanics. They are the great intellectual achievements of the first half of this century. .... Unfortunately, however, these two theories are known to be inconsistent with each other – they cannot both be correct. One of the major endeavours in physics today ... is the search for a new theory that will incorporate them both – a quantum theory of gravity. (Hawking 1995, p. 13)

Reading this passage through the competing-theories lens invites total misunderstanding. Physicists perceive relativity and quantum mechanics not as competing theories, but rather as different and complementing conceptual approaches to the fundamentals of physical reality. These two narratives illuminate separate facets of what unification physicists see as ultimately the same domain of inquiry, but which cannot yet be reconciled with each other. The unification dream, with its implicitly deeper level of understanding, arises directly out of the coexistence of the two narratives, the heuristic significance of each being enhanced by the existence of the other. Physicists do not seek to discredit either relativity or quantum mechanics, but rather to create 'a new theory that will incorporate them both'.

Another and more common conceptualization of the unification project of physics centres on the four forces of nature: gravity, electromagnetism, the weak nuclear force and the strong nuclear force. Physicists aim to develop a theory that merges the four forces into a single narrative scheme, or, as Hawking puts it, 'to find a unified theory that will explain all four forces as different aspects of a single force' (Hawking 1995, p. 76). The theories of gravity, electromagnetism, and the two nuclear forces, as well as the theory of the electroweak force (a unification of the theories of electromagnetism and the weak nuclear force) are referred to as 'partial' theories, because their frameworks of interpretation permit only partial and unreconciled views of the domain of force phenomena. *They are conceptually different ways of looking at that domain, and because they are conceptually different they reveal different dimensions of that domain*. Here again, as with electromagnetism, narrative pluralism is the indispensable prerequisite of fundamental scientific advance.

Shifting between narratives with fundamentally different conceptual systems can be a daily occurrence for twenty-first-century physicists. The time is long past when one could make a mark in theoretical physics without the ability to move freely between conceptual gestalts. Modern physics

requires not only mathematical prowess but also conceptual agility. Unlike theory replacement, unification of narratives for a given domain demands the ability to jump back and forth between three or more conceptual systems: those of the incommensurate narratives and that of the narrative intended to effect the merger. But physicists working on unification projects are not alone in requiring conceptual ability. Today to become a physicist of any kind, one must master the basic concepts of both relativity and quantum mechanics. All the rest of modern physics is derived from one or the other of these two theories *whose conceptual frameworks differ radically*. Indeed 'the basic concepts of relativity and quantum theory', notes David Bohm, 'directly contradict each other' (Bohm 1983, p. 176). General relativity conceives of space and time as continuous; quantum theory conceives of them as discontinuous. General relativity conceives of matter as particulate; quantum theory conceives of it as a wave–particle duality. General relativity conceives of physical objects as having actual properties; quantum theory describes them as having only potential properties within the given physical situation. General relativity conceives of all physical reality as determinate and all events as in principle having a causal explanation; quantum theory admits indeterminacy and events incapable of causal explanation. Conceptual differences greater than these are scarcely imaginable. In their fundamentals, relativity and quantum theory share little in common as descriptive approaches to physical reality. Yet for most of a century these two metaphysically dissimilar narratives have worked not in competition but in tandem to produce arguably the greatest advances in the history of science.

Anti-knowledge

Robin Horton has categorized the similarities and differences between African traditional thought and Western science. He identifies a general principle of divergence:

> What I take to be the key difference is a very simple one. It is that in traditional cultures there is no developed awareness of alternatives to the established body of theoretical tenets; whereas in scientifically oriented cultures, such an awareness is highly developed. It is this difference we refer to when we say that traditional cultures are 'closed' and scientifically oriented cultures 'open'. (Horton 1971, p. 230)

A similar distinction pertains to communities of scholars and scientists associated with various domains of inquiry. Some are *open narrative communities*, in the sense that, like modern physics, they understand and support the epistemological importance of examining a domain from more than one narrative point of view. Others, like traditional societies, are *closed narrative*

communities in that they insist that there is only one legitimate way of looking at their domain, all others being taboo. Open narrative communities may be the rule in the natural sciences, but in the human sciences they are few and far between. Closed narrative communities, however, rarely exist in isolation but rather in opposition to one or more other narrative communities focused on the same empirical domain. These oppositions do not create situations like those featured in the competing-theories narrative of scientific progress.

In the human sciences, narrative pluralism – far from being a normal state of affairs – rarely exists except as a temporary truce among mortal enemies. The conflict endemic to these less successful fields of formal inquiry is idiosyncratic and inadequately understood. The Popper/Kuhn narrative of scientific development contributes little to comprehending these domains, where theories 'compete', but *not* in the traditional philosophy-of-science sense. Unlike natural scientists, social scientists never need to come up against reality's hard-edged recalcitrances. With rare exceptions, the links between social scientists' narrative beliefs and the world around them are conceptually tenuous. Rarely do their domains generate significant falsifiable predictions, making it virtually unknown for a narrative community in the human sciences to reach the point where, in Kuhn's words, it 'can no longer evade anomalies that subvert the existing tradition of scientific practice' (Kuhn [1962] 1970, p. 6). This freedom to forever evade reality when combined with monist beliefs and true-believer mentalities, leads to various narrative pathologies, of which four are especially important.

Narrative cleansing

Closed narrative communities typically live in open hostility towards 'alien' narratives. There exists a danger of radically misunderstanding the basis of this belligerence. The despised narratives rather than being 'competing' theories in the sense of the Popper/Kuhn story of scientific progress, are complementary theories in the sense of the narrative pluralism of twentieth-century physics. Advocates of closed knowledge narratives often publicly embrace an extreme and primitive form of philosophical idealism, whereby they declare that their conceptual framework, rather than offering a point of view on an empirical domain, determines the extent of that domain. This can be true even of narratives founded on a strictly materialist metaphysics. Behaviouralist psychologists maintain that psychological phenomena not visible through their conceptual lens do not really exist. Horton describes a similar mindset ('the magical world-view') common to traditional cultures:

> Since he ('the traditional thinker') can imagine no alternatives to his established system of concepts and words, the latter appear bound to reality in an absolute

> fashion. There is no way at all in which they can be seen as varying independently of the segments of reality they stand for. Hence they appear so integrally involved with their referents that any manipulation of the one self-evidently affects the other. (Horton 1971, p. 235)

Similarly, the behaviouralist claim to universality entails that when it changes its conceptual framework, as it does from time to time, then the domain of psychological phenomena changes also. Those parts and aspects of the domain that cannot be perceived from the current conceptual point of view are said not to exist.

Knowledge narratives deployed hegemonically block or discourage other knowledge narratives and thereby the scrutiny of other aspects of reality. It can be said that this mode of narrative deployment constitutes *antiknowledge*. Consider a hypothetical example. The narrative called 'Newtonian physics' could have been deployed (and perhaps was for a while) to block the study of elementary physical phenomena not covered by the Newtonian narrative, such as electromagnetism and the two nuclear forces. Physicists could have retreated into subjective idealism and refused to recognize as 'physical' those phenomena that cannot be embraced by the Newtonian narrative. They could have decreed that non-physical phenomena are precisely those phenomena that are incapable of being analysed with the Newtonian narrative. This kind of radical inversion of the scientific ethos and retreat into ultra-subjectivism is commonplace in the human sciences. For example, a standard economics graduate textbook informs its readers that '*noneconomic* problems are precisely those problems that are incapable of being analyzed with the *marginalist* paradigm' (Silberberg 1990, p. 2). This mindset, which promotes and protects *a priori* thinking and is endemic to today's 'mainstream' economics, anthropologists identify as characterizing traditional cultures. Their members, writes Evans-Pritchard, 'reason excellently in the idiom of their beliefs, but they cannot reason outside, or against their beliefs because they have no other idiom in which to express their thoughts' (cited by Horton 1971, p. 231). This 'absence of any awareness of alternatives', notes Horton (1971), 'makes for an absolute acceptance of the established theoretical tenets, and removes any possibility of questioning them' (p. 231).

Daniel Robinson, in his classic study of the history of psychology, describes an important example of anti-knowledge with a structure similar to the one noted by Horton in traditional cultures. Surveying the contemporary scene in American university psychology departments, Robinson notes that 'hardly a vestige' remains of the programme of experimental analysis of consciousness from earlier in the century:

> But observe the difference between this shift in emphasis or complete abandonment of interest and the changes that have occurred in physics and biology. We

> *do* have minds, we *are* conscious, and we *can* reflect upon our private experiences because we *have* them. Unlike phlogiston or the inheritance of acquired characteristics, these phenomena exist and are the most common in human experience. The absence of orthodox Wundtians or Titchenerians or Jamesians, therefore, cannot be attributed to the disappearance of their subjects. Rather, it is to be understood as the result of the inability of the accepted *method* of psychological inquiry to address these subjects. The contemporary psychologist, if only insensibly, has made a *metaphysical* commitment to a method and has, perforce, eliminated from the domain of significant issues those that cannot be embraced by that method. (Robinson 1986, p. 398)

Anyone coming from the natural sciences might wonder why social scientists expend so much time and energy 'defining' and redefining their disciplines. But this otherwise pointless activity is a natural adjunct of anti-pluralism, it being an easy shortcut to narrative cleansing. The anti-pluralist seeks to establish as off limits those areas and aspects of the empirical domain not visible from his or her single chosen conceptual vantage point. Laying down a definition that excludes phenomena that are invisible through that system works to establish a professional taboo against the extension of human knowledge and understanding to all the rest of that empirical domain. This technique of defining away the unwanted is common to many forms of anti-pluralism. Two notorious examples are the Nazis defining 'German' so as to exclude Germans who were Jewish, and America's founding fathers defining 'citizen' so as to exclude Americans of African descent.

A movement that began on the fringes of economics in the 1990s illustrates points raised in this section. The history of economics is diverse but nevertheless anathema to the idea of pluralism. Beginning with the French Physiocrats in the mid eighteenth-century, economists of all varieties have been inclined to believe that their approach to economic phenomena reveals, if not the whole truth, at least all of it that is worth knowing. It is with these broad conceptualizations, which are called 'schools', rather than with subject areas, that economists – like psychologists – form their primary professional identity. The assorted teachings and members of these narrative schools are labelled orthodox or heterodox depending on whether their school is the dominant one or not. Until very recently economists of all varieties have been comfortable with this quasi-theological scheme of things.

But from the 1960s on, neoclassical economists were increasingly successful at purging economics departments of economists who viewed economic reality through other conceptual lenses. This cleansing took place worldwide, a process that accelerated with the rise of neoliberalism, which justifies itself by appeal to the neoclassical narrative.

Traditionally non-neoclassical schools of economics have quarrelled among themselves hardly less than with the neoclassical school. But in the

mid-nineties, faced with near extinction, a peace movement began among these schools. Under the banner ICARE (Confederation of Associations for the Reform of Economics) (later changed to ICAPE, with 'Pluralism' substituted for 'Reform') it sought, declared its manifesto, 'to promote a new spirit of pluralism in economics, involving critical conversation and tolerant communication among different approaches'. But as these words show, this is a pluralism in the mode of a council of churches, a strategic pluralism rather than the epistemological pluralism of the natural sciences that this chapter endorses. Even so, ICAPE's conciliation campaign helped to break down among non-neoclassical economists the Popperian and Kuhnian tradition of viewing economics through the lens of competing narratives. This proved to be prophetic. In the summer of 2000 a group of French economics students circulated a petition that attracted attention from the media in France and subsequently from economists worldwide. The students labelled mainstream economics 'autistic' because its allegiance to a single narrative necessarily means that in the main it refuses to look at economic reality. The students called for 'a plurality of approaches adapted to the complexity of objects analysed'. Out of this appeal emerged the Post-Autistic Economics movement.

Fake pluralism

As a means of fending off criticism of its autism, of further concealing its ideological role (see below), of diverting calls for pluralism and, perhaps most of all, just as a pastime, economics's neoclassical mainstream plays a game of relaxing the assumptions. It loosens one or two assumptions around the edges of the theory and then does a bit of analysis. This is no better than when viewing a sculpture to lean to the left or to the right or kneel or stand tiptoed as a means of seeing another side of the work. Yet the whole mainstream project is now so infected with this methodological dilettantism that it seems necessary to spell out the difference between fake and real pluralism.

Even more than with a word, the meaning of a concept is its use. The meaning of a word depends upon the referent of the sentence, which, as Wittgenstein noted in his *Tractatus Logico-Philosophicus* ([1921] 1975, 2.01, 2.001, 2.02), is a 'state of affairs'; likewise the meaning of a concept depends on the framework in which it appears. For example, take something so simple and straightforward as the concept of economic growth defined in terms of GNP. When you transfer this concept from the neoclassical framework which views the economy as a closed system that includes the ecosystem ('land, labour and capital') to the conceptual framework of ecological economics which views the economy as an open subsystem of the ecosystem, this concept's meaning, in all its dimensions, changes fundamentally. It also

changes fundamentally when transferred from the masculinist neoclassical framework to a feminist economics that ascribes economic value to production not entering into market relations, for example family-provided nursing and child care. Each of these three conceptual frameworks, having the limited point of view common to all such creations, identify and describe a different 'state of affairs'. These examples illustrate two fundamental points: One must think from *inside* a conceptual system in order to: (1) grasp the meaning of its concepts, and (2) gain the vantage point that it offers on the world. It is only when you shift from one conceptual system to another, *like physicists do*, not when you relax some assumptions of one system, that you have real pluralism.

Narrative inversion

A knowledge narrative may become *invert*, meaning that instead of being used mainly as an instrument for explaining reality, its focus becomes itself. Turning away from the empirical phenomena that inspired it, it becomes transfixed with its own existence. This may take the form of formalism, where the narrative's empirical content is subordinated to the articulation of formal devices, where a language 'refers to the observer's logic but not to the subject' (Piaget 1973, p. 25), as in much recent economics and political science, or with an obsessive hermeneutic interest in 'reading' and interpreting the formative texts of the narrative, theology being the supreme example, but with psychoanalysis sometimes not far behind.

In subject areas where experimentation is difficult or impossible, mathematical models may have no connection with the concrete or empirical world. Symbols in the equations, instead of referring to measurable quantities, may be only imaginary placeholders, like 'Monopoly money' is imaginary money. In these cases – and they are especially common in economics – the models are merely playthings, 'being no more than a play of mathematical relations' (Piaget 1973, p. 25), referring only to those relations themselves, rather than to relations in the empirical world. The practitioners are not 'engaged in forging tools to arrange and measure actual facts so much as making a marvellous array of pretend-tools which would perform wonders if ever a set of facts should turn up in the right form' (Worswick 1972, p. 79). In economics the inversion often goes even further. There exist branches of economics that differ from branches of mathematics only in two respects: they are of no real mathematical interest and some of their axioms and terminology may have in the distant past been related to some empirical question. In these pursuits, so favoured by promotion and grant- and prize-giving committees, further assumptions are made willy-nilly to facilitate mathematical manipulation rather than from any desire to simulate reality. And by varying the

empirically empty assumptions, thereby generating an endless range of conceivable logical possibilities, a virtual infinity of 'models' can be fabricated, each generating one or more publications and all impregnable to empirical critique – a scientist's nightmare, but a careerist's dream.

Concealed ideologies

A conceptual system defines, at the exclusion of others, a point of view towards its object of inquiry. For the human sciences this fact poses a moral danger. Their conceptual systems relate to their objects of enquiry in two ways that invite them to play an ideological function as well as an epistemological one. Both of these relations are recursive. First, a social-science conceptual system can alter the objects of its inquiry by becoming part of the conceptual and belief apparatus through which humans define themselves, perceive others and make choices, thereby changing the structures and propensities of the human world. With the spread of mass higher education, this recursive phenomenon becomes more common, pervasive and profound. Second, unlike the natural sciences, the human sciences are ultimately a means from on high of preserving or reconstructing the basic realities that they study, these in total being the human project. Different conceptual systems present different sets of choices, real or imagined, to be chosen and acted upon by human populations at large. It can never be the case that each of these sets of choices will equally favour every group in society.

This means that, regardless of value judgements, it is the nature of all social theorizing, economics being no exception, to favour some groups in society over others, so that any attempt to block inquiry and analysis from multiple theoretical perspectives, namely anti-pluralism, is an ideological move.

Since Napoleon's popularization of 'ideology' in a derogative sense, many commentators have attached various meanings to the word, meanings inspired partly by shifting historical and social contexts, partly by a desire to make the phenomenon intelligible from more than one conceptual viewpoint and partly also, of course, by ideology. But the common presumption of these formulations has been that an ideology is necessarily manufactured and/or disseminated, consciously or unconsciously, with an ideological end in mind. The presumption of intent holds not only for the concept as developed in the negative sense by Marxist and non-Marxist writers, but also for Mannheim's neutralized concept which identifies ideology as a distinct type of cultural formation, functionally indispensable in non-traditional societies. But the preceding taxonomy of differences in conceptual systems shows that the element of intent is not a necessary condition for an economic theory to function as an ideology. Each conceptual system for a

given human field necessarily offers a different viewpoint of that field, and thereby suggests different possibilities for shaping, directing and organizing it. Consequently, if, for whatever reasons, one conceptual system's partial view is made the only view on offer, its influence on shaping human experience in a particular direction will be no less than if it had been designed to do so. *Where there exists a plurality of conceptual systems that illuminate different dimensions of a social object but the teaching of only one system is permitted, that system functions as an ideology.*

One must be careful here not to fall into a logical hole. Because every possible conceptual system can view its social object only from a particular point of view, it is self-defeating to equate ideology with systematized bias *vis-à-vis* the social realm, lest the social sciences are to be regarded as but a subcategory of ideology. The test of whether or not an economic theory is ideological is not its essence, or how and through whom it came to be, or who uses it. Instead the test is *how* it is used. A knife can be a deadly weapon or a tool for preparing the family dinner. Likewise an approach to economics can be an exercise in ideology or a tool for the advancement of understanding. A conceptual system regarding human affairs becomes an ideology when its partisans refuse to countenance the use of other systems as well, as when a group of economists refuse to teach their students how to view the economic realm from conceptual points of view other than the one that they favour. It is important to note here how the epistemological and ideological dimensions relate. *An economic theory becomes an ideology precisely at that moment when its partisans decide to curb the growth, and prevent the dissemination, of knowledge of how to see all those aspects of the economy that their approach leaves in the dark*. In economics, ideology comes about mostly through the way economics is taught, so that the primary agents of ideology in economics are not theorists and technical practitioners, but rather the teachers and, most especially, the authors of textbooks.

Summing Up

Even more than physics, modern medicine, where the general practitioner shifts freely between knowledge narratives, exemplifies the antithesis of the monistic approach to knowledge that characterizes traditional societies and many human sciences. The germ theory of disease along with psychosomatic, genetic and lifestyle explanations of disease are each a family of narratives, and between which the competent doctor shifts freely back and forth in seeking a true and full explanation of his or her patient's complaint. These narrative families have overlapping domains – for example, diet (not enough red wine and too much butter) and stress (not enough leisure and

too much aggro) contributing through biochemical processes to genetic susceptibility to heart disease. But there is no yearning or pressure in the community of medicine for a reduction of its many knowledge narratives to a master narrative, or for a unification of narratives as in physics. Instead the medical community understands that its multiplicity of narratives for explaining disease and its absence is needed to serve the complexity of medicine's empirical domain. Indeed, it is almost self-evident that the ill-health and good-health of the human organism are causally more complex than the fundamental properties of the physical universe, and, therefore, not open to narrative unification. It should be self-evident that this is even more true of the socio-economic realm.

If the human sciences are to be a constructive part of the human conversation, they must be willing to adjust the conceptual vantage points of their narratives both to fit changes in the topics of that conversation through time and to illuminate the diverse perspectives of its participants. Above all, the conceit that because one is a social scientist one is blessed with a privileged or God's-eye view of the human world must not be indulged. Richard Rorty's injunction to philosophers is no less apt for social scientists: 'to be rational is to be willing to refrain from … thinking that there is a special set of terms in which all contributions to the conversation should be put – and to be willing to pick up the jargon of the interlocutor rather than translating it into one's own' (Rorty 1980, p. 318). Epistemologically this is the recognition that a plurality of narratives enriches our understanding of any subdomain of the human project, that, whereas in the special case such narratives may be incompatible, in general they are complementary and their plurality is essential to the advancement of knowledge and the good health of society.

Note

1. It is not generally appreciated how much the popularity of Kuhn among people in the humanities is due to the satisfaction, sometimes glee, they take in what they *see as* his portrayal of the scientist as implicitly intellectually inferior to themselves. Your typical university literature lecturer, for example, thinks nothing of in a morning shifting through a whole range of gestalts (Marxist, Freudian, historical, New Criticism, deconstructionist, etc.) in interpreting a literary work.

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6
Three Arguments for Pluralism

J. E. KING

Is there a single correct alternative to neoclassical economics? The purpose of this short chapter is to suggest that there is not, and to show that this fact is increasingly recognized by eminent practitioners of several varieties of heterodox economic theory.[1]

For most mainstream economists, of course, there is only one way to do economics. It requires the construction of a model, collection of relevant data and subsequent testing. The model itself must be consistent with the fundamental principle of methodological individualism: that is to say, it must be based on the assumption of optimizing behaviour by rational agents. The tests must employ the most advanced econometric techniques rather than – or at least in addition to – descriptive statistics. For the defenders of mainstream economics these simple rules are what make it a science, which is envied and increasingly imitated by the practitioners of less favoured disciplines in the areas of management and social studies (Lazear 2000).

This is a seductive story, and it is widely believed, inside and outside economics (Fine 2000). When applied to the more disreputable branches of business 'thought' there is probably something to be said for it. If, however, it is taken as mandating the liquidation of sociology, political theory, social psychology and anthropology as autonomous bodies of scholarly knowledge it is obvious nonsense. As a methodological prescription for economics it is, to say the least, very questionable. In what follows I examine three counter-arguments, each making a different case for pluralism in economic thought. Two of the authors I cite are followers of the Cambridge economist Piero Sraffa, one is an institutionalist, and two are post-Keynesians

Apart from Pierangelo Garegnani, Heinz Kurz and Neri Salvadori are the two most prominent and tenacious defenders of modern-day 'classical' economics, by which they mean the study of the laws governing the pace of accumulation and the way in which output is distributed between the social classes, by means of a rigorous long-period analysis of a competitive capitalist economy. In a recent collection of essays they turn, rather surprisingly, to the

defence of pluralism. Economic reality, they note, is widely believed to be very complicated. The questions that economists ask are therefore inherently difficult, and it is unlikely that they have simple answers. Since no theory can consider all relevant factors in any particular economic context, there is a strong *prima facie* case for theoretical pluralism. Different theories will often be complementary rather than alternative, so that 'to seek dominance for one theory over all the others with the possible result that all the rival theories are extinguished amounts to advocating scientific regress'. To paraphrase Voltaire: in a subject as difficult as economics a state of doubt may not be very comfortable, but a state of certainty would be ridiculous (Kurz and Salvadori 2000, p. 237). Even classical theory has its limits. Kurz, in particular, has long acknowledged that it must be married to Keynesian macroeconomics if a comprehensive understanding of capitalist society is to be attained (Kurz 1990).

In his latest book, the well-known institutionalist Geoff Hodgson argues that the notion of a single, 'general' theory applicable to human behaviour in all societies, at all points in time, is a dangerous delusion that has led astray not only neoclassical economists but also many heterodox theorists. Failure to appreciate the need for historical specificity in economic theorizing has not only blighted the work of several generations of general equilibrium theorists, but also reduced the analytical achievements of some of their most vocal opponents, including Clarence Ayres, John Maynard Keynes and Joan Robinson. One does not have to agree with all the names on Hodgson's charge sheet (see King 2003) to accept the truth of his contention that:

> there are several problems with general theorizing in the social sciences. One is of analytical and computational intractability. Facing such computational limits, general theorists typically simplify their models, thus abandoning the generality of the theory. Another related problem with a general theory is that we are confined to broad principles governing all possible structures within the domain of analysis. In practice, a manageable theory has to confine itself to a relatively tiny subset of all possible structures. Furthermore, the cost of excessive generality is to miss out on key features common to a subset of phenomena. (Hodgson 2001, p. 16)

Hodgson's own proposal for the reconstruction of economic theory, putting the history back, is innately and profoundly pluralistic (2001, chapters 18–23).

The post-Keynesians Victoria Chick and Sheila Dow make an equally powerful, if largely implicit, case for pluralism in their penetrating analysis of what is implied by mathematical modelling in economics. Formalizing an argument is not, they suggest, an unambiguous improvement, as neoclassicals believe. On the contrary, it is a matter of costs and benefits. Formalism entails a particular view of the world, namely that it displays event regularities strong enough for it to approximate to a closed system. It also requires that the

meaning of economic terms be fixed rather than context-specific, and that these terms are separable rather than internally related. If these assumptions are rejected, classical or formal logic is inapplicable and Keynes's 'ordinary logic' may be needed in its place. Ordinary, commonsense or human logic 'generates knowledge which is imperfect, partial or vague', and provides 'reasoned grounds for belief which are nevertheless not conclusively demonstrable' (Chick and Dow 2001, p. 711, 714). Economic statements may therefore be true in some historical and institutional circumstances, but false in others. Here Chick and Dow share common ground with Hodgson, since their argument casts doubt on

> the possibility of finding immutable laws applicable to, say, feudalism and capitalism alike, or even to capitalism in various stages of its development. From this perspective, a theory can be 'right' at one time and become 'wrong' (more accurately, outdated) at another. The notion of imbuing a closed theoretical system with meaning is thus not an objective procedure; it requires the exercise of judgement. (Chick and Dow 2001, p. 709)

In this way their critique of formalism leads them to pluralism, not just in substantive theory but also in method, since Keynes's ordinary logic 'supports a methodology which encompasses a range of methods in order to build up knowledge' (Chick and Dow 2001, p. 719; cf. Dow 1997).

Note that Chick and Dow do not completely deny the legitimacy of formalism in economics, in all circumstances, for all purposes. On the contrary: some problems lend themselves to closed-system thinking and cry out for precise, formal solutions. They argue only that it is a serious mistake to suppose that all economic problems are of this type. They would certainly disagree with Kurz and Salvadori on the size of the contribution that can be expected from formal reasoning. The two Sraffians Kurz and Salvadori follow Garegnani in placing great emphasis on the so-called 'core' of classical economic theory, which consists of propositions that can be established with certainty about the relationships between inputs, outputs, prices and distributional variables in a closed economic system where the same rate of profit is paid in all industries (Kurz and Salvadori 1995). The two post-Keynesians, Chick and Dow, see very little point in exercises of this type, while Hodgson, the proponent of institutional economics, seems to deny their validity altogether. Certainly he shows no sympathy for those self-proclaimed institutionalists who use prey–predator models, chaos theory and similar sophisticated mathematical tools derived from the biological sciences.

If pluralism does not (quite) rule out formalism, what does it exclude? Unqualified relativism, for one thing; logical incoherence, for another. Hodgson is the most outspoken in denying that 'anything goes', and the most sternly critical of postmodernist claims in this regard:

> An acceptable policy of pluralism concerns the policy of institutions towards the funding and nurturing of science. Such a policy involves 'pluralism in the academy'. But it would not extend to the individual practices of science itself. This confusion, between encouraging contradictory ideas in the academy and encouraging them in our own heads, is widespread in post-modernism. … There is much to be said for tolerance of many and even antagonistic scientific research programmes within an academic discipline or university. But we should not tolerate the existence of inconsistent ideas within our own heads. The policy towards science must be pluralistic and tolerant, but science itself must be intolerant of what it regards as falsehood. … Any failure of social science to erect an adequate and coherent general theory is not rectified by applauding incoherence. (Hodgson 2001, p. 35)

Horses for courses, as Geoff Harcourt has always put it (see Comim 2000), but they must each have four legs and a jockey and proceed anti-clockwise around the course.

Sheila Dow has also defended the principle of consistency against its postmodernist and constructivist opponents. Thus she proposes that a clear distinction be drawn between 'pure' and 'modified' pluralism. To be a pure pluralist entails 'a refusal to appraise methodologies and thus also (a refusal) to advocate one method rather than a plurality'. This, she maintains, offers 'no scope for scientific (or indeed any) discourse'. According to modified pluralism, however,

> no one system of knowledge can claim to have captured reality; each is partial, reflecting one vision of reality. Each school can support its approach to knowledge with reason while recognizing the legitimacy of alternative approaches. … World-view and theory of knowledge cannot be eradicated; yet recognition of differences at this level allows for more reasoned debate over appraisal criteria and analysis of different methodologies. (Dow 1996, pp. 45–6)

Kurz and Salvadori also insist on the need for logical consistency in economic theorizing. For them this criterion is enough to rule neoclassical analysis out of the race, since its conception of capital is fundamentally flawed. If the 'principle of substitution' is central to mainstream theory, they argue, it should be applied in a logically consistent manner. In the long period, this means that an increase in the price of one input induces a decrease in the quantity of that input per unit of output. 'All propositions of the theory can be traced back to this basic idea. If it is not true in general, the theory appears to be in trouble' (Kurz and Salvadori 2000, p. 238). But it has been known since the mid-1960s that it is, in general, false when applied to the collection of heterogeneous commodities known as 'capital'. From a quite different perspective the post-Keynesian Paul Davidson has criticized what he terms the 'babel' of New Keynesian economics, in which market imperfections that prevent downward price and wage flexibility are

denounced as the fundamental cause of involuntary unemployment while in the same breath a falling price level ('deflation') is decried as a serious macroeconomic evil (Davidson 1999; compare Solow 1997 and Taylor 1997 for graphic examples of this incoherence). Horses for courses, once again, but all four legs must be pointing in the same direction.

No single case for pluralism in economics emerges from this brief discussion, and indeed it would be a cause for concern if one had. Similarly, there is no single version of 'unscientific' heterodox economics to stand in opposition to mainstream economic 'science'. Sraffians, institutionalists and post-Keynesians do very different things, often in radically different ways – as do Marxists, social economists, feminists, greenies and other schools of political economy. As Abbie Hoffman is supposed to have said, in the course of the 1968 Chicago conspiracy trial: 'Conspire? We couldn't agree on lunch.' But they did agree to keep on talking, which in the last resort is what pluralism is all about.

Note

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7
Economics as Ideology

PETER SÖDERBAUM

Mainstream neoclassical economics – and no other economics – is taught at university departments of economics and at business schools in all parts of the world. Its ideal is to build on one paradigm (theoretical perspective) and to extend this perspective in different directions to cover international economics, environmental economics, health economics and so on. The hope of neoclassical economists is furthermore to offer a useful paradigm that is close to the philosophy behind physics, chemistry and other natural sciences. Positivism in the sense of objectivity, value neutrality, testing of hypotheses and, whenever possible, mathematical presentation are some of the features of the neoclassical approach. A large number of scholars have vested interests in the monopoly position of the neoclassical paradigm, and since the 1970s, there is even a Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel for extraordinary achievements.

Gunnar Myrdal is one of those who have received the Nobel Prize. Not unexpectedly, the Nobel committee emphasized his achievements within neoclassical economics, but unlike other neoclassical economists at one stage in his career he had openly declared his sympathies for institutional economics. He argued that the study of problems related to poverty, health and environment in developing countries and elsewhere has to be based on an interdisciplinary perspective. Myrdal also took an interest in theory of science issues and argued that it is an illusion that economics can be value-free and neutral. 'Values are always with us' in social science research, he wrote (Myrdal 1978). He pointed out that we have interests in choosing one problem area for our studies rather than another and values are involved when making a choice among possible theories, methods and ways of presenting results. Scientific criteria play a role in making all these choices, but so also do other values that make up the total ideological orientation of the scholar.

The 'fact' that ideology is also present means that the 'one-paradigm position' in departments of economics becomes untenable. Limiting

economics to one paradigm means that one ideological orientation is emphasized at the expense of all others. This position is not compatible with normal ideas of democracy. Departments of economics should avoid the role of being political propaganda centres. The ideological diversity in a democratic society would be better reflected by the inclusion of more than one paradigm as part of a pluralistic strategy. Furthermore, one specific paradigm, such as the neoclassical one, may perform well in relation to some fields of study while being more of a problem in relation to other fields.

In defence of the neoclassical monopoly position at departments of economics, it could perhaps be argued that there are other social sciences such as economic history, political science, business economics and sociology that are based on other paradigms than the neoclassical one. While this is true, my point here is that the monopoly of the neoclassical paradigm at departments of economics has a considerable impact on the understanding of economics among major actors in society. If their mental maps are largely influenced by neoclassical economics, and if some categories of problems cannot be dealt with successfully within the neoclassical perspective, then we have a problem. I will argue that neoclassical economics is not enough – and is even part of the problems faced – in relation to present environmental and development issues.

In this chapter the one-paradigm idea of economics will be questioned. Those acquainted with the history of economic ideas – a subject nowadays often avoided at departments of economics – know that there have been many currents over the years. Among alternatives to neoclassical theory, institutionalism will be emphasized here. I believe, furthermore, that comparing different paradigms, for instance neoclassical theory with institutionalism, is an important way of learning. Such comparisons make it possible to illuminate not only differences in some narrow scientific sense but also the ideological tendencies connected with each paradigm.

On the Meaning of Paradigm, Ideology, Pluralism and Democracy

I have already used concepts that may be unfamiliar to some students. 'Paradigm' here refers to a 'theoretical perspective' within a discipline or at an interdisciplinary level. Neoclassical economics with its microeconomics and macroeconomics is a good example of a relatively clear-cut paradigm. Microeconomics refers to individuals as consumers maximizing the utility of alternative bundles of commodities within their monetary budget constraints and to business companies maximizing profits. Individuals and firms interact in markets for commodities and factors of production. Markets are

understood in terms of the forces of supply and demand. Macroeconomics refers to the whole national economy where the state may influence all consumers and companies, for instance through monetary and financial policy.

As a paradigm, institutional theory may be less clear-cut. It is more relevant to speak of different versions of institutionalism. According to the one emphasized here, individuals and organizations are understood as 'actors' in the economy. Individuals are guided by their 'ideological orientation' and the same is true of organizations. Neoclassical Economic Man (NEM) is replaced by a Political Economic Person (PEP), that is, an individual with many roles rather than exclusively that of consumer. To understand the individual in the economy, roles such as 'professional', 'citizen' and 'parent' are also judged to be relevant. An organization is similarly understood as a Political Economic Organization (PEO). Business companies are of course important players in the economy but so are government agencies, municipalities, environmental organizations, churches and universities. Individuals and organizations alike are understood as actors in a political sense. They all can influence the dialogue about the future of our societies, for instance on issues related to taxation, environment and development more generally. They also engage in more limited roles in relation to markets of various kinds.

The term 'ideology' is used here not in a derogatory sense but in a broad sense as 'means–ends philosophy'. Ideology is based on beliefs about 'progress' at the level of society, of organizations and for the individual herself. Normally, each actor has a positive attitude to her 'ideological orientation'. Established political ideologies such as liberalism or socialism are certainly included in this definition, but so also are 'issue-related ideologies' such as 'business ideologies', 'healthcare ideologies', 'transportation ideologies' or 'environmental ideologies'. According to the ideological orientation of some business actors, only monetary profits and other monetary indicators count; other business actors may point to the importance of a number of non-monetary dimensions, for instance the measurement of social and environmental performance. One healthcare ideology may emphasize centralization of healthcare functions while another may point to the advantages of decentralization. One transportation ideology emphasizes time savings in transport while another lays stress on the avoidance of negative impacts such as environmental pollution and traffic accidents. A compromise between different objectives is of course as 'ideological' as 'single-objective' ideologies.

Ideologies are seldom reducible to simple mathematical equations. To allow for complexity, the term 'ideological orientation' is to be preferred in referring to a guiding principle of individuals and organizations. An ideological orientation is typically fragmentary rather than complete; it is

uncertain and subject to reconsideration (as part of public and private debate, for instance) rather than certain. The ideological orientation of an actor still serves as a guide with respect to the direction of decision-making and behaviour.

'Pluralism' stands for a belief that competition between paradigms and ideologies is good for society. Each actor tends to believe in one paradigm and ideological orientation more than others. This belief can be of a 'fundamentalist' kind or combined with an open attitude in relation to other theoretical perspectives and ideologies. In the latter case of a pluralistic attitude, there is a willingness to listen and learn from advocates of other perspectives.

The opposite of pluralism is monism. In natural science, there is a tendency to believe in one paradigm at a time and then consider the possibility of paradigm shift in a Kuhnian sense (Kuhn 1970). One paradigm may be abandoned when another is found to be more compatible with the results of empirical experiments etcetera. Neoclassical economists tend to be monists in this sense. But today even some natural scientists are open to a complementary role for different paradigms in understanding specific phenomena. Paradigms that are incompatible within the current state of knowledge may each contribute to the understanding of a phenomenon, for instance light. In the case of economics, which is our main interest here, 'paradigm coexistence' (Söderbaum 2000, pp. 29–30) appears to be a much more relevant idea than paradigm shift. The mistake by neoclassical economists is not that they believe in neoclassical economics but rather that they believe in the exclusion of other paradigms. While excluding theoretical perspectives, they at the same time exclude ideological orientations (other than those connected with neoclassical economics) and thereby reduce the possibilities of a constructive dialogue in society.

Most people who follow or participate in the development dialogue globally, regionally or locally understand that mainstream ideas about progress in society are challenged by an increasing number of actors. Simplistic ideas about economic growth as the solution to every problem are no longer convincing (see Hamilton 2003). In relation to poverty, health and environment, there are a number of unsustainable trends. And it is no longer possible to 'solve' problems in the home region; we all depend on each other at a global level. In this situation of difficult choices we cannot rely only on experts but need a broad dialogue based on the emergence of ideas from a well-functioning democracy.

The meaning of 'democracy' is a big subject and I will here only point to one aspect that appears to be crucial. Each actor in a democratic society may believe in one ideological orientation more than others but should respect the existence of other ideological orientations so long as they do not

contradict democracy itself. In attempts to solve present problems, we need to be informed not only by experts in some scientific sense but also by clearly articulated ideological orientations. What is the meaning of sustainable development as an alternative to the previous emphasis on economic growth in GNP terms, for instance?

The Ideology of Neoclassical Economics

My statement above that neoclassical economics is not only science but also ideology needs to be elaborated a little. While classical economists such as Adam Smith and David Ricardo always referred to 'political economy' in a broad sense, the neoclassical project starting around 1860 can be seen as an attempt to separate 'economics' from 'politics' and present a 'pure' economics. Following Myrdal, it is argued here that the belief in a value-free economics is an illusion and that the neoclassical project in this respect was a failure. It is about time to return to the vocabulary of 'political economics'.

Neoclassical Economic Man assumptions are specific in ideological terms. Some roles and relationships are emphasized while others are excluded. Human beings are regarded as consumers and wage-earners, thereby limited to market relationships. The idea of utility maximization excludes other forms of ethics. Building on Political Economic Person (PEP) assumptions instead means that you do not deny the political and ideological aspects of human behaviour. Each individual is acting in many roles and guided by her ideological orientation. This orientation may be built on utilitarianism or alternatively on other kinds of social and environmental ethics. Economists have no right to select one ethics as the 'correct' one for purposes of economic analysis. Reference to 'ideological orientation' as a variable furthermore suggests that the neoclassical focus on commodities and markets may legitimize a kind of 'market ideology' and 'consumerism' that for many of us appears too limited, if not dangerous to society. In relation to the current debate about sustainable development, something else is needed.

Neoclassical assumptions about profit-maximizing firms are similarly specific in ideological terms. As already argued, the focus on one kind of organization, that is, the firm, appears strange in relation to the present dialogue about development and a well-functioning economy where many kinds of organizations are involved. Excluding civil society organizations such as churches and universities, and public organizations at the national and municipal levels, cannot easily be defended if one wishes to understand the functioning of an economy.

For business organizations (and other organizations), monetary performance is of course important but so also is non-monetary performance.

Environmental Management Systems are a case in point. Stakeholder models of business organizations suggest that rather than just maximization of profits, conflicts of interest and power relationships are also relevant. According to Political Economic Organization (PEO) assumptions, the organization is furthermore 'polycentric' in the sense that each individual connected with the organization is an actor with her specific ideological orientation, which to some extent may depart from that of the leadership of the organization. Such tensions are not only problematic in a negative sense but may well also be a source of learning and reconsideration of the 'core values' or 'business concept' of the organization.

Each individual (or organization) interacts over time with her context. In neoclassical theory, only a market context is taken into account. As part of institutional theory, a multifaceted context is considered which is social, cultural, institutional (markets included), physical, man-made and ecological. Behaviour is guided by ideological orientation and may be habitual or a result of conscious choice, that is, decision-making. In neoclassical micro-economics, decisions are based on some optimization principle (maximum utility or maximum profits) devoid of other ethical considerations. Institutional theory does not deny egoism. Any healthy individual will consider her own best interests. But the individual is at the same time part of a number of 'we-contexts', suggesting that the concern for others is a normal feature in her ideological orientation. Amitai Etzioni has proposed an 'I and We Paradigm' (Etzioni 1988) where egoistic tendencies and concern for others are combined rather than mutually exclusive. Decision-making as part of the institutional perspective is furthermore not exclusively understood as 'optimization' but rather as a 'matching' process between the ideological orientation of each decision maker and the expected impact profile of each alternative. In this way not only quantitative but also qualitative and visual aspects of impacts become relevant.

In neoclassical theory, 'markets' are presented in terms of the 'forces' of supply and demand. Contrary to this impersonal view, actors in the market-place can be made more visible as part of an institutional perspective. Each market actor is guided by an ideological orientation (according to PEP and PEO assumptions). Fairness and other ethical aspects may be involved in the relationship between two market actors. Each actor may furthermore be part of different networks of cooperation, and the relationship between two actors may be cooperative as well as competitive. In business actor network theory, trust is furthermore an important factor (Ford 1990). Each market relationship has a history and each market actor is embedded in a social and institutional context (Söderbaum 2002).

In neoclassical theory, 'progress' is assumed to be a matter of GNP growth at the national level, increased profits for organizations and utility

maximization for individuals. The alternative here is an ideologically open attitude to progress at all levels. Some actors are concerned about present environmental, social and institutional problems in society and may, for example, refer to a specific definition of sustainable development as their idea of progress. At the levels of organizations and individuals there may similarly be many ideas of progress rather than one. Monetary and material aspects may be important, but how they relate to non-monetary dimensions and ethical considerations has to be a subject of study rather than assumed away.

Cost–benefit Analysis and Democracy as a Case

Neoclassical economists claim to offer clear ideas about 'efficient resource allocation' in the economy. Reference is made to 'welfare theory' and 'applied welfare economics'. The practical instrument or method used is cost-benefit analysis (CBA). Just as the firm considers monetary costs and revenues in its attempts to estimate profits from alternative investments, a similar approach is advocated by neoclassical economists at the national level, that is, for society as a whole. What are the costs and benefits in monetary terms of alternative investments in infrastructure such as roads, airports and dams? Not only constructions costs, maintenance costs and benefits directly associated with the project are identified, but also all impacts affecting economic subjects in a nation. The market 'value' of single impacts is estimated with reference to existing markets, prices at comparable real markets or through fictitious markets, using the 'willingness-to-pay' approach. All costs and benefits connected with a project are then aggregated in monetary terms to a 'present value' or 'benefit–cost ratio'.

The claim that neoclassical economics has a clear idea about correct resource allocation as demonstrated in cost–benefit analyses is correct. But this at the same time reveals a problem. 'Ideology' was previously defined as a means–ends philosophy, and 'ideological orientation' was suggested as a guiding principle for different actors. CBA claims to rank mutually exclusive investment proposals, but it builds on a specific idea of what is good and best for society in different decision situations and therefore qualifies as an ideology. As an ideology, CBA is more precise than most other ideologies, being a market ideology of a very specific kind. Not only is it based on the belief that society should be understood in market terms, and that other frames of reference of a conceptual or ethical kind are thereby automatically excluded, but it is close to the controversial 'economic growth' ideology (Johansen 1977). Should science dictate the correct ideology for politicians, citizens and actors of different kinds? The answer is no. Some other role for science is called for.

What then are the alternatives to CBA? While CBA is a 'highly aggregated' approach, one can think of 'highly disaggregated' approaches as alternatives (Table 7.1). CBA is also 'ideologically closed', whereas there are alternatives that are ideologically more open. This means that there are four possible categories of approach.

Table 7.1 Categories of approaches to decision-making

	Ideologically closed	Ideologically open
Highly aggregated	I	II
Highly disaggregated	III	IV

It is clear that CBA belongs to category I (highly aggregated, ideologically closed). Ezra Mishan, himself a textbook writer in economics (1971), argues that CBA can only be used if there is a consensus in society about the approach to valuation and progress of CBA (Mishan 1980). If there is no such consensus – and Mishan points especially to disagreements about how to value environmental impacts – the CBA method is no longer useful. If there is a diversity of ideological orientations in society then as previously indicated CBA is no longer compatible with our ideas about democracy. In a democratic society, each citizen or actor is encouraged to form her own ideological orientation so long as it does not contradict democracy itself. This includes a right to have an opinion about what is good for society and how to relate different policy or project impacts to each other (Söderbaum 2001). The CBA ideology of trading impacts against each other in one-dimensional terms at 'correct' prices is just one among many possibilities.

In its final report, the World Commission on Dams (2000) expressed scepticism about the use of CBA to legitimize the construction of large dams in various parts of the world. The issue of resettlement of thousands of individuals, in some cases tribal people who have been accustomed to a specific way of living and a specific context, raises issues of ethics that cannot be solved by simplistic cost–benefit calculation. The World Commission on Dams pointed in the direction of Multicriteria Approaches (MCA) that are more open-ended (category II and IV in Table 7.1) but there are other possibilities as well, for instance Positional Analysis (PA). The latter approach (category IV in Table 7.1) starts with PEP assumptions, and uses 'systems thinking' and 'positional thinking' with the purpose of 'illuminating' a decision situation in relation to different and possibly relevant ideological orientations (Söderbaum 2000).

Conclusions and Recommendations for Education in Economics

Today, there are many kinds of heterodox economics. Those of us who depart from the neoclassical mainstream do it in many cases for ideological reasons rather than for scientific reasons in a narrow sense. We do not like the ideology of neoclassical economics and tend to regard the monopoly positions of this specific ideology as an essential part of the problems that exist in modern society. Neoclassical economics is reasonably coherent in logical terms and may be useful for some purposes. CBA belongs, however, to the weakest elements of mainstream theory, though other parts can be considered within the scope of a pluralistic attitude where the parallel existence of competing paradigms is accepted.

In this chapter, the 'political' aspect of economics has been stressed. As already mentioned, 'political economics' was the term used until 1870 when the ideas of a pure economics became dominant. This emphasis on ideology and the political aspects may appear strange to some students. But if ideology is involved and if various actors (such as transnational corporations, for instance) play a political role in the economy, as I have argued, then this language has to be reintroduced in textbooks and courses in economics. The idea of *homo politicus* or a Political Economic Person has been supported in a number of articles recently (Jakubowski 1999, 2000; Faber et al. 2002; Siebenhüner 2000). A textbook in organizational theory discusses 'ideology' in one chapter and 'power' in another (Jackson and Carter 2000). I can understand if some professors and perhaps even students hesitate because the expert position of economists in an extreme sense is in danger if you admit that ideology exists in economics. On the other hand, this is a matter of being honest and it is quite possible that a different kind of expertness will restore the legitimacy of economics as a discipline in a democratic society.

In their education all students of economics are acquainted with neoclassical theory, but they often know less about the history of economic ideas and about present alternatives to the neoclassical paradigm. Considering the present crisis of neoclassical economics, there are many good reasons to reintroduce courses about the early economists and their ideas and to point to the tensions that have always existed between different schools of thought. Students should have a chance to study institutional microeconomics, ecological economics and other alternative approaches. This means also that the professors of economics must broaden their competence. Present tendencies towards conceptual and ideological closure have to be counteracted.

The importance of the 'models' we use in understanding individuals, organizations, decision-making, markets, progress, etc. has been stressed

throughout this chapter. Textbook writers are in a powerful position to influence future generations of economists though it is a difficult task to open up economics when one generation after another has become accustomed to only one idea of what 'correct' economics is. The current argument about the role of 'models' or 'schemes of interpretation' at the same time furnishes reasons for optimism. While neoclassical economists tend to protect their monopoly in departments of economics, a lot happens in other arenas. Socio-economics, social economics, development economics, ecological economics, institutional economics, political economics, interdisciplinary economics – many labels are used that indicate the existence of alternatives. 'Models' of various phenomena play a role in an ongoing change process. As part of neoclassical theory, individuals can be regarded as 'consumers' if you are in favour of a consumer society and see globalization as an extension of market forces to make people happier in all parts of the world. If you instead want to emphasize the role of the individual as citizen (and indeed consider all roles, the one of being consumer included) with democracy rather than markets as the main ideological priority, then some other understanding will follow.

An understanding of the ideological and political character of economics furthermore means that the future of economics is not exclusively a concern for professors and students at departments of economics. While university leaders and university professors are all responsible, in addition politicians, professionals of all kinds and citizens can contribute constructively. In relation to present environmental problems, and sustainability issues more generally, the European Union and national governments have a role. The author recently participated in a workshop on 'sustainability economics' and 'international institutions for sustainability' at a respected German economics research institute (Deutsches Institut für Wirtschaftsforschung, DIW). At a meeting with the World Council of Churches, the World Bank and the International Monetary Fund in Geneva, I was similarly impressed by the position taken, roles played and documents produced by the representatives of churches.

I hope that environmental organizations and civil society organizations also increasingly will understand the role of neoclassical economics as a barrier to new thinking in the development dialogue. Behind the present activities of such criticized institutions as the World Trade Organization, the World Bank, the IMF and even in some respects the European Union is a 'mental map' of influential actors very much connected with neoclassical economics. Only with a pluralistic attitude allowing for more than one paradigm will a better world be possible. A degree of competition rather than monopoly and protectionism seems to be good for society in a number of contexts, universities not excluded.

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8
Metaphor and Pluralism

GEOFFREY HODGSON[1]

'Complexity' is the word of the moment. Books about a new 'emerging science' based on a recognition of complexity have now made their way into the best-seller lists. Such populist outcomes should make us cautious. Nevertheless, there is some substance in the claims made by those who see an emergent paradigm in the making. There are several methodological implications of this shift. One of central importance is the breakdown of the reductionist project in sciences addressing complex systems, such as economics and biology.[2] Two further implications follow immediately: first, the necessity for a much more self-conscious and open use of metaphor in the analysis of complex systems, and, second, the recognition that there can be no complete and final explanation and that we shall always be faced by a plurality of competing theories.

The first section of this chapter discusses the breakdown of reductionism, with particular attention to economics and biology. The second section asserts the need for the self-conscious use of metaphor in science and argues that metaphor has always, in fact, been constitutive for science and has a subterranean presence in economic theory. The third and final section discusses the implications for theoretical pluralism.

The Limits to Reductionism

Inspired in particular by classical physics and its apparent analytical and practical successes, orthodox economists have long been labouring under a reductionist research programme. The complexities of economic systems have been addressed by attempting to build up a composite picture from atomistic, individual units, just as the particle forms the elemental unit in Newtonian mechanics. Accordingly, the attempt has been to explain the whole through its analytical reduction to its presumed microfoundations and component parts.

Many economists pay lip service to methodological individualism and the view that social wholes should be explained solely in terms of the individuals comprising them (Hodgson 1988, Chapter 3). Yet as Lars Udéhn (1987) points out, the complexity of social phenomena means that this methodological practice has never been successfully applied except in trivial cases, and is bound to be unsuccessful in any complex social system.

Whilst mainstream economic theory has long been engaged in an attempt to place economics on secure and individualistic microfoundations, it was quickly realized that the potential diversity among individuals threatened the feasibility of this project. Many types of interaction between the individuals have to be ignored to make the analysis tractable. Indeed, it was not easy to develop a composite picture from a diversity of types of individual agent.

Even with the standard assumptions of rational behaviour, and its drastic psychological and epistemological limitations, severe difficulties are faced. As Arrow (1986, p. S388) has been led to declare: 'In the aggregate, the hypothesis of rational behaviour has in general no implications.' Consequently, it is widely assumed that all individuals have the same utility function. Among other things this denies the possibility of 'gains from trade arising from individual differences' (p. S390).

Fabrizio Coricelli and Giovanni Dosi (1988, p. 126) argue that 'the project of building dynamic models with economic content and descriptive power by relying solely on the basic principles of rationality and perfect competition through the market process has generally failed'. Attempts to base macroeconomics on neoclassical microfoundations involve faith in the 'invisible hand' and in the substantive capabilities of individuals to calculate endlessly and make supremely rational choices. Yet the results of this theoretical endeavour show no more than a very crippled hand, incapable of orderly systemic coordination even in relatively simple models:

> Moreover, note that these results are obtained despite an increasing attribution of rational competence and information processing power to individual agents. Certainly ... the attempt to 'explain' macroeconomics solely on the basis of some kind of 'hyper-rationality' of the agents ... and the (pre-analytical) fundamentals of the economy (i.e. given technology and tastes) has failed. (Coricelli and Dosi 1988, p. 136)

Hence it is no exaggeration to say that in economics the microfoundations enterprise has effectively collapsed, and for reasons well known to and understood by the leading theorists of the genre.[3] The gravity of the present crisis for mainstream economics can be illustrated by considering a few selected topics.[4]

First, theoretical work in game theory and elsewhere has raised questions about the very meaning of 'hard core' notions such as rationality. Yanis

Varoufakis (1990) surveys some of the recent results concerning the problems of rational decision-making in the circumstances where a limited number of other actors are believed to be capable of 'irrational' acts. Such 'irrationality' need not stem from stupidity; it is sufficient to consider the possibilities that rational actors may have incomplete information, limited computational capacities, slight misperceptions of reality, or doubts concerning the attributes of their adversaries. Agents do not have to be substantially irrational for irrationality to matter. Irrational behaviour may emerge simply where some people are uncertain that everybody else is rational.

Second, the intrusion of chaos theory into economics has put paid to the general idea that economics can proceed simply on the criterion of 'correct predictions'. With non-linear models, outcomes are oversensitive to initial conditions and thereby reliable predictions are impossible to make concerning any extended period. In particular, chaos theory has confounded the rational expectations theorists by showing that even where most agents know the basic structure of the economic model, in general they cannot derive reliable predictions of outcomes and thereby form any meaningful 'rational expectations' of the future (Grandmont 1986).

Third, recent research into the problems of the uniqueness and stability of general equilibria have shown that they may be indeterminate and unstable unless very strong assumptions are made, such as the supposition that society as a whole behaves as if it were a single individual. Essentially, this demolishes the entire microfoundations project. Facing such profound problems, Alan Kirman (1992, p. 118) writes: 'there is no plausible formal justification for the assumption that the aggregate of individuals, even maximizers, acts itself like an individual maximizer'. He concludes: 'If we are to progress further we may well be forced to theorize in terms of groups who have collectively coherent behaviour. ... The idea that we should start at the level of the isolated individual is one which we may well have to abandon' (Kirman 1989, p. 138). The theoretical implications of these uniqueness and stability results for general equilibrium theory are devastating. The indeterminacy and instability results produced by contemporary theory lead to the conclusion that an economy made up simply of self-contained individual agents has not structure enough to survive.

Typically, the textbook macroeconomics that is spun out of neoclassical theory goes well beyond the rigours of general equilibrium theory, to make bold and general claims concerning the relationship between wages and unemployment, and inflation and the money supply. Only the more honest and careful neoclassical theorists have questioned such macroeconomic derivations from microeconomic assumptions. For instance, Arrow (1986, p. S386) states that he knows 'of no serious derivation of the demand for money from a rational optimization'.

The ubiquitous problem of analytical intractability

As in other sciences, a strong reductionist tradition still survives in biology. In the nineteenth century, after the publication of Charles Darwin's *Origin of Species*, there was a prominent tendency on behalf of his followers to assume that explanations of social phenomena were reducible to biological terms. One of the best-known equivalent cases in the twentieth century is the work of Richard Dawkins (1976) where he maintains that the behaviours of organisms, groups and whole species can be largely explained in terms of their genes. Similarly, sociobiologists such as Edward Wilson (1975) attempt to explain the social behaviour of animals and humans in terms of the constituent genes, without sufficient recognition of the explanatory autonomy of social culture. It may be the case that biology is of relevance for the study of social phenomena (Hirst and Woolley 1982) but the extreme view that they can be understood wholly or largely in biological terms has been rightly challenged.

Yet biological reductionism has unmanageable computational problems. Consider the prediction of evolution with multiple loci or alleles: 'even the simplest multi-locus case of two alleles at each of two loci is analytically intractable. This should not be surprising: the problem of dimensionality nine (there are nine possible genotypes, with independently specifiable fitness parameters) is already more complicated than the three-body problem of classical mechanics' (Wimsatt 1980, p. 223). Like the three-body problem, this biological computation has been solved for a variety of special cases (Roughgarden 1979, pp. 111–33) but has not been solved in general. Similar problems of intractability arise from the Schrödinger equations for subatomic particles.

Consider the apparently simple case of the three-body problem in mechanics. While this problem has been solved for two bodies, the differential equations that result from applying these laws to three bodies are so complicated that a general solution has not been found. Instead, partial solutions have been achieved by resorting to approximations or constraints of various kinds, such as the assumption that one body has negligible mass (Stewart 1989, pp. 66–72). Hence mathematical solutions cannot be found to configurations of this very first level of complexity, involving just three bodies. This indicates the operational limits of reductionism, even in mechanics. The hope that predictions can be made with such a simple three-body system is in vain. There is here no consolation for the biological or economic reductionist who aims to predict by breaking down all complex phenomena to the interactive behaviour of atomistic or individual parts.

Chaos theorists have shown that in non-linear systems, tiny changes in crucial parameters can lead to dramatic consequences (Gleick 1988; Stewart

1989). The result is not simply to make prediction difficult or impossible; there are serious implications for the notion of reductive explanation in science. We cannot with absolute confidence associate a given outcome with a given set of initial conditions, because we can never be sure that the computations traced out from those initial conditions are precise enough, and that the initial conditions themselves have been defined with sufficient precision. Hence in chaos theory the very notion of explanation of a phenomenon by reference to a system and its initial conditions is challenged.

As leading mathematicians of chaos have themselves proclaimed, chaos theory 'brings a new challenge to the reductionist view that a system can be understood by breaking it down and studying each piece' (Crutchfield et al. 1986, p. 48). The impact of chaos theory for science as a whole is likely to be profound. Not only is the common obsession with precise prediction confounded; the whole atomistic tradition in science of attempting to reduce each phenomenon to its component parts is placed into question.

However, this does not mean that such non-linear equations relating to a lower level of analysis are worthless. Although they may be of limited computational or predictive use, they retain some explanatory power. Furthermore, as noted later below, chaotic systems do exhibit some kind of order about which deductions may be drawn.

The limitations and biases of the counter-strategies

Strategies have been devised to attempt to deal with the general analytical problems associated with complex systems. For instance, Herbert Simon (1968) has examined 'the hypothesis of near decomposability' through which it is assumed that a complex system can be decomposed into a set of subsystems. For this to be feasible, all strong interactions must be contained within the boundaries of subsystems, and interactions between variables or entities in different subsystems must be appreciably weaker than those relating variables or entities in the same subsystem. If this is the case then a short-run approximation to the behaviour of the system can be made by ignoring the interactions between subsystems, and analysing each one as if it were isolated.

However, the general applicability of this principle is in doubt. Apart from the remaining problem of long-term interactions, biologists have shown that under feasible conditions there can be permanent and substantial linkage disequilibrium between subsystems (Maynard Smith 1978, Chapter 5; Roughgarden 1979). This suggests that systems can be treated as being nearly decomposable only in a limited number of special cases.

In a seminal work of genetic reductionism in biology, George C. Williams (1966) claims that reductive problems can be solved one locus at a time and

then extended to a global solution by 'iterating over all loci'. This is now recognized by critics as invalid. Williams wrongly presumes that gene frequency alone is an adequate basis for a deterministic theory of evolutionary change, and ignores context dependence. This refers to a situation where the fitness or behaviour of an organism may be significantly dependent on its environmental context, often leading to two-way interactions between a unit and its environment. As William Wimsatt (1980, p. 240) argues: 'Illegitimate assumptions of context-independence are a frequent error in reductionist analyses.'

In the course of his argument, Wimsatt (1980, p. 241) highlights 'the practical impossibility of generating an exhaustive, quasi-algorithmic, or exact analysis of the behaviour of the system and its environment'. In response to this complexity:

> the reductionist must start simplifying. In general, simplifying assumptions will have to be made everywhere, but given his interest in studying relations *internal* to the system, he will tend to order his list of economic priorities so as to simplify, first and more extremely, in his description, observation, control, and analysis of the environment than in the system he is studying. After all, simplifications internal to the system face the danger of simplifying out of existence the very phenomena and mechanisms he wishes to study.

However, there are clear pitfalls in ignoring the complexities of the environment and some of its interactions with the system in question. Therefore the reductionist research strategy, Wimsatt rightly concludes, has an inbuilt bias towards the inclusion of certain types of relations and the exclusion of others.

It should be pointed out that the general idea of a reduction to parts is not being overturned here. Some degree of reduction to elemental units is inevitable. Even measurement or classification are acts of reductionism. Science cannot proceed without some dissection and some analysis of parts. However, although some reductionism is inevitable and desirable, complete reductionism is both impossible and a philosophically dogmatic diversion. What is important to stress is that the process of analysis cannot be extended to the most elementary sub-atomic particles presently known to science, or even to individuals in economics or genes in biology. Complete reductionism would be hopeless and interminable. As Karl Popper has declared: 'I do not think that there are any examples of a successful reduction' to elemental units in science (Popper and Eccles 1977, p. 18). Reduction is necessary to some extent, but it can never be complete. What is contentious is not reductionism *per se*, but its chosen scope and extent, and the ultimate reliance placed on it in comparison with, or to the exclusion of, other general methodological procedures.

The Role of Metaphor

Faced with this problem of complexity and the failure of complete reductionism, some other methodological approach is required. As Brian Arthur has explained:

> If you have a truly complex system, then the exact patterns are not repeatable. And yet there are themes that are recognizable. In history, for example, you can talk about 'revolutions', even though one revolution might be quite different from another. So we assign metaphors. (Quoted in Waldrop 1992, p. 334)

Among the preliminary tasks of scientific analysis are taxonomy and classification, involving the assignment of sameness and difference. Classification, by bringing together entities in discrete groups, must refer to common qualities. To peer into the complex reality and make some sense of it we are guided inevitably by frameworks and habits of thought that we inherit from our past. We construct 'ideal types' (Dyke 1985). We use metaphor. As Arjo Klamer and Thomas Leonard (1994, p. 31) put it: 'Science needs metaphor since it provides the cognitive means to chart the unknown.'

In contrast, scientists often regard metaphors as mere literary ornaments. It is sometimes suggested that they should be removed to reveal the essential theory below. For instance, in economics, Joseph Schumpeter (1954, pp. 17–18, 30, 119, 211, 537, 788–90) persistently alleged that analogies with physics or biology were at best an irrelevant diversion and at worst completely misleading.[5] He wrote at a time when positivistic views of science were at their apogee, and metaphor and analogy were thus regarded as an obstruction on some hypothetico-deductive road to truth.

Typically, in economics and elsewhere, recourse to mathematical modes of expression is motivated in part by a desire to remove all such metaphorical 'literary frills'. However, modern philosophers of science take a very different view. Mary Hesse (1980, p. 111) complains that: 'It is still unfortunately necessary to argue that metaphor is more than a decorative literary device, and that it has cognitive implications whose nature is a proper subject of philosophic discussion.' Similarly, Max Black (1962, p. 237) concludes in a prominent study of metaphor and analogy in science: 'Metaphorical thought is a distinctive mode of achieving insight, not to be construed as an ornamental substitute for plain thought.'

Clearly, for a positivist, a formalist or a nominalist the use of metaphor is a superficial matter, even a distraction: a confusing renaming of entities that can have nothing to do with their essence. But this kind of response is challenged by modern philosophers who argue that metaphor is constitutive and perhaps even indispensable for science. Now we are 'beyond positivism' (Caldwell 1982) in the methodology – if not the perceived practice – of

economics and other sciences. We cannot found theory simply on facts; the facts do not 'speak for themselves', and can neither verify nor strictly falsify our theories. Metaphor is not an ornament: it is an unavoidable means of constituting and ordering our thoughts. As Friedrich Nietzsche wrote in the nineteenth century:

> What, then, is truth? A mobile army of metaphors, metonyms, and anthropomorphisms – in short, a sum of human relations, which have been enhanced, transposed, and embellished poetically and rhetorically, and which after long use seem firm, canonical, and obligatory to all people. (Kaufman 1982)

Metaphors may lead or mislead. By their nature, they are never complete, precise, or literal mappings. If they were precise representations they would not be metaphors, and the juxtaposition of similar but different conceptual frameworks would be lost. This juxtaposition, involving a degree of similarity and dissimilarity, can have both creative and damaging effects.

Metaphors are more than similes. For example, to describe the economy as 'evolving' is not simply to state that the economy develops like an organism or a species in the natural world. It also may prompt the investigator to consider the many meanings and ambiguities in the term 'evolve' and the many extensions and facets of the implicit analogy between the natural and the social world.

In economics the triumph of formalism has done nothing to limit the extensive use of metaphor, with terms such as 'human capital', 'market forces', 'consumer sovereignty' and 'natural rates of unemployment'. The use of metaphor affects not merely the phrasing, but the structure and substance of the discipline (Samuels 1990). Yet the metaphorical references may be partially obscured by the progress of formalism in economics. As mathematical symbols replace words, the analogies may seem to disappear. This is an illusion. As Donald McCloskey (1985, p. 74) puts it: 'Non-economists find it easier to see the metaphors than do economists, habituated as the economists are by daily use to the idea that of course production comes from a "function" and of course business moves in "cycles"'.

The role of the mechanistic metaphor in economics

The use of metaphor in economics has often been explicit. Even at the foundation of modern economic science, Adam Smith appealed specifically to metaphor and Newtonian mechanics in his essay on 'The Principles Which Lead and Direct Philosophical Enquiries: Illustrated by the History of Astronomy'. As Brian Loasby points out, Smith used Newtonian astronomy as primarily a set of 'connecting principles' (Loasby 1989, pp. 1–5; 1991, pp. 6–8). These were held to make sense of his experience and were deemed to

be fitting for his own theoretical work. Indeed, Smith was remarkably candid about the role of metaphor and 'imagination' in science. In the tradition of Descartes and Newton, he said, the aim must be to simplify, to reduce to elemental laws, to render 'nature a coherent spectacle' made up of a few 'primary and elementary objects' (Smith 1869, p. 386).

It has been argued extensively by Philip Mirowski (1989b) and others that modern economics is still dominated by the metaphor of a mechanistic system. Many commentators typify the kind of mechanistic ideas that permeate economics as essentially Newtonian, although Mirowski sees as crucial the additional influence of the energetics movement in physics in the latter half of the nineteenth century. Whatever the precise details of the account, the consensus is that modern economics is still heavily influenced by the kind of mechanistic thinking that dominated physics around the middle decades of the nineteenth century. In particular, the evidence for the substantial influence of physics on the architects of the 'marginal revolution' is substantial. As Mirowski shows, the founders of modern mainstream economic theory all made explicit reference to the mechanistic metaphor, and indicated that their work was guided by such a vision of constitution and structure.

However, the precise details of the transfer of metaphor need to be examined. Here some questions can be raised about Mirowski's account in his *More Heat Than Light* (Mirowski 1989b). He argues that the transfer was overt and generally self-conscious, supposing that the economists involved were continuously aping physics and its every twist and theoretical turn.

There are at least two flaws in this version of the story. First, it underestimates the subtlety of the process of metaphorical transfer. Whilst there are cases of the direct appropriation of concepts and mathematical formalisms – amply documented by Mirowski – the constitutive transfers are at a deeper and less conscious level, affecting the ontology, epistemology and methodology of the subject. With such 'deep level' transfers the contamination of economics by mechanistic thinking is even more profound than Mirowski suggests.

The second flaw is that Mirowski disregards the sociology of the economics profession. We may illustrate this by considering later developments in physics in the twentieth century and their influence on economics. For instance, Mirowski makes a case that quantum theory has had an effect on econometric practice from the 1930s (Mirowski 1989a). Apart from this, however, the general effect of post-quantum physics on economics since 1930 has not been crucial.

If economics has been driven by physics in the direct and overt manner suggested by Mirowski then he would have to account for the fact that the deeper implications of modern quantum theory have not been incorporated. Indeed, the organicist and indeterminist aspects of modern quantum theory

are in profound contradiction to the Cartesianism, reductionism and atomism at the root of both the earlier physics and of modern economics.

Consideration of these points suggests a slightly different story, although it cannot be discussed in detail here. Although Walras, Jevons, Edgeworth and Pareto did ape the physics of their time, the professionalization and institutionalization of economics since then has locked the subject into that particular genre. Once the new norms of scientific activity became established in the emergent economic journals and economics departments of the late nineteenth and early twentieth century then they set down the self-reinforcing standards for subsequent research. Scholars seeking publication or promotion had a better chance of success if they conformed to these norms. The canon of mathematical formalization was endowed with particular prestige. As in other cases of institutional evolution, the phenomenon of locked-in behaviour depending much on initial circumstances is normal rather than exceptional.

Accordingly, the initial role of physics was crucial – in contributing greatly to the initial intellectual environment and in legitimating the allegedly scientific credentials of the subject – but since the early decades of the twentieth century economics has been driven less by physics than by its own momentum. It is institutional practice, including a very outdated view of science and a over-obsession with mathematical formalism, that drives economics along its present groove – not an overt and unabating desire to replicate everything going on in modern physics or other natural sciences.[6] This does not mean that the role of the mechanistic metaphor in economics is diminished. Far from it: it is subtle and ubiquitous.

Although no other social science has used mathematical techniques as extensively as economics, the cache of mathematical tools employed by economists has typically been rather limited. Nineteenth-century physics did not only offer 'scientific' credentials: it provided an armoury of formal techniques, with differential calculus in pride of place. It is only recently that the kind of mathematics bestowed in the last third of the nineteenth century has begun to be overshadowed by other formal developments, particularly game theory. Notably, it is now recognized that the latter's application threatens core tenets such as the axioms of rationality (Sugden 1991).

In overt terms, however, modern economics has almost forgotten the source of the crucial metaphor from which it gained so much formal and theoretical inspiration during the eighteenth and nineteenth centuries. The history of economic thought, now generally and almost completely disregarded by economists, is noticeably absent from undergraduate syllabuses and professional job descriptions. With the ever-sharper focus on mathematical form rather than conceptual substance, attention has been shifted away from the nature and origin of core assumptions in economics.

This is one reason why the exposure of the role of metaphor is so important. It can reveal deeply embedded structures of thought, and provide a clue to why the subject develops along one path rather than another. The reforming theoretician can then identify, and attempt to remove or alter, the malign features at the theoretical core. If we ignore metaphor, or regard it like Schumpeter as a useless diversion, we are impelled to place all our faith in facts and *a priori* argument. By this neglect we are condemned to remain with current orthodoxy. The facts themselves are structured by the dominant theories, and orthodoxy offers us a highly restricted choice of axioms upon which theory may be built.

Whilst Mirowski claims that mainstream economics has continued consciously to ape physics, his ironic thrust is to suggest that they often got the physics wrong. Yet the intellectual aptitudes of economic theorists in a foreign discipline are really of little relevance. The whole point of metaphor, as the history of science reveals, is that ideas from elsewhere do not have to be perfectly understood or entirely appropriate in order to inspire novelty.[7]

Our revised version of Mirowski's story has a better and deeper irony. In the 1870s neoclassical economics attempted to gain the accolade of a science by adopting a thoroughly reductionist research programme, based on the tenet of methodological individualism. In Newtonian spirit, the idea was to found economics on allegedly 'self-evident' axioms about human behaviour, and to develop the formal theoretical apparatus from there. Such a view of science contrasts with the self-conscious application of metaphors to complex systems, the intuitive insight, or the *gestalt*. The deeper irony was that precisely at the moment of foundation of the neoclassical system its architects were forced to make extensive use of metaphor from another domain of study. On reflection, this is a deep embarrassment to the Cartesian or Newtonian way of doing things. It is no accident that, far from chasing every turn in physics, subsequent neoclassical economists quickly forgot this source of original inspiration.

Contrary to Mirowski, the problem for economics is not the general appropriation of metaphor from the natural sciences but more fundamentally the adoption of a positivist conception of science in which the role of metaphor is specifically denied. In this manner the source of the constitutive assumptions of modern economics has been ignored, despite their continuous transmission through the orthodox mathematical formalisms and their reinforcement through the sociological structure of the economics profession. For this reason, the ingrained mechanistic metaphor in economics is even more difficult to remove and to replace.

The argument that metaphors are indispensable in scientific discourse counters the mistaken and positivist view of a metaphor-free science. In

addition, and concerning particular subjects such as economics, it would lead us to the self-conscious evaluation of metaphor, rather than the futile attempt to remove it entirely as found in the work of Schumpeter and many others.[8]

From the mechanistic to the biological metaphor in economics

The deficiencies of the metaphor taken from classical mechanics have been discussed by a number of authors (Georgescu-Roegen 1971; Gowdy 1985; Hodgson 1993; Norgaard 1989; Sebba 1953; Thoben 1982; Veblen 1919). There are a number of perennial problems involved, all relating to the limitations of Cartesian philosophy and Newtonian principles. For instance, movement is reversible in the 'conserved system' of Newtonian mechanics; there is no arrow of time. 'Classical mechanics only knows motion, whereas at the same time the processes of motion are completely reversible and in no way give rise to any qualitative changes' (Thoben 1982, pp. 292–306, 293). Although in some non-conserved, mechanistic theoretical systems the possibility of irreversibility emerges – such as with the addition of friction – the reasons are quite different from those in more complex systems. Above all, with mechanistic presuppositions, cause and effect can be mirrored by logical syllogism, and logical replaces historical time (Robinson 1974).

It is easy to trace the derivation of the ideas of rationality and equilibria, the core concepts of neoclassical economics, from the inheritance of mechanistic thought: 'Classical mechanics considers a system of material points upon which directional forces operate at a distance according to calculable laws of motion. The choice of paths is governed by the principle of least action, which may be termed the economic principle if we take the term in its widest sense as denoting a maximum–minimum principle' (Sebba 1953). Hence, subject to a combination of forces, economic agents optimize to the point of equilibrium as if they were mere particles obeying mechanical laws.

In addition, there is a general difficulty of incorporating information, learning and knowledge in a mechanistic scheme. In classical mechanics there is no place for thoughts and ideas: all is mere matter, subject to Newtonian laws. As Norbert Weiner (1954, p. 29) remarked: 'In nineteenth-century physics it seemed to cost nothing to get information'.

It is not proposed here that the use of mechanistic thinking in economics has been entirely without value. Nevertheless, the limitations are severe. In sum, the mechanistic metaphor excludes knowledge, qualitative change, and irreversibility through time. It entraps economics in equilibrium schema where there are no systematic errors and no cumulative development. Clearly much is missing here. The strength of the alternative, biological, metaphor is that a place can be found for these important features of economic life.

Another extremely important reason why ideas from biology are of relevance to economics is that both economic and biotic systems are highly complex. They both encompass tangled structures and causalities, involve continuous change, and embrace huge variety. Partly for this reason, there is the problem of levels of abstraction and appropriate units of analysis. This has been faced up to and debated by a number of prominent biologists, but far less attention has been given to this vital issue in economics. The adoption of biological metaphors may help to redress the balance.

Taking recourse to biology is not simply a tactic. It is held that real world economic phenomena have much more in common with biological organisms and processes than with the mechanistic world of billiard balls and planets. After all, the economy involves living human beings, not merely particles, forces and energy. Nevertheless, there are risks involved in this trade; biology has often been abused by social scientists in the past, sometimes with horrendous social and political consequences.

It is still widely assumed that evolutionary thinking leads to the rejection of any kind of state subsidy or intervention, and the support for *laissez-faire* on the basis of the idea of 'survival of the fittest'. However, it is wrong to assume that evolutionary theorizing always points to the optimality of competitive outcomes, or to laws of evolutionary 'progress', or to the sagacity of *laissez-faire*. According to modern theory, evolutionary processes do not necessarily lead to – by any reasonable definition – optimal consequences. Similar arguments apply in the economic as well as the biotic context (Gould and Lewontin 1979; Hodgson 1993, Chapter 13).

Moreover, it should be emphasized that biology has internal problems of its own; it is no panacea. Indeed, biology is not itself free of mechanistic metaphor and reductionist methods. A large number of biologists are committed to reductionism, even concerned to explained biological phenomena in physical terms. Within its boundaries, however, there are also pronounced attempts to transcend such strains of thought.

Furthermore, the explicit recognition of the underlying metaphor can perhaps help the theorist to develop 'auto-immune systems' against a rejected strain of thought. For instance, attention to the problem of excessive formalism would lead to the consideration and development of appropriately descriptive and historical approaches to counter an emerging infatuation with the tools and techniques of mathematical biology. Whilst the adoption of a biological metaphor rather than a mechanical one does not itself cure the problem of excessive formalism,[9] the recognition of the complexity and irreducibility of socio-economic reality should warn against this addiction.

It is important to emphasize that all metaphors create difficulties as well as solutions. Accordingly a precise transfer of ideas is neither possible nor desirable. As Renate Mayntz (1992, pp. 68–9) puts it:

> What we do ... is not to use the natural science theory to explain social phenomena, but the natural science theory stimulates a new way of viewing the social phenomena that, guided by some rather abstract notions, triggers a process of social science theory building. Strictly speaking, the potentially most productive mode of borrowing is therefore one where transfer in a direct way does not take place at all.

A pre-eminent problem with both the biological and the mechanical analogy is the conceptualization of the human agent. It has been argued by several critical economists that – despite the rhetoric – orthodox and mechanistic economics provides no room for real individual choice (Loasby 1976). Biological natural selection invokes genetic replication and random variation or mutation, but seemingly affords no role for intentionality, purposefulness or choice.

The problem of 'vitalism', involving choice, will and purpose, has been persistent within biology. Although 'vitalism' is now out of fashion, it raises real issues of importance, even if the notion is shunned by biologists who confine themselves to causal rather than intentional explanations. Dissenters to strict Darwinism, including Arthur Koestler, have tried to instate concepts of will and purpose in the science, but with limited effect. This whole problem should not be ignored or underestimated: It is addressed at length elsewhere (Koestler 1967; Hodgson 1993, Chapter 14).

Despite all the problems and dangers, it is suggested here that modern biology provides a rich source of ideas and approaches from which a revitalized economics may draw. In all, the application of an evolutionary approach to economics seems to involve a number of advantages and improvements over the orthodox and mechanistic paradigm. For instance, it enhances a concern with irreversible and ongoing processes in time,[10] with long-run development rather than short-run marginal adjustments, with qualitative as well as quantitative change, with variation and diversity, with non-equilibrium as well as equilibrium situations, and with the possibility of persistent and systematic error-making and thereby non-optimizing behaviour.

In short, an evolutionary paradigm provides an alternative to the neoclassical 'hard core' idea of mechanistic maximization under static constraints. The theory of rational choice at the core of mainstream economics relies on static assumptions, the notion of an eventually constant decision environment, and the idea of global rationality, which are all challenged by evolutionary theory (Cooper 1989; Goldberg 1975; Hodgson 1994).[11]

Finally, connecting economics with biology may also result from a recognition that as human beings we are part of the natural world. Although the levels and modes of analysis in the two disciplines are different, reflecting the emergent properties in each sphere, the provision of conceptual and theoretical links pays respect to the common reality (Bhaskar 1979; Hirst and Woolley 1982).

In Conclusion: Theoretical Pluralism

The Latin word *cogito* comes from *coagitare*, to shake together. Much creativity in science comes from the integration of hitherto unrelated ideas or frames of thought. Examples are plentiful (Koestler 1964). Pythagoras observed a blacksmith at work and noticed that iron bars of different lengths gave out sounds of different pitch under the strokes of the hammer: conceptions of sound and length, of music and measure, were amalgamated. In 1820 Hans Christian Oersted saw that an electric current flowing through a wire deflected a nearby compass. He thus discovered the hitherto unrecognized link between magnetism and electricity, created the concept of electromagnetism and inspired the development of the electric motor. Michael Faraday set himself the problem of finding the connections between light, heat, magnetism, and electricity and developed a unified theory of electromagnetic radiation. Energy and matter later became unified in Albert Einstein's theory of relativity.

The founder of pragmatism, Charles Sanders Peirce (1958, pp. 123–5) argued that an important source of creativity in science was the 'abductive' transfer of metaphor from one scientific discourse to another. 'Abduction' was Peirce's addition to the traditional dichotomy of induction and deduction. It concerned the creative process of forming an explanatory hypothesis. According to Peirce (1934, p. 90), induction 'never can originate any idea whatever. No more can deduction. All the ideas of science come to it by the way of abduction.' Hence abduction alone could account for creativity and progress in science. By abduction, Peirce seems to have in mind the spark of intellectual creativity or intuition, kindled in the tinder of assimilated facts. He writes:

> The abductive suggestion comes to us like a flash. It is an act of *insight*, although of extremely fallible insight. It is true that the different elements of the hypothesis were in our minds before; but it is the idea of putting together what we had never before dreamed of putting together which flashes the new suggestion before our contemplation. (Peirce 1934, p. 113).

For Peirce, the 'abduction' via metaphor of ideas from another context enables us to put together 'what we had never before dreamed of putting together'.[12] As long ago as 1882, Peirce (1958, p. 46) wrote:

> But the higher places in science in the coming years are for those who succeed in adapting the methods of one science to the investigation of another. That is what the greatest progress of the passing generation has consisted in. Darwin adapted biology to the methods of Malthus and the economists; Maxwell adapted to the theory of gases the methods of the doctrines of chances, and to electricity the methods of hydrodynamics. Wundt adapts to psychology the methods of

> physiology; Galton adapts to the same study the methods of the theory of errors; Morgan adapted to history a method from biology; Cournot adapted to political economy the calculus of variations.

Ultimately, the clashes and tensions between different approaches in a subject provide the sources of creativity and novelty. A possible source of creativity in science is through the juxtaposition of two different frames of reference, so that already existing but previously separate ideas can cross-fertilize. Accordingly, Larry Laudan (1977, p. 103) argues that the amalgamation of different research traditions may produce a sum greater than the constituent parts. Arthur Koestler (1964) has coined the term 'bisociation' to describe the kind of adjoining of different ideas that occurs in the act of scientific creation. Providing such a bridge between different discourses and contexts, metaphor can thus be both creative and constitutive.

There is no guarantee that metaphor can play a creative role. Conceivably in a given context there is a large set of metaphors that would be employed to no positive benefit. However, the inspired choice of metaphor seems to be a major source of theoretical innovation in science.

Compare the degree of theoretical pluralism inside modern economics with that in modern biology. Although both disciplines are still strongly affected by a reductionist imperative in the culture of modern science, in biology there are debates concerning methodology and core propositions. For instance, in biology there is a debate about the nature and boundaries of the subject, embracing a discourse concerning reductionism and the appropriate units of evolutionary selection and another debate over the viability of further reduction from genetics down to molecular biology, and even below to chemistry and physics (Sober 1984). In contrast, confident in the Newtonian metaphor of the indivisible, 'individual' particle, mainstream economics traditionally proscribes discussion of the psychological or social foundations of individual purposes and preferences as being beyond the bounds of the discipline.

The internal lack of consensus within biology is itself refreshing. A variety of forces and tensions within that subject point to an organicist ontology, a less rigid methodology, and the transcendence of mechanistic thinking. These indicators are discussed in the work of leading mainstream biologists such as Theodosius Dobzhansky and Ernst Mayr, as well as more heterodox scientists such as Niles Eldredge and Stephen Jay Gould, along with historians of biology such as Edward Manier (Dobzhansky 1968; Eldredge 1985; Gould 1982; Manier 1978; Mayr 1985a).

Partly because of the acknowledged complexity of the phenomena that it attempts to analyse, biological science exhibits a theoretical pluralism (Gould and Lewontin 1979; Mayr 1985b). Substantially, this involves a break from

the Newtonian practice of single explanations that are supposed to displace all superfluity. Characteristically, Darwin himself placed great stress on his argument that natural selection was not the only possible element in evolution. In the last edition of the *Origin of Species* he wrote:

> As my conclusions have lately been much misrepresented, and it has been stated that I attribute the modification of species exclusively to natural selection, I may be permitted to remark that in the first edition of this work, and subsequently, I placed in a most conspicuous position – namely at the close of the Introduction – the following words: 'I am convinced that natural selection has been the main, but not the exclusive means of modification.' (Darwin 1872, p. 395)

Hence Darwin accommodates the possibility of a plurality of different but complementary theories. Note that this kind of pluralism is that of 'theory complements', rather than of 'theory substitutes' where different and incompatible theories are involved.[13] Furthermore, as David Hull (1973, pp. 3–36) points out, Darwin's methodology is not rigidly axiomatic. There is a rigorous deductive core, but it is deemed to prove little on its own and it is thus placed in the context of a mass of empirical material. Again this contrasts with the Cartesian-Newtonian tradition in science.

Hence in biology deductive arguments are combined with contingent empirical premises and conclusions. Typically, in biology a number of theories and explanations compete in their claims to identify the main, rather than the exclusive, cause in given real circumstances. Fortunately, biology does not present the near-monopoly of methods and approaches that threatens to stifle economics today.

Faced with these problems there is no instant solution. Neither is there an adequate and well-formed heterodoxy waiting in the wings. Austrian economics, post-Keynesian economics, Marxian economics, institutional economics: they are all afflicted with deep internal theoretical problems of their own. We have to be candid about the limitations of even our most favoured approach in economics, whatever it may be.

An obvious but crucial argument for theoretical pluralism in science can be derived from biology itself. Darwinian evolution depends on variety, and variety is the evolutionary fuel. Without the maintenance and regeneration of a variety of forms evolution would come to a stop. The evolution of scientific ideas requires diversity and pluralism as does evolution in nature. Let a thousand flowers bloom.

Clearly this applies to economics as well as other sciences. In this spirit we can do little better than follow the example of the self-avowed 'neoclassical' economist Frank Hahn (1984, pp. 7–8):

> The most strongly held of my views ... is that neither is there a single best way for understanding in economics nor is it possible to hold any conclusions, other than

> purely logical deductions, with certainty. I have since my earliest days in the subject been astonished that this view is not widely shared. Indeed, we are encompassed by passionately held beliefs. ... In fact all these 'certainties' and all the 'schools' which they spawn are a sure sign of our ignorance ... we do not possess much certain knowledge about the economic world and ... our best chance of gaining more is to try in all sorts of directions and by all sorts of means. This will not be furthered by strident commitments of faith.

Hahn's welcome comment seems to endorse a theoretical pluralism, embracing both neoclassical and non-neoclassical approaches. This pluralism contrasts with the prevailing view that economics is defined in terms of a single, and neoclassical, method or approach. Particularly since the famous essay by Lionel Robbins (1932), among economists the prevailing practice has been to regard this subject as being defined by a single type of method or analysis, with an associated set of core assumptions. If the subject is defined in this way then not much theoretical pluralism within economics is possible; we are stuck with a single type of theory or approach.

In economics today there is increasing standardization of thought and the core assumptions are defended with renewed zeal. Perhaps the most optimistic interpretation possible of this widespread phenomenon is suggested by Hahn (1991, p. 49) who writes: 'After all in other spheres, say religion, one often encounters increased orthodoxy amongst some just when religion is on the decline.' Yet even this statement is not that optimistic: a religion under threat can lead to an Inquisition rather than a Renaissance. Yet unless renewed variety is generated within the discipline its degeneration into a narrow, over-abstract, and practically non-operational formalism will continue apace.

Strictly speaking, however, this pluralism concerns the policy of institutions towards the funding and nurturing of science. It is 'pluralism of the academy' and it does not concern the *individual practices* of science itself. There is much to be said for tolerance of many different and even antagonistic scientific research programmes within a department, university or nation. But we should not tolerate the existence of inconsistent ideas within our own heads. The role of diversity is not to sanctify or foster contradiction. Tolerance of the right of a scientist to practise, even when we may disagree with his or her views, does not imply tolerance of any method and proposition. The policy towards science must be pluralistic and tolerant, but science itself cannot be so. Pluralism does not mean that 'anything goes'.[14]

Methodological anarchism means the tyranny of the strong, and the strongest are those currently in power. Hence, despite anarchistic appearances, the precept of 'anything goes' is ultimately conservative. In contrast, the policy of academic pluralism provides a relentless challenge to orthodoxy and heterodoxy alike. We have to recognize the immense and enduring value of

pluralism within the discipline without abandoning precision and rigour in our own work.

Notes

1. The author is very grateful to Philip Mirowski, Bruna Ingrao, Roy Weintraub, Uskali Mäki, Tony Lawson, Ernesto Screpanti and several other participants at the workshop on 'Pluralism in Economics', in Bergamo, Italy, in June 1994 for critical and helpful comments on an earlier draft of this chapter.
2. Whilst central to the argument here, the concept of complexity has proved notoriously difficult to define. Stent (1985, pp. 215–16) makes a useful stab at the problem, arguing that 'the complexity of a phenomenon is not to be measured by the number of component events of which it is constituted, but rather by the diversity of the interactions among its component events.' See Lloyd (1990) for a review of definitions of complexity.
3. See also Lavoie (1992, pp. 36-41), Rizvi (1994), Screpanti and Zamagni (1993, pp. 344-53).
4. There are many other problems in modern economics, and the crisis in economic theory afflicts heterodox as well as orthodox traditions. However, for reasons of brevity the other topics and schools of thought are not discussed in detail here.
5. By contrast, Veblen (1919) and later American institutional economists embraced the analogy with biological evolution, making them the true inheritors of the 'evolutionary' epithet in economics.
6. As Peter Allen and M. Lesser (1991, p. 166) have wittily suggested, the evolution of economics as an academic profession is a case of lock-in comparable to the evolution of the peacock's tail. Although the male's beautiful tail makes it more successful in mating and producing offspring, there is no useful function or enhancement of fitness in terms of finding food or escaping predators. Just as the beautiful tail evolves with the peacock, economics has evolved an ever more intricate and beautiful mathematical formalism, similarly with little or no functional advantage for the development of economic policy. The developers of abstract theorems and proofs are awarded with prestige and resources, although there is an increasing suspicion by those on the outside that economics has less and less to do with real economies.
7. Examples of this are legion in both the physical and the social sciences. The unconvinced should read Koestler (1959) and take – as one of several examples in that brilliant book – Kepler's obsessive belief in the false idea that everything in the universe is built around some basic geometrical symmetries and the way in which it led to the birth of modern cosmology. Likewise, in economics, Marx adopted from the classical economists the untenable notion of 'value' as a substance, and one generated by labour alone: with it he constructed the wonderful theoretical edifice of Capital. Yet essentially this theoretical structure survives without any labour (or substance) theory of value (Hodgson 1982), and still provides some of the best insights into the workings of modern capitalism.
8. Mayntz (1992, p. 53) notes that in the past sociologists used the metaphor of natural selection, but because 'no attempt at conscious theory transfer was made ... the question of how biological and sociocultural evolution might differ was not explicitly raised.' This is surely an object lesson for all those who want to keep their metaphors at arm's length.
9. Note the disturbing degree of mathematical formalism in the – otherwise often useful and innovative – *Journal of Evolutionary Economics*.
10. It should not be assumed, however, that all evolutionary processes in biology are irreversible: see Mani (1991). An extended discussion of irreversibility in economics is given by Dosi and Metcalfe (1991).

11. Furthermore, as Nicolai Foss (1994) has argued, there is a key ontological difference between neoclassical economics and the kind of 'evolutionary' economics exemplified by Nelson and Winter (1982) and Witt (1987). Whereas evolutionary economics theorizes on the basis of a universe that is open, with novelties and emergent properties, neoclassical economics suppresses novelty. The importance of such open systems and emergent properties is stressed by philosophical realists such as Bhaskar (1979) and Lawson (1989). See also Dyke (1985) who also discusses the problem of closure in complex phenomena. The existence of open systems and emergent properties is incidentally another major reason why the reductionist project in science is confounded.
12. One of the clearest recent attempts to elucidate the notion of abduction, in a realist philosophical framework which is specifically related to economics, is by Tony Lawson (1989, pp. 68–73). Warren Samuels has suggested that the advocacy of methodological pluralism implies the non-existence of a given reality. A contrasting point is made here. As no theoretical analysis of a reality can completely represent or mirror that reality in all its complexity, therefore we cannot rule out other viable and complementary analyses in principle. Philosophical realism and the assertion of a given reality 'out there' are perfectly compatible with such a proposition.
13. The kind of pluralism advocated here is further qualified below.
14. This statement concurs with the convincing argument by Sheila Dow in her 'Structured Pluralism', *Journal of Economic Methodology* 11:3, pp. 275–90, September 2004.

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9
Explanatory Pluralism

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Ever since in June 2000 a group of economics students in France published a petition with their complaints concerning (the contents of) their education – giving birth to the post-autistic economics movement – there has been much discussion and writing about (the lack of) pluralism in economics.[1] One of the major voices arguing against so-called mainstream economics, and pleading for more heterodoxy and a more pluralistic approach to economics in the academy, is Tony Lawson.

In this chapter, I want to analyse whether Lawson's proposals for explaining economic action are really pluralistic. In order to do so, I will start by giving a general outline of the lines of critique Lawson develops against mainstream economics. Next, I will focus more specifically on Lawson's critiques and alternatives concerning social scientific explanations and their relation with ontological presuppositions. After presenting Lawson's alternative to mainstream economics, I will start scrutinizing his proposals concerning explanation and social ontology, and evaluate his plea for pluralism. The final section discusses the conclusions and some open problems concerning pluralism in economics and the plea for heterodoxy.

Tony Lawson's Criticisms of Mainstream Economics

Tony Lawson is one of the most prominent defenders of critical realism in the social sciences.[2] In his work he is focusing on the discipline of economics and the most developed account of critical realism in economics up to now can be found in his *Economics and Reality* (1997a). Lawson starts this book by illustrating the unhealthy state of the mainstream or orthodox economics (that is, centring on econometrics and formalistic 'economic theory') by pointing to three types of inconsistencies between theoretical perspectives and actual practices that exist in the current mainstream economics approach:

1. Inconsistency at the level of method. Lawson (1997a, 5–8) observes that mainstream economists employ methods and practices of inquiry that are inconsistent with the theoretical perspectives on method they claim to draw upon. For instance, in econometrics many practices are inconsistent with the accepted theory, little more than lip service is paid to methodological rigour (for example, the frequent and frequently important refitting of econometric relationships, the repeatedly made *ad hoc* revisions to estimated parameter values, and the practice of using patterns in recent residuals to revise intercept estimates for forecasting purposes).
2. Inconsistency at the level of social theory. As an example of this, Lawson states that most mainstream economists appear to support 'an intuition that human beings possess the capacity of exercising real choice' (Lawson 1997a, 8), but these individual real choices are neglected as 'individuals are represented in such a way that, relative to their situations, there is almost always but one preferred or rational course of action and this is always followed' (Lawson 1997a, 9).
3. Inconsistency at the level of methodology. Lawson notices that the mainstream frequently concludes that 'methodology' or 'philosophy' is irrelevant to (progress in) economic science, discourages debates about these issues, and wants to take on the appearance of being philosophically neutral. The very same mainstream economists, however, can be found arguing and defending positions that can only be described as overtly methodological or philosophical (Lawson 1997a, 11–13).

Having summed up some of the symptoms of the unhealthy state of mainstream economics, Lawson makes a diagnosis. The 'blame' for the disorder afflicting contemporary economics is laid on the deductivist mode of explanation that reigns in mainstream economics: 'The main 'culprit', I shall argue, is a mode of explanation that can be referred to as deductivist, or, more particularly, it is the conception of 'laws' (or 'significant results' or 'theoretical formulations') upon which deductivist explanation ultimately depends' (Lawson 1997a, 16).

After putting the blame on deductivism and the covering-law model, Lawson elaborates the way in which this model of explanation is based on a perspective on reality that he calls – following Roy Bhaskar and others – empirical realism, which sees reality as that which is given in experience, or makes the objects of scientific investigation the same as the objects of sense experience. The uncritical acceptance of empirical realism by mainstream economics explains, according to Lawson, its recurrent problems. Therefore, Lawson criticizes empirical realism and its epistemic counterpart positivism (to the latter he attributes at least two fundamental assumptions, namely that science is essentially monistic in its development and deductive in structure; see Lawson 1997a, xv) and shows the consequences of adopting them within

economics. He points at the minimal concern or neglect that is shown by mainstream economics for ontology, the nature of the social world; in order to start solving the problems, Lawson proposes to shift attention from epistemological questions to ontological questions. Drawing on the writings of Bhaskar, he proposes a different perspective on reality, elaborating transcendental realism as the medicine to cure contemporary economics.

This transcendental realism differs from empirical realism in two significant ways. First, according to Lawson it is necessary to abandon the view, defended by empirical realism, that the generalizations of nature consist of event regularities (as this event regularity account rests on an illegitimate generalization of a situation that emerges as a special case); the world is not composed only of events and states of affairs and our experiences or impressions. Instead, we have to accept an account of the world as structured (that is, irreducible to the events of experience), and intransitive (that is, existing and acting independently of their being identified). The world is not solely constituted by events and states of affairs and our perceptions of those actualities, but also by underlying structures, powers, mechanisms and tendencies that exist, whether or not detected, and govern or facilitate actual events. From this perspective, three ontologically overlapping, but distinct, domains of reality are distinguished: the empirical (experience and impression), the actual (actual events and states of affairs in addition to the empirical) and the real (structures, mechanisms, powers and tendencies, in addition to actual events and experiences). Second, on the transcendental conception, these three domains are out of phase with one another.

I will go deeper into this transcendental realist position and the influence of Bhaskar in the next section, but let me now round up Lawson's strategy of criticizing mainstream economics and the implications for his mode of explanation. Pointing out inconsistencies in contemporary economics, Lawson puts the blame on 'the minimal concern that is shown either for tailoring methods to insights available regarding the nature of the social world, or for explicitly determining the sorts of conditions under which chosen methods would be appropriate' (Lawson 1997a, xii–xiii). As a remedy he suggests reconsidering empirical realism, and proposes his transcendental realism as an alternative. Introducing this new ontology of transcendental realism will have an impact on the mode of explanation in science, and in economics in particular: 'Explanation … entails providing an account of those structures, powers and tendencies that have contributed to the production of, or facilitated, some already identified phenomenon of interest. It is by reference to enduring powers, mechanisms and associated tendencies, that the phenomena of the world are explained' (Lawson 1997a, 23). We will further stipulate the consequences for the modes of explanation in the next sections.

The transcendental realism is then presumed to be capable of solving the above-mentioned inconsistencies in contemporary economics. The inconsistencies on the level of method will be addressed by introducing a different mode of inference and explanation: the deductive covering-law model of mainstream economics (presupposing closed systems), will be replaced by a causal approach derived from an ontological perspective that emphasizes the open and structured nature of reality (of both the natural and the social realms). Transcendental realism entails, as well, a different mode of inference from the one sponsored by positivism, according to Lawson: 'the essential mode of inference sponsored by transcendental realism is neither induction nor deduction but one that can be styled retroduction or abduction or 'as if' reasoning' (Lawson 1997a, 24).[3] The inconsistency on the level of social theory, as mentioned above, will disappear by elaborating an account of human agency and choice within Lawson's perspective on the nature of social reality, emphasizing the interdependency of structure and agency (see Lawson 1997a, 168–69 on, the Transformational Model of Social Activity). Finally, the inconsistencies on the level of methodology will disappear within the transcendental realist approach to economics, as this approach obviously does not refrain from explicit methodological discussion, and considers methodology and philosophy as necessary activities for the transformation and development of economic science. (It rejects the seeming philosophical neutrality of mainstream economics.)

This section has provided a very short summary of Lawson's critique of mainstream economics, and his proposal for developing a better way of doing economics. The key to his solution is the reorientation of ontology. In what follows, I will scrutinize whether this strategy should be adopted, whether it will lead to a better standard for explanations, and whether it will lead us to pluralism.

Tony Lawson's Alternative View on Ontology and Explanation

The epistemic fallacy

First, I want to discuss a very central issue in Lawson's proposals for the reorientation of economics, namely the unveiling of the epistemic fallacy, being the fallacy that transposes what is an ontological matter into an epistemological matter; this is a failure to sustain adequately the distinction between ontology and epistemology, resulting in the relative neglect of ontology. The mainstream economists analyse statements about being solely in terms of statements about knowledge, and thus reduce ontology to epistemology. Therefore, as a reaction to this neglect, Lawson argues, it is

> opportune to develop a perspective on the way that social reality is, rather than merely to assume under the sway of the epistemic fallacy that it must conform to the sort of a priori, typically formalistic, methods of which economists are currently, if largely unthinkingly, enamoured. (Lawson 1997a, 154)

After the unveiling of the epistemic fallacy, the focus should be replaced on ontology, or as Steve Fleetwood puts it:

> Clearly any such awareness (that the basic (deductivist) method of mainstream economics is inappropriate to its subject matter) presupposes a prior analysis of the nature of social phenomena – as does any project of developing an alternative. Hence, for the critical realist project, socio-economic ontology figures centrally. (Fleetwood 1999, 129)

Central in the focus on (socio-economic) ontology, then, figures the transcendental argument for social structures, elaborated by Roy Bhaskar. He derives an account of a metaphysics of science, called transcendental realism (see above), by inquiring what the world must be like before it is investigated by science, and for scientific activities to be possible. Bhaskar's transcendental realism defends the existence of social structures and society as follows:

> conscious human activity consists in work on given objects and cannot be conceived as occurring in their absence. A moment's reflection shows why this must be so. For all activity presupposes the prior existence of social forms. Thus consider saying, making and doing as characteristic modalities of human agency. People cannot communicate except by utilizing existing media, produce except by applying themselves to materials which are already formed, or act save in some other context. Speech requires language; making materials; actions conditions; agency resources; activity rules. Even spontaneity has as its necessary condition the pre-existence of a social form with (or by means of) which the spontaneous act is performed. Thus if (as previously argued) the social cannot be reduced to (and is not the product of) the individual, it is equally clear that society is a necessary condition for any intentional human act at all. (Bhaskar 1979, 34)

Tony Lawson (1997a, 30–31; 1997b, 28–29) provides a similar argument to prove that there are identifiable social structures operative in the social realm that are real and irreducible to human agency. 'In other words, a transcendental argument from intentional agency … establishes the sui generis reality and the temporal pre-existence of forms of social structure, as its necessary conditions and means' (Lawson 1997b, 29). All based on an *a priori* analysis.

Thus, the epistemic fallacy should be avoided by focusing on ontology, that is, a prior analysis of the nature of social phenomena, which leads Lawson to subscribe to the existence and irreducibility of social structure

(following the transcendental argument). Is this, however the (only) way to avoid the epistemic fallacy? Before addressing this question, I will outline Lawson's proposals on explanations.

Explanations and mechanisms

How does the *a priori* reorientation (away from the epistemic fallacy) affect Lawson's position on social scientific explanations? In his account of explanations in economics, Lawson emphases his rejection of the positivist covering-law model: the conception of reality presupposed by the covering-law model, the theory of laws or law-like statements on which it depends, and the commitment to closed system modelling should all be rejected. Recognition that the social world is open and hardly amenable to scientifically interesting closures, according to Lawson, means that the mainstream model of explanation has to be replaced by the identification of mechanisms, structures, capacities, et cetera. (situated in the domain of the real, see above): 'I do argue that the primary aim of science and explanation is to identify and understand the underlying structures, capacities, mechanisms, etc. which causally bear upon (facilitate, influence, produce) surface phenomena, including events, of interest' (Lawson 1999, 233).

I will look into his proposals concerning social scientific explanations in a more detailed way later, as they are important for a good understanding of Lawson's pluralism, but for now I just want to draw attention to the tight connection of an ontological critique and *a priori* reorientation with its consequences concerning the (ideal) mode of explanation.

Scrutinizing Lawson's Proposals on Ontology and Explanation

Let us now subject Lawson's proposals to close scrutiny. First we will deal with some questions concerning Lawson's proposals on social ontology, for example to what extent does Lawson's ontological reorientation consider actual social scientific practice; how does Lawson avoid the epistemic fallacy? The following subsection will deal with Lawson's ideas on explanation and its relation to social scientific practice.

The ontological fallacy?

Lawson adopts Bhaskar's ontological conviction and the transcendental argument – as I have mentioned in the previous section – that 'the social cannot be reduced to (and is not the product of) the individual, [and that]

it is equally clear that society is a necessary condition for any intentional human act at all' (Bhaskar 1979, 34). Bhaskar had formulated an argument about underlying mechanisms and structures in the natural sciences as well. This argument could not, however, be transferred directly from the natural sciences (in which, via experimentation, scientists are able to acquire knowledge of underlying mechanisms at work) to the social realm. Some substantial modifications were necessary, as experimentation in social sciences is rare. So the 'proof' of the existence of structures in the social realm provided by the transcendental argument for social structure is not analogous to the argument from experiments in the natural sciences. The argument from experiments starts from a widely accepted and successful practice (or method of investigation), while the argument for social structures starts from a (folk) social theory. The critical realist approach suggests that our confidence in the existence of social structures is based on a transcendental derivation from the category of 'choice' (rather than on a careful mix of observation and induction, and taking into account social scientific practice). Therefore, I do agree with Allin Cottrell (and other critics: see Boylan and O'Gorman 1995; Guala 2002; Parsons 1999) that Lawson's transcendental argument for social structures is not uncontroversial:

> Where the basic premise of Bahskar's transcendental inference – that scientific experimentation has led to remarkable successes in producing sought-after effects outside the lab – is fairly uncontroversial, Lawson's – that human choice is really free, and in a strong metaphysical sense that is incompatible with determinism – is highly controversial. (Cottrell 1998, 350)

I do not want to go into the details of the argument (although I do share the scepticism of the critics), but instead want to draw attention to the way an ontological stance has been adopted here by Lawson (and with it a restriction of methodological options). Lawson's method of reasoning about the relation between social ontology, on the one hand, and epistemology and methodology, on the other, is a recurrent phenomenon in the philosophy of the social sciences.[4] It starts with certain *a priori* or necessary truths concerning social ontology, be they justified by 'metaphysical commonplaces', (questionable) transcendental arguments or political convictions, and then, the methodological consequences seem to follow *automatically* from the ontological stance.

What happens, according to me, is that preconceived ideas on ontology are imposed too hastily, while it is the job of the social scientist to investigate the 'nature' of social reality. Why does *a priori* analysis have to inform us on the nature of social phenomena? Why is so much weight given to *a priori* metaphysical and ontological debates – which easily turn into unending

battles of intuitions – at the expense of methodological ones, which might maximize (our understanding of) good social scientific practice?

I want to argue not that social structures (or other ontological assumptions of the critical realist's stance) do not exist, but that the way their existence has been defended by Lawson, and by critical realism in general, is problematic, just as it has been problematic in earlier attempts to impose preconceived ontological ideas in the (philosophy of the) social sciences (for example, Watkins 1973). The attempt to justify the claim that the world has indeed the form argued for in transcendental realism does not convince (or, better, does not convince me more than other stands in the unending battle of metaphysical intuitions we experience in the philosophy of the social sciences). Moreover, as the ontological choice made by critical realism does have an impact on methodological options, I want to warn of an ontological fallacy: taking an *a priori* ontological stance that transposes or reduces epistemological and methodological matters to a matter of ontology.[5] Analogous to the epistemic fallacy the ontological fallacy points at a failure to sustain adequately the distinction between ontology and epistemology, that is, to deal with both ontology and epistemology in a non-reductive way.

In criticizing Bhaskar's and Lawson's transcendental argument I do not want to suggest that no attention should be paid to ontological 'depth', or that their ontological convictions would be necessarily wrong, or that all ontological problems should be reduced to epistemological problems. I rather want to point to the seemingly unproblematic character of the ontological point of view imposed by critical realists and its seemingly minimal concern with epistemology and methodology. Lawson speaks of epistemological relativism (Lawson 1997a, 58–9), with which he correctly points to the historical and social relativity of knowledge. But he seems, as well, to suggest that the ontological framework as defended in the transcendental argument would leave maximal choice (nothing has been decided) among epistemological and methodological options, call it pluralism, so long as the 'nature of reality' is respected. This is a nature on which Lawson decides *a priori*: 'a realist orientation of the sort I am intending to defend insists that methods of social science can, and indeed should, be designed to take account of available insights concerning the nature of social material' (Lawson 1997a, 16). Does Lawson rely on the available insights? Does he not just start from a preconceived idea on the nature of reality? Moreover, these preconceived ideas are not epistemologically or methodologically neutral:[6] the existence of emergent social structures (see Lawson 1997a, 63ff), for instance, and the adoption of the Transformational Model of Social Activity (see Lawson 1997a, 168–9) do have an impact on the (ideal) explanatory practice (that is, ideal social explanations will require accounts of both individual intentional action and

deep social structures and relationships, since the Transformational Model of Social Activity insists that both the social and the individual matter; that is, on the one hand, social structures and social relationships condition, sometimes even determine, the actions of individuals, but on the other hand, the social structures and social relationships are reproduced – and, in this sense, the social is a consequence of individual action – by individual intentional actions).

Concluding this section, I want to insist on the importance of analysing the ontological starting points of the social scientists themselves, and of the actual state of social science on ontological matters (as a counterweight to the *a priori* analysis and the ontological fallacy). Second, it has to be acknowledged that imposing an *a priori* ontology is not epistemologically neutral and does affect the methodological options available (it does not leave us with a maximal choice). Summarizing, I want to emphasize that my main critique of Lawson's dealing with the epistemic fallacy is not so much that his ontological proposals are 'wrong', but rather concerns the *a priori* way he is imposing this ontology without making the implicit methodological options, consequences and restrictions explicit, and without taking social science practice into account. Let us now turn to a critical analysis of Lawson's ideas on explanation.

A new standard of explanations?

Boylan and O'Gorman (1999) have criticized Lawson's standard of explanation on several points, mainly pointing at his neglect of the impact of contextual and pragmatic factors:

> In short, no one explanation has precedence over another; there is no such thing as the explanation. This is, in my opinion, the tacit presupposition of transcendental realism and, as we have just indicated, this presupposition does not stand up to critical scrutiny. We must take seriously the context-ladenness of scientific explanation. (Boylan and O'Gorman 1999, 144)

Lawson reacts to their critique by stating:

> First, they point out that most if not all explanation is inherently contrastive: the question addressed implicitly takes the form of 'why this outcome rather than that one'. Second, they note that the particular contrast adopted in any study will depend on context, and (implicitly) the presuppositions and interests of whoever is asking the question. Third, they observe that the causes of phenomena will typically be numerous, with different ones identified by different investigators, according to their individual areas of expertise. In fact, the only part of all this that I have problems with is the assumption that such insights are somehow at odds with my own position. (Lawson 1999, 234)

Despite Lawson's claims that the critique of Boylan and O'Gorman is not at odds with his position, I support Boylan and O'Gorman in their critique since Lawson does underestimate the impact of contextual and pragmatic factors on the form of explanation being used. This becomes clear in Lawson's distinction between a pure kind of explanation and an applied kind of explanation, for example: 'But contrastives (in applied explanations), if vital for our purposes, are not sufficient. The possibility of a useful science depends on there being relatively enduring mechanisms or processes to uncover' (Lawson 2001a, 385). 'My approach is more general in that … it covers both pure and applied explanation' (2001a, 385). Let me first explicate Lawson's distinction between pure and applied explanation, before I develop my critique.

Lawson (1997a, 220) sees economic explanation as divided into 'two relatively distinct movements or separate modes of activity'. The first movement deals with the pure or abstract or theoretical explanation, in which underlying structures, powers, mechanisms and their tendencies are identified. According to him it is a necessary condition for explanatory activity that 'certain relatively stable and enduring mechanisms do at some time and place come to be reproduced, to endure…' (Lawson 1997a, 220) and that these mechanisms are explained by the pure explanations. I do agree that this first kind of explaining is legitimate.[7] The second movement concerns the applied or concrete or practical explanation (I call this second form of explanation, explanation of particular facts or contrasts between facts). Here, the idea is that concrete phenomena of experience are explained by 'drawing upon antecedently established knowledge of relatively enduring structures and mechanisms (rather than revealing them), and investigating the manner of their joint articulation in the production of the novel event in question' (Lawson 1997a, 220). So, the pure explanation is considered as the primary focus, without which no applied explanation can be provided, and it is considered the primary aim of science to identify these mechanisms and structures (see above). Here Lawson seems to adopt a bad habit of the covering-law model, namely to focus primarily (in the applications of the model) on the confirmation of the law (respectively the mechanisms in Lawson's case), and not on the development of an adequate explanation of a particular social fact (suggesting a hierarchy between both).[8]

Let me now look into Lawson's account of the second-order applied or practical explanations, and the idea of (explanatory) pluralism. I have been defending a pluralism of explanations that takes into account that different knowledge-interests can lead to different forms of explanation. I will give a short overview of this framework, which will help us in the further analysis of Lawson's proposals. In analysing social scientific practice, it is obvious that different knowledge-interests are reflected in different explanatory requests

(or explanation-seeking questions). In earlier publications I have shown how different questions about one social fact can lead to the use of different forms of explanations (Weber and Van Bouwel 2002; Van Bouwel and Weber 2002a). In developing this framework, I used the erotetic model of explanation that regards explanations as answers to why-questions. Different explanatory requests can be distinguished in social scientific practice. (I do not consider the explanatory requests and motivations mentioned here as the only possible ones, but I do believe they are omnipresent in social science practice.)

First, we can have noncontrastive, explanation-seeking questions concerning plain facts, for example of the form: Why does object *a* have property *P*? Was the fact that object *a* has property *P* the predictable consequence of some other properties of object *a*? These noncontrastive questions can have different motivations. One possible motivation is sheer intellectual curiosity (the desire to know how the fact 'fits into the causal structure of the world', to know how the fact was produced from given antecedents via spatio-temporally continuous processes). A more pragmatic motivation is the desire to have information that enables us to predict whether and in which circumstances similar events will occur in the future. The form these explanations of plain facts (answers to noncontrastive questions) have, can be to show in detail how the observed fact was actually caused, which implies providing the detailed mediating mechanisms in a non-interrupted causal chain across time, ending with the explanandum, or – considering the second motivation – the explanation can follow the covering-law model (see above).

Second, explanation-seeking questions can require the explanation of a contrast, for example, of the form: Why does object *a* have property *P*, rather than property *P0*?[9] (*P*-contrast). Why does object *a* have property *P*, while object *b* has property *P0*? (*O*-contrast). Why does object *a* have property *P* at time *t*, but property *P0* at time *t0*? (*T*-contrast). The explanations of contrasts can have a therapeutic function, or are motivated by 'unexpectedness'. They isolate causes that help us to reach the ideal (*P*-contrast) or to remove the observed difference (*T*- and *O*-contrast). Alternatively, they could be meant to tell us why things have been otherwise than we expected them to be. The form of a contrastive explanation enables us to obtain information about the features that differentiate the actual causal history from its (un)actualized alternative, by isolating the causes that make the difference; this information does not include information that would also have applied to the causal histories of alternative facts.[10]

By making the different possible explanatory requests explicit (cf. *O*-, *P*-, *T*-contrasts or a plain fact) the motivation and the explanatory information

required will be taken into account. It can be shown that one social fact can be the subject of different questions, and hence of different forms of explanation. Consequently, taking into account the explanatory question is not something of secondary importance, as it decides which form of explanation will be used. To be able to answer these different kinds of explanatory questions we will need different forms of explanation (relying on different causal dependency-relations) and here we find more limitations in Lawson's proposals concerning explanation.

Let me first mention that Lawson's proposals might not have too many problems dealing with the explanatory request involving *P*-, *O*- and *T*-contrasts, as he himself works with contrast, and states that the effects of causal mechanisms can be identified through formulating contrastives (see Lawson 1997a, 206ff).

Second, given my framework of explanatory pluralism, it is obvious that I do agree with Lawson's rejection of the covering-law model as a model for all explanations. This does not imply, however, that I support his drastic rejection of the covering-law model used by the mainstream economists. In my view, we cannot dispense with the covering-law model. Starting from a pluralistic stance, we should consider the possibilities and limitations of covering-law explanations of actions. On the one hand, they are poor instruments from the point of view of therapy for instance (one of the motivations we have linked to contrastive questions). The reason is that the covering-law explanation cites beliefs about causal relations that we want to retain in the ideal situation. Because covering- law explanations must aim at completeness (leaving out causally relevant factors can make the derivation relation collapse), the causes that are relevant from a therapeutic point of view are always put on the same line with causes that are irrelevant from that point of view. On the other hand, covering-law explanations provide us with information that contrastive explanations do not give us, such as the information that enables us to predict whether and in which circumstances similar events will occur in the future. Because they have to aim at completeness in order to guarantee derivability, they include all causally relevant factors (compare Weber, Van Bouwel and Vanderbeeken forthcoming). Although this kind of prediction is certainly not the only goal of the social sciences, it does form a part of it (for example, to control social outcomes, to be confident that proposed measures will have the intended effects) (see Cottrell 1998, 353–4). By strongly rejecting the dominant form of explanation in mainstream economics, Lawson might reject a form that could be useful in some circumstances, and that does not necessarily have to be incompatible with his form of realism.

Lawson's view on explanation (see above, 'I do argue that the primary aim of science and explanation is to identify and understand the underlying

structures, capacities, mechanisms, etc.') is very much determined by his focusing on ontology (and 'solving the ontological problem' seems to promise the solution of all problems related to explanatory practice). I do not want to question the importance of identifying mechanisms, but it is important to stress that different explanatory requests, different motivations for providing explanations of a social phenomenon or fact, ask for different forms and applications of the mechanism-idea (as good explanatory information should be the goal, rather than merely the confirmation of identified mechanisms – the bad habit that reminds us of a once-dominant covering-law model). Focusing on contextual and pragmatic needs shows that modifications of Lawson's explanatory framework should be considered, and raises the question of to what extent Lawson is a (explanatory) pluralist.

Before I turn to an analysis of Lawson's pluralism, I want to give a last example of the way in which Lawson's approach limits explanatory options in the social sciences. Consider the following quote of Lawson, in which his ontological convictions are related to the discussion on reductionism:

> 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. (Lawson 2001c, 179)

Here, Lawson starts from a very distorting (and monolithic) presentation of reductionism, being – according to him – the method linked to ontological individualism or reductionism. The entanglement of ontology and methodology is complete here! (For a critique of mixing up ontological and methodological arguments, see Van Bouwel and Weber 2002b.) The way in which Lawson here deals with reductionism, obstructs the attention that should be paid to different versions of reductionism. Here again we can distinguish different epistemic motivations and goals (for reduction, this time), ontological parsimony, decomposition, unification and correction, as is done by Daniel Steel (2004). Whereas Lawson seems to presuppose that one model of reduction should be accepted or rejected (a model in which all the above four goals are reached), we might consider versions of reduction where unification is achieved, but not necessarily ontological parsimony or decomposition. Or a version in which correction is the main motivation, and not the other goals. As such we might take into account what the goal of the reduction is, and whether the particular reduction reaches this goal. An approach in which a monolithic reductionism is opposed to anti-reductionism seems to be less fruitful as it leads to a winner-takes-all kind of debate. As I have argued, it is more recommendable to differentiate forms of

explanation and consider which form lives up to our epistemic needs in the most desirable way given the explanatory request at hand. The same can be said about the versions of reductionism. Settling the issue of reduction in a general way is unlikely to be successful; instead, it has to be addressed for specific social theories and explanations and specific individualist motivations, taking into account the context and epistemic motivations of the social scientist. As it is an issue that has to be decided case by case, a more contextual and pragmatic approach than the one developed by Lawson is needed.

In the analysis of scientific practice we might focus on the different epistemic interests and motivations for providing explanations or reducing theories, as I have suggested earlier. The debate between reductionism, on the one hand, and the nonreductionism defended by Lawson, on the other hand, would then no longer be fought between two (monolithic) camps in which winner takes all, but would shift to developing a framework that can tell us which form of explanation or theory can provide us with the explanatory information we are looking for in a specific case. Such a framework would help us understand the plurality of explanations and theories that we encounter in the social sciences, and guide us in optimizing explanatory practice and making maximum use of available explanations, depending on epistemic interests. This form of explanatory pluralism will still have to consider ontological issues, in order to secure the compatibility and complementarity of theories and explanations, but it will neither have to wait for the metaphysical battle to be ended, nor accept that explanatory options are limited *a priori*. Even forms of reductionism can go hand in hand with such an explanatory pluralism, as is convincingly argued by Daniel Steel (2004). In this scenario of explanatory pluralism, more attention will be paid to the actual practice of social scientists.

Are Lawson's Proposals Pluralistic?

In his 'Back to Reality' (2001b), Tony Lawson pleads for more pluralism in the academy and the opening up of the discipline of economics – criticizing the monism of mainstream economics. This will imply a return to variety and greater pluralism in method, according to him. Now that I have discussed Lawson's alternative to mainstream economics in the previous sections, the question should be raised of whether his contribution is pluralistic.

In order to start answering this question we will have to make explicit what kind of pluralism we are talking about. Lawson (1997b, 2001b) himself is not very clear about this. I will first (in a generous reading of Lawson) point out how he can be understood as being a methodological and

explanatory pluralist. Then, starting from a more demanding view on pluralism, I will formulate some doubts about Lawson's pluralistic stance.

In developing his critical realist methodology as an alternative to the mainstream economics methodology, Lawson does indeed add an alternative to the methodological options available in economics, does increase diversity and – in that sense – does contribute to more pluralism in the academy. Moreover, as I have discussed in earlier sections, Lawson does distinguish different kinds of explanation, so we might call him an explanatory pluralist.

But we might have a more demanding idea about pluralism: one where intellectual exchange and discussion are stimulated, where the focus is on complementarity (and an idea of when to use the complementary approaches), rather than on an isolated or non-communicating diversity. Does Lawson live up to these demands?

First, as we have elaborated above, Lawson's rejection of the mainstream mode of explanation, the covering-law model, cannot be counted as a good start for a pluralist; a pluralist would be interested in checking the compatibility and consistency of different modes of explanation. John Davis points in the same direction, when he questions heterodoxy in economics:

> Many of those who are the strongest proponents of methodological pluralism – heterodox economists – are entirely critical of neoclassical economics. Their motivation, I would conjecture, is not that their own theoretical approaches are also correct – a theoretical pluralist view – but rather that neoclassical economics is mistaken and misguided in its most basic assumptions, and that their own approaches remedy the deficiencies of neoclassicism – a theoretical monist view. (Davis 1997, 209)

So, according to Davis, the self-declared pluralists of the heterodoxy (and we can include Lawson here) formulate alternative methodologies that are not so much meant as complements, but rather as substitutions of mainstream economics. Referring to an earlier section of this chapter, we might ask whether a version of the covering-law model and some versions of reductionism cannot be considered as complements to Lawson's proposals, so that prediction and other motivations can be addressed, rather than be rejected.

A second important observation concerns Lawson's reading of mainstream economics; more specifically the suggestion that it is a monolithic and monist enterprise and does not pay attention to the pluralism present in mainstream economics. Esther-Mirjam Sent (forthcoming) illustrates the failure to achieve monism, and the recurrent pluralism, in the history of so-called mainstream economics. She shows that both the movement towards monism about theories, and the attempts to treat economic agents monistically, have failed:

> Monism about theories required an evaluation of 'the individual' as well as 'the social.' However, on the one hand, mainstream economics has no notion of the social other than the summing up over individuals. On the other hand, it cannot maintain a unique focus on the individual, because this would preclude complete explanation of competitive markets … For monism about economies, these findings resulted in an effort to populate economies with one representative agent. This effort to reduce differences of opinion resulted in major stumbling blocks, including a problematic connection between the 'representer' and the 'represented' as well as a lack of trade, which, supposedly, is one of the main foci of economy analyses. These difficulties resulted in a move towards game theory, which laid bare new problems with monism about economies. (Sent forthcoming)

The failures of the monist movement in mainstream economics that Sent is pointing at strengthen the case for pluralism, but leave us with the question: why does Lawson presume mainstream economics to be monist?

Third, concluding that mainstream economics, monistically presented by Lawson, is pluralist does not imply that looking for alternatives to the mainstream should be abandoned. It only points, once again, to the risk of throwing the baby out with the bath water. Presenting mainstream economics as a monolithic unity, as is done by Lawson, invites critics to reject it *en bloc*. As is obvious in Lawson's work, and as I have illustrated above in considering his proposals concerning explanation, Lawson's quest for heterodox economics is focused not so much on elaborating compatibility with mainstream (or neoclassical) economics, as rather on creating an alternative of his own, that tends to be substitutive, rather than complementary. (This alternative was established on – or wrapped in – an ontological reorientation, firmed up by a transcendental argument, which led to epistemological consequences, of which we might ask whether they are products of an ontological fallacy.)

Finally, Lawson presents himself as a pluralist against the mainstream monism, while the mainstream looks less monistic than presumed, and Lawson himself looks less pluralistic. Although it seems too early to conclude that Lawson is a monist, I want to pay attention to the risk of strategic pluralism. This term 'strategic pluralism', as used by Ron Giere, refers to groups that advocate pluralism as primarily just a strategic move in the game of trying to dominate a field or profession. Those in the minority proclaim the virtues of pluralism in an effort to legitimate their opposition to a dominant point of view. But one can be pretty sure that, if the insurgent group were itself ever to become dominant, talk of pluralism would subside and they would become every bit as monistic as those whom they had replaced (Giere forthcoming).

So, the analysis of the methodological and explanatory contributions of

Lawson point to some serious limitations in respect to pluralism. If they really want more pluralism in the academy, Lawson and the heterodoxy in general should make sure that they stay 'on speaking terms' with the orthodoxy and do not isolate themselves (or get isolated) too much.

Conclusion: Some Open Questions about Pluralism

Undoubtedly, pluralism comes in many forms; you can have pluralism about methods, models, perspectives, methodologies, explanations, etc. When Lawson pleads for a more pluralistic approach within the economics academy (compare Lawson 2001b), he may be considering any of these forms of pluralism. However, talking about pluralism needs to go hand in hand with establishing the compatibility and complementarity of the different components, if not you will end up with an 'anything goes' situation. I have been pointing out that Lawson tends to throw the baby out with the bath water, emphasizing the limitations but neglecting the (limited) possibilities of the mainstream approach. The pluralism defended by Lawson (wrapped in an ontological [fallacy] paper), and others on the part of heterodox economics (compare Sent 2003) needs further elaboration and a more differentiated dialogue with the orthodoxy in order to discuss the contours of pluralism. We need to avoid the heterodoxy becoming isolated, or (eventually becoming the orthodoxy) monopolizing the conversation and elaborating an alternative monism. Rather, questions about scientific pluralism should be addressed and debated over the whole spectrum of the economics discipline.

These questions might involve: Are the differences in goals and interests,[11] or the kind of problem one faces, concerning a particular economic phenomenon, the source of pluralism? Or, should pluralism be considered as a response to the complexity of the world? Is a complete account unachievable due to the cognitive limitations of human beings? Should the plurality of perspectives in economics be considered a temporary state? Are the components of pluralism mutually consistent? What should a framework of explanatory pluralism in economics look like? Can an advocate of pluralism avoid the 'anything goes' approach? What does pluralism imply about the relation between the approaches of philosophers to science and those of historians, sociologists, anthropologists, economists and rhetoricians?

I hope Tony Lawson will pay more attention to these kinds of questions in his future work, because at present he is just all too economic in his explanation of pluralism.

Notes

A slightly different version of this chapter was originally published in *Philosophical Explorations*, Vol. 7, No. 3, 2004.

1. An overview of the history of and contributions to post-autistic economics can be found on the movement's website www.paecon.net. I must mention that the topic of pluralism in economics has been discussed and defended earlier, for example, by a group of economists (organized by Geoffrey Hodgson, Uskali Mäki and Donald McCloskey), in a 'Plea for a Pluralistic and Rigorous Economics', published in 1992 as an advertisement in the *American Economic Review*, Vol. 82, No. 2, p. xxv; and in Salanti and Screpanti (1997). For an overview of the different defences of pluralism in economics, see Sent (forthcoming).
2. I refer those readers unfamiliar with critical realism to Roy Bhaskar – the founder of what is now a broad church within the social sciences – who considers critical realism as a third position, next to positivist and hermeneuticists' views, being 'a qualified, critical and non-reductionist, naturalism, based upon a transcendental realist account of science and, as such, necessarily respecting (indeed grounded in) the specificity and emergent properties of the social realm' (Bhaskar 1998, xiv). An introduction to the literature on critical realism can be found in Archer et al. (1998).
3. Retroduction is described as consisting 'in the movement, on the basis of analogy and metaphor amongst other things, from a conception of some phenomenon of interest to a conception of some totally different type of thing, mechanism, structure or condition that, at least in part is responsible for the given phenomenon. … It is a movement, paradigmatically, from a "surface phenomenon" to some "deeper" causal thing' (Lawson 1997a, 24).
4. To be fair to Lawson, I must mention that he considers the conclusion of the transcendental argument necessarily constituting contingent claims: 'It represents, in short, an investigation that necessarily takes contingent historical premises and specific social conditions, and aims to produce hypothetical and conditional conclusions; an investigation which can never be foreclosed, it is always open to elaboration and transformation' (Lawson 1997b, 19). However, his conviction is firm: 'The transcendental realist account in question, I submit, emerges as the most adequate, indeed uniquely sustainable, analysis that is available' (Lawson 1997b, 23).
5. The idea of an ontological fallacy in relation to critical realism has been used before by Wade Hands (1999, 181).
6. I do not want to defend neutrality as the norm to follow; I just want to unveil Lawson's choices.
7. I would call these kind of explanations *explanations of regularities or causal-dependency relations* (rather than explanations of 'enduring mechanisms'), but let me put that in a footnote for now.
8. Does the 'antecedently established knowledge of relatively enduring structures and mechanisms' have to be the subject of theoretical explanations before it can be used as a condition *sine qua non* for formulating practical explanations? How would practising economists evaluate this?
9. *P* and *P0* are supposed to be mutually exclusive.
10. Against scholars who are convinced that every explanation of a fact is an explanation of an explicit or implicit contrast (e.g. van Fraassen 1980, 130; Ylikoski 2001, 31; against Humphreys 1989, 137), I argue that explanations of facts are to be distinguished from explanations of contrasts both in structure and in motivation. Furthermore, I want to

emphasize that I do not consider the motivations mentioned here as the only possible ones.

11. Interests that can be understood as knowledge-interested, but might as well have a political basis, or be based on gender, or on social situation, historical context, forms of inequality, etc.

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PART III
Pluralist Practice in Economics

10
Beyond Talking the Talk

ALAN FREEMAN AND ANDREW KLIMAN

Critical Pluralism: An Introduction

This chapter argues that pluralism in economics requires formal rules of conduct to guarantee pluralism in research. These should provide for transparent and professional standards for research, presentation and editorial judgement.

The guiding principle of this reform is what we term *critical pluralism*. There are two key ideas in this. The first is that truth, or progress towards it, arises only if empirical reality is tested against a multiplicity of theoretical explanations of that reality. Pluralism is thus not a normative or ethically desirable adjunct to science but a necessary prerequisite to producing valid knowledge. Critical pluralism would, therefore, impose on the researcher the obligation to:

1. engage with, and critically examine, explanations alternative to her own;
2. clearly state the alternative presuppositions that differentiate her own explanation of observed reality from the alternatives considered;
3. clearly identify the evidence in support of her own conclusion;
4. clearly identify the evidence that supports the researcher's interpretation of the alternative views against which she tests her conclusions, in order to provide for a fair test.

This proposal differs substantially from prevailing informal standards. In our view, these informal standards exercise a suppressive function. Nevertheless, in our experience, when most economists, whether heterodox or orthodox, come to realize what we are really proposing, their initial reaction is usually hostile. This is at first sight surprising, particularly since heterodox views suffer the most from the suppressive effects of the current procedures extant in economics. It is doubly surprising given that the kind of measures we propose – for example, the right of reply, the right of appeal, safeguards against misrepresentation – are generally regarded in society as reasonable guarantees of objectivity and justice.

The hostility conveys, we think, something about the profession of economics. It tells us that the problem does not simply lie with orthodoxy, and will not be overcome by the mere existence of heterodoxy. It requires a different model of pluralism, something different from the present informal conception which consists, we believe, of an 'evolutionary struggle between competing orthodoxies' in which the practices that produce a suppressive orthodoxy are merely reproduced among its rivals. In essence, we argue that heterodox economists have made the mistake of reducing pluralism to diversity. This is the principal factor that perpetuates the continued dominance of orthodoxy.

The foundation of this system is what we term 'monotheoretic practice', by which we mean that each individual school of thought, no matter how heterodox and different from the mainstream, conceives of its role in economic debate as the working out of a single explanation within a single paradigm, attempting to demonstrate its superiority against both existing orthodoxy and alternative heterodoxies. In the 'market for economic theory', a successful product is conceived of not as an economist who engages with other ideas, but as a theory that defeats them.

We relate this to the cult of the economic expert, which conceives that the primary function of the economist is to determine, on behalf of others, the correct policy for these others to follow. The duty of an economist lies elsewhere, we believe, than merely judging which ideas are correct. It lies in showing where the ideas come from, and exhibiting the evidence in support of or against them, in order that others may themselves judge what is correct. Economic research, from this standpoint, should consist of a concerted battle to lay bare and make transparent the presuppositions on the basis of which economic judgements are made.

This battle is required not only so that economists themselves may be freed up to pursue truth; it is required so that non-economists can regain access to, and control over, what the economists are doing. We think the current practices of economics are rooted in its constitution as a distinct discipline; the separation of economics from social science as a whole has allowed it to be converted into a pseudo-science hermetically sealed against external standards of judgement.

We believe that economics is for this reason incapable of self-reform. Our reform programme is therefore addressed not primarily to economists but to the consumers of their product. Those economists who seek the reform of their discipline need to appeal outside their profession, to remove the freedom from external accountability to which economics alone among the sciences lays claim. When a bridge falls down through bad design, the engineer is held to account. When a patient dies under medical treatment, society is rightly entitled to question the competence of the doctor. Yet

when a country collapses while implementing an economic recommendation, it is the people and not the advisors who are blamed. Our proposed reforms are intended to correct this by imposing on the economists a long-overdue responsibility for the consequences of their own actions.

It may be thought that such a reform programme as ours is normative and ethical, rather than positive or scientific. We will argue that, on the contrary, without formal guarantees of pluralism and critical method, economics does not function as a science. Our guiding principle is to identify those practices that society must impose on its economists, should it require these economists to provide it with scientifically valid information.

Monotheoretic Heterodoxy: An Inadequate Informal Norm

Our own field of inquiry is value theory and Marx's critique of political economy. It may therefore be thought that our experiences and conclusions relate only to a narrow specialism – the value theory of Karl Marx – and that our chapter is therefore a discussion internal to Marxian economics. We believe to the contrary that the experiences we have had relate to economics as a whole. The problem we wish to address is the way that heterodox schools of thought themselves deal with difference and with a plurality of views. If we ask any economist, for example, whether Keynesianism as a whole, or Marxism as a whole, behaves any differently from orthodox economists towards differences in its own ranks, then if that economist is honest she would have to conclude that her own 'camp' behaves no differently than the currently dominant camp.

For instance, 'Keynesianism' actually constitutes a multiplicity of 'Keynesianisms', including dominant and suppressed views of Keynes. But the practitioners of the dominant view of Keynes behave, in relation to 'heterodox' views, with an intolerance equal to if not greater than the intolerance of neoliberalism towards Keynesianism of any variety.

We believe that this experience is general in economics. The perceived integrity of the 'schools' does not exist, and this extends well into the mainstream. Thus if one examines more closely the general approach that is now labelled 'marginalism' and which is itself generally identified with orthodoxy, one finds that this school has in fact marginalized the first marginalists, in that the Austrian approach, that emphatically rejects general equilibrium, contained in its ranks the founding fathers of marginalism, not least Menger himself. It is not unfair to suggest that resistance to pluralism is general throughout economics, regardless of whether it is orthodox or heterodox.

Thus there is a paradox which cannot be addressed merely by taking issue with orthodoxy: heterodoxies are just as lacking in pluralistic practice as the

orthodoxies against which they set themselves. The general intolerance of economics towards difference and pluralism is, alarmingly, *universal*: it is not confined to 'the bad guys'.

The conclusion we draw is that heterodox economics is the victim of a false model of pluralism; it conceives of it as a struggle of 'school against school' – Keynes *versus* Marx *versus* Sraffa *versus* neoclassical theory. It thus sees itself as engaged in a battle with orthodoxy to ensure that each 'school' is fairly represented. For instance, the principles of the International Confederation of Associations for Pluralism in Economics, an organization that has made a signal and important contribution to the cause of pluralism in economics, begin:

> ICAPE is dedicated to the idea that pluralism and intellectual progress are complements. This is not to say 'anything goes', but that each tradition of thought (Austrian, feminist, old and new institutionalist, Marxian, neoclassical, Post Keynesian, Sraffian, etc.) adds something unique and valuable to economic scholarship.

Of course, the exclusion of any 'school' is an obstacle to progress. But the difficulty does not stop, or indeed even start, at this point. The schools *themselves* are among the greatest enemies of pluralism. It is precisely within each individual school that we find the mechanisms through which difference is suppressed, in order to establish what is 'true' Marxism, what is 'true' Keynesianism, 'true' Institutionalism, and so on.

The false model of pluralism is, in the first place, wrong in its tacit assumption that each particular school represents a single view, rather than a multiplicity of views. The false camaraderie of the Marxists is a classic example. The major obstacle to expressing a different reading of Marx is the perception that, since Marxists are an endangered and beleaguered species, the expression of difference among Marxists is a dangerous practice, a threat to the existence of all. The irony is that it becomes possible to contend that the suppression of Marx's work is an act of illegitimate censorship (rather than, as is claimed, the reasonable rejection of an inconsistent system) only by repudiating the interpretation of his value theory that dominates Marxian economics, in favour of an alternative interpretation that eliminates Marx's supposed internal inconsistencies. This irony only reflects the wider problem: heterodoxy cannot be limited to a battle to substitute one received truth for another. It requires instead a different way of striving to arrive at truth.

However a deeper error is, in our view, involved. Our point is not to argue that economics is even more diverse than the pluralists have recognized, or to multiply indefinitely the number of different viewpoints that economics needs to take into account. Our point would still be valid even if there were only two different views in the whole of economics,

because it concerns the way in which the proponents of divergent views deal with each other's ideas. If there were only two schools in economics, they would still be acting in the most profoundly unscientific way if each saw its job as simply to develop its own viewpoint without engaging with the other.

In our view, as already mentioned, the fault in economics lies in the entire notion that the job of the economist is to judge, on behalf of the consumers of economics, what is a correct theory. We sustain that the function of economic research is, on the contrary, to lay bare the concealed assumptions behind all theories so that the consumers of our output may for themselves judge between them.

The prevailing informal norm is rooted in a reductionist syllogism: since, the heterodox researcher reasons, there is only one truth, there is therefore no need to examine many theories. All we must do is find the 'one true theory' and then apply it. From this standpoint, which is in fact shared by many heterodox economists, the only thing wrong with orthodoxy is that it has not found the truth. Therefore, the only necessary step to reform economics is to substitute the correct, heterodox single truth for the false, orthodox single truth.

This is the fundamental justification for the general practice in most heterodox economics itself, which by and large conducts itself as a multiplicity of orthodoxies. It perceives economic debate as a clash of a great variety of different views – each of which, however, is the property of a single school, the subject of a single article, the object of a single research programme, or the lifework of a single researcher.

Monotheoretic Practice and the Cult of the Economic Expert

Behind the prevailing informal norm just described, there lies an almost universal view of what an economics researcher 'does' which becomes clear only when we examine the practice of the heterodox. Essentially, economics 'research' is held to consist of applying one particular theory or idea to the study of some aspect of empirical reality. In consequence, the recipients of economic advice are told to adopt a policy because it is scientifically or technically correct. They are rarely told that it is one particular view, or given the option to adopt different policies. Far less are they permitted to require of the policy advisor that she offer a range of alternative options based on a range of alternative theories.

All heterodox economists recognize this as monotheoretical practice to a greater or lesser degree precisely because they are heterodox; that is, they can see that orthodoxy contains mistakes. In general, however, they do not

translate their insight into the errors of others into a reflection on their own practice.

In orthodox or mainstream economics, monotheoretical practice is concealed because researchers do not need to put a name to the theory they are using. An economist at the International Monetary Fund is not required to say, when telling a country what to do, that this is the neoliberal view of what the country should do. She, or more usually he, just says 'this is what economics tells us you should do'. Nevertheless such judgements contain a concealed and unstated bias, since by the word 'economics' the researcher or advisor actually means one particular theory – the current dominant paradigm.

Heterodox economists cannot so conceal their bias, so at least their product is labelled. Although this is an improvement, it is not sufficient. In order to say that one differs from the mainstream, and in order to make common cause for a different theory and a different policy, one has to put a name and a description to what one proposes. One has to call it 'New Keynesianism' or 'evolutionary economics' or 'National Systems theory'. This is, in and of itself, unobjectionable. However, each such school then proceeds to conduct itself as an orthodoxy-in-waiting, concentrating its work on the one hand on acquiring the necessary homogeneity to compete in the marketplace of ideas, and on the other hand on supplanting or replacing its rivals as the fount of truth. Monotheoretic practice, therefore, does not *simply* arise from the existence of orthodoxy; and cannot simply be eliminated by replacing it with heterodoxy: its roots lie in the way that all economists, including heterodox ones, organize their relations to each other.

Moreover, the notion that the function of economic research is to apply and develop a single theory is utterly rooted in the way that it is organized. A struggling heterodox PhD student, or grant applicant, has enough trouble mastering one author or body of theory. Surely it is an impossible burden to demand that the researcher should be conversant with and consider not only this theory but its critics and alternatives? How many times, at heterodox conferences, have we heard a presenter, when asked why she or he considered no alternative approach, simply state that she had no time, was not interested in it, or would 'leave the other approaches to those who were involved in them'?

Material Roots: The Practice of Economic Research

It is at this point that one begins to realize why opposition to a consistent pluralism is so deep-rooted. It arises, in our view, from a combination of two factors: the material circumstances of its practitioners, and the ideological bias imposed by the clients and patrons of economic analysis and theory. The

combined effect of these two factors is that economics, as a whole and notwithstanding isolated achievements, does not function as a science.

In this section we consider the first factor. The fact of the matter is that genuine pluralism is actually very difficult. To take the most basic point, it requires a 'genuinely pluralist' economist to be actively acquainted not just with a single viewpoint but also with the entire range of theory that could have a bearing on whatever she or he wishes to study. A core element of the International Working Group on Value Theory (IWGVT) guidelines states that presenters have to engage with, and study, as an integral part of the way they explain one particular theory, the views of its critics and the possible alternatives to it.

Faced with this demand, a substantial body of participants in our mini-conferences simply gave up and went back to doing what they had always done. Pluralism is a lot of work. If it were imposed as an editorial criterion, we make a rough estimate that around 90 per cent of currently published articles would be rejected on these grounds alone – never mind what would happen to research grants.

A system of rewards and sanctions that insisted on pluralism, as a criterion for funding, promotion and publication, would impose a very different structure on economic knowledge. But the evolutionary success factor for an economist is not to engage with the work of others, but either to work within the dominant paradigm or to differentiate herself from others, to construct an 'original body of work' which 'no one else is doing'. Indeed, in either case, it is in the *interest* of an economist to reduce the attention given to the theory and work of others, precisely in order to promote and sell her own particular product, her own particular theory.[1]

Thus to be a consistent pluralist is a daunting research option. It involves twice or three times as much work. It reminds one of Ginger Rogers's famous statement about her partnership with Fred Astaire that 'I did everything Fred did, but backwards and in high heels'. The necessity of pluralist practice is in fact revealed by the very different practice of business economists, who have to advise people with large amounts of money to spend as to their best course of action. In fact, the very best working economists, and the very best economic units *do* recognize the necessity of a multitheoretical approach, *do* triangulate from evidence, *do* examine a variety of explanations for what they hope to explain or predict.[2] It is a perfectly possible thing to do. But it is hard work and it is time-consuming, and as a consequence it is not seen outside of the closed world of the business decision makers.[3] Above all in research, in publication, and in those areas of the practice of political economy where vested class and political interests are most openly at stake – notably giving advice to countries and governments – pluralism is virtually absent.

But it goes further: pluralism is not merely difficult but career-threatening. Not least, the pursuit of many different theories, even those relating to the same practical problem, is often regarded as time wasting. At any time, a particular dominant theory is always in vogue; the alternatives are perceived as not only irrelevant but, more often than not, eccentric or downright suspect.

Thus, from the very start of a researcher's career, profound material conditions impel her or him at every step to a narrow theoretical specialization. Monotheoretical research practice is an intrinsic organizational factor in what is considered 'good' in economics.

To realize how deeply ingrained is this attitude of thought one need only consider some of the more common arguments offered against pluralism. How many times have we heard that a presenter is 'too busy' to consider the ideas of her critics? Or that a journal rejects a submission because 'this journal does not publish this kind of material – it should be published somewhere else'? This 'natural' framework of thought informs us that the most efficient use of research time is to engage in narrow specialization to the exclusion of all alternatives, and that the legitimate purpose of publication is to promote one particular set of views, consistent with the particular theoretical prejudices of the editorial board and its reviewers. Pluralism will be taken care of, it is argued, by an analogue of the competitive, liberal-market model: each school is free to publish its own material, present its own work, in its own conferences, its own schools and in its own journals.

But this is precisely to render economics a kind of microcosm of the liberal market. Schools of thought are thrown into a discreetly savage competition with each other: they compete for publication, for space at conferences, to control journals, for research grants, to win institutions, indeed to capture the ears of entire governments. At every stage the 'judges' are practising monotheorists, deaf to difference; the result is a battle of organized clans of partisans masquerading as disinterested schools of thought.

In consequence, judgements about what is bad or good are made on a basis that drives the judges towards subjectivism. What rational person will promote or fund a researcher whose interest lies in defeating her ideas? Journal articles and research proposals are submitted for consideration to the 'peers' of the author concerned, where 'peers' means 'other researchers that adopt the same label'. What advantage can any referee secure from consenting to the publication of an article that threatens to undermine her own research specialism?

Recognizing these material pressures, a genuine pluralism would insist on controls. It would submit any article to a range of referees both within and outside the specialism of the author. Among those referees, it would include

those who share not only the author's 'label' but also the particular paradigm within which that author works. It would expect the referees to judge the merit of the article not on the basis of whether or not they agree with its content or endorse its approach, but on the basis of whether the article complies with the norms commonly accepted among communities of scholars: are the conclusions provided substantiated with evidence that supports them? Are alternatives considered? Is the logic consistent in its own terms?

And if the referees failed in their duty to adhere to these standards of objectivity, genuine pluralism would provide an overriding accountability – just as do courts of justice – in the shape of formal appeal procedures in which it is legitimate to examine whether the referee has or has not done her or his job.

The very fact that this kind of pluralistic practice is considered 'too difficult' constitutes a verdict on what the profession of economics regards as good practice. To be sure, consistent pluralism would make good economics very hard to do. This is not, unfortunately, an excuse. It is hard to be a good doctor but society does not accept this as a justification for unleashing quacks to practise on the sick. It is hard to be a good chemist but we do not let alchemists wander at large, poisoning people and blowing things up. It is hard to be a good astronomer but we do not hand over command of space flights to the writers of Sunday horoscopes. Society demands standards of its professionals because it wants them to do their job, and it has every right to do so.

Bad economists are arguably a more serious social danger than bad chemists, engineers or doctors. They damage not only individuals, but entire countries and populations. It simply isn't a good enough excuse to say that it is too difficult to do it right. At the very least, if this approach is going to be adopted, the public is entitled to a few health warnings.

Ideological Roots: The Myth of the Evolutionary Selection of Ideas

We turn now to the central argument of this chapter, which is that critical pluralistic practice is not merely a normative question, a matter of 'tightening up' sloppy practices. The existing practices are in fact integral to a system of organization of economics that leads it to function not as a science, but as a theology.

It is for this reason that, we must insist, what is at stake is not personal injury or rights, but the actual content of the output of economics.

As explained above, we argue that the 'traditional' organization of the economics profession, the routine commonsense practices it considers 'normal and acceptable' when judging whether to publish an article, accept a

job application, promote a researcher, fund a project, or grant a PhD, are – when taken in their totality – a systematic instrument for suppressing a plurality of views and imposing conformity.

This much is widely recognized. We doubt there is a single heterodox economist who does not have some experience of the suppressive mode of functioning of economics. However, we want to go further. Until now, the standard reaction in heterodox economics has been, in our view, to 'play the game' – that is, to challenge the *output* of orthodox economics but not to challenge its *practices*. Pluralism, from the standpoint of this reaction, is then a luxury – something to aspire to or to lament the lack of – but not something to be implemented. Pluralism is to economics what monosodium glutamate is to some Chinese cooking. It is generally held to improve the taste, everyone wants it when it's on offer, but it is probably injurious to health, and the true master chef has no need of it.

Our central thesis is that pluralism is not the condiment but the main course. *Because economics is not pluralist, it is not scientific.* Thus at stake is not just whether economics is 'nasty' or treats people badly, but whether its content is correct. The organization of economics, we have argued, constitutes an unconscious reproduction of the model of the liberal market. Each school sets out its stall, marshals its supporters, and competes for fame and fortune, and may the best school win.

Indeed this model is celebrated. It is held to embody liberal virtue, since anyone has the right to speak; it is held to embody market principles, since the ideas must compete for attention; and to select for truth, since on Darwinian principles the fittest, and therefore the best, must surely emerge.

But there is no guarantee that an evolutionary struggle will produce a superior outcome by any other criterion than survival. Economics itself furnishes a counterexample: the competition of currencies which, as we know from Gresham, is an evolutionary system in which Bad drives out Good.

The notion that the 'competition of schools' must, through some evolutionary process, lead to the selection of scientifically superior ideas contains a great error. It is assumed that the 'fittest' economic theory must necessarily be the most truthful. But the evolution of ideas in economics selects not for truth, but for political acceptance, above all by those classes in society who fund it.

The empirical evidence is strong. Throughout the history of economics, employers, financiers and other privileged classes have constantly weighed in on the side of those economic ideas that offer a rational justification for their own particular privileges, over those ideas that offer a general explanation for all the workings of a capitalist economy. Theories that explain that differences in wealth between wage workers and property owners are the natural state of the world, that high wages are incompatible with growth,

that unemployment arises from the choices of the unemployed, or that neo-liberal precepts are the surest guarantee of economic welfare, begin with an evolutionary advantage which is transparently obvious from the ideas to be found at the top of the profession and in its elite institutions.

Are the results nevertheless in some sense 'true?' This hypothesis is supported by scant evidence. In the words of Paul Ormerod (1994):

> Economists from the International Monetary Fund and the World Bank preach salvation through the market to the Third World. … Yet economic forecasts are the subject of open derision. Throughout the Western world, their accuracy is appalling. Within the past twelve months alone, as this book is being written, forecasters have failed to predict the Japanese recession, the strength of the American recovery, the depth of the collapse in the German economy, and the turmoil in the European ERM.

One has only to consult, for example, the IMF's own external audit on its role in Argentina, to realize that even within economics itself, the most serious doubts prevail, at least in private, about the accuracy of its predictions:

> The International Monetary Fund's handling of the crisis in Argentina three years ago almost certainly deepened a recession that threw millions of Argentines into poverty and sparked political chaos throughout the country, according to a report released yesterday by the IMF's internal audit unit. By overlooking Argentina's growing indebtedness in the 1990s and continuing to lend the country money when its debt burden had become unsustainable, the fund significantly contributed to one of the most devastating financial crises in history, the report concluded. The crisis peaked when the Argentine government defaulted on nearly $100 billion in debt to private creditors and had to abandon the 'convertibility' system that pegged the peso to the dollar at a one-to-one rate. The ensuing crash led to an 11 per cent decline in Argentine output in 2002, sent the jobless rate soaring and toppled a series of presidents in a country that the IMF had once hailed as a model of free-market reform and development. (Blustein 2004)

Nor can it be seriously maintained that the basis for such doubt is limited to particular episodes or problems, as if only minor corrections or improvements were necessary to correct fundamental errors that lie behind the mistakes that economists habitually make. There are very general phenomena, such as the persistence of famine in the midst of plenty, or the long-term growth of international inequality, that economics *repeatedly* fails to explain.

Empirically, it is incorrect to hold that truth necessarily emerges from the evolutionary competition of ideas. Ideas can evolve backwards as well as forwards. The simple Darwinian analogy does not hold.[4]

The evidence of economics's own output is thus sufficient to cast serious doubt on the prejudice that the liberal market in economic ideas selects for truth.

There are also sound theoretical reasons for believing that these empirical failures are not at all accidental. A careful study, which is beyond the scope of this chapter,[5] of the actual outcome of major paradigm struggles in economics shows that its selection process is functional. It acts to prevent the emergence of modes and topics of inquiry, of theoretical frameworks or paradigms, that, to put it crudely, risk representing material interests as they truly are.

Even without a detailed historiography of economic thinking, we can appreciate the specificity of economics by contrasting its output with that of many other social sciences. Historical or sociological inquirers, including historians of economic thought itself, are given at least some training in identifying the material interests behind the various theoretical ideas that compete for our attention. They identify the losers and gainers from the policies arising from such ideas, and seek to bring to light the classes of society that, owing to the benefits they will receive, have acted to secure the acceptance of these ideas and policies.

Identification of the interests at stake is an uncomfortable outcome for these classes because it makes clear the subjective basis on which such ideas have been promoted. This is why economics has become the social science of preference. In contrast to history, sociology, etc., its specific ideological function – in a certain perverted sense, its great ideological achievement – is to disguise the material origin of theories and to present the unequal benefits arising from policies based upon them as natural economic necessity. It is therefore not at all surprising that the inventors of theories that achieve this outcome should find themselves rewarded with endorsement, employment, promotion, publication and, indeed, acclaim.

On both empirical and theoretical grounds, there are compelling reasons to believe that the selection procedure of economics is unsound. It is for this reason that we argue that, without pluralism, economics cannot be considered scientific.

Can Economics Reform Itself?

Is the output of economics inevitable? Can a pluralistic practice yield better outcomes, a superior selection process? The answer is 'yes but …' It is possible that the outcome and the process can be improved. But economics will not reform itself. Even more specifically, no movement of economists alone, however well intentioned, can achieve reform. Reform, we believe, requires conscious organization because scientific practice requires a continuous battle with the anti-pluralistic tendencies of the profession and discipline of economics, tendencies that, given its social role and function, come naturally to it.

To explain why, we focus on some essential differences between the natural and the social sciences. In the natural sciences, the conception of evolutionary selection through the competition of schools probably works, most of the time and with some (rather important) exceptions. By a crude and uncritical analogy, economists assume that what works for natural scientists must also work for them.

As we have already indicated, it is erroneous in any case to assume that an evolutionary law will always work to improve. It is particularly erroneous to suppose that an evolutionary process will work in the same way for the natural sciences as it does for the social sciences and above all for economics. The outcome of an evolutionary selection process depends on the criterion and process of selection. Evolution in a predator–prey habitat produces ferocious wolves and fleet deer, but animal husbandry produces tame dogs and bucolic cows. The two selection processes produce different outcomes because they use a different mechanism and apply different criteria. This happens in turn because their objectives and functions are not the same.

The criterion of success for a natural science theory is empirical. It is that of *prediction*. Although it is true that the selection process of the natural sciences is, as Kuhn has noted, tortuous and meandering, nevertheless this process ultimately rejects those theories whose predictions persistently fail to conform to a sufficiently widely observed reality.

This is probably not due to the exemplary intentions of the natural scientists. It arises from the social function of science in a capitalist economy. Left to their own devices, it is perfectly possible that natural scientists would conduct themselves little differently from the economists and indeed, in earlier periods of history, there is strong evidence that they did so.

However, a capitalist economy, in which successful competition of capitals demands the successful implementation of technology, imposes a powerful objective constraint, independent of whether the scientists behave like conscientious Popperians or Kuhn's prejudiced 'normal scientists'. Scientific theories are desired by capitalist society because they give rise to products and processes, and these are required to work. This severely limits the selection of bad ideas: theories that lead to bridges or buildings falling down, or aeroplanes falling from the skies, have a limited shelf life.

The triumph of the modern natural sciences was the outcome of a political struggle of the rising urban capitalist class against the aristocracy. Once the new capitalist class had separated the direct producers from the land, it needed to unite them with factories and technology, and so needed to acquire dominion over nature in order to organize production. It was not in the interests of any but the landed classes that sound theories, which would lead to ever-expanding increases in productivity, should be rejected simply because they were incompatible with aristocratic privilege. The

capitalist class therefore looked with great favour on, and indeed participated directly in creating,[6] a selection process for natural scientific inquiry, on the basis of Enlightenment principles of abstract reason, to secure the greatest opportunity for themselves to reap the technological benefits. The selection process of the natural sciences is thus driven by the overriding compulsion to accumulate through technical advance, imposing on it a distinctive competitive pluralism. Faced with a theory with difficult religious implications that makes more profit, capital generally chooses it regardless of theological niceties.

In short, the selection process in the natural sciences is, possibly against the will of the natural scientists, *intrinsically pluralistic*. What we mean by this is that the sciences are organized in such a way that, in the course of their quest to explain natural phenomena, observed reality is tested against *a wide range of possible theoretical explanations* of that reality. In particular, empirically successful theories are not excluded *a priori,* as in economics, on allegedly methodological grounds. In short, potentially successful theories are not as a matter of course excluded on ideological grounds from empirical testing.[8]

Because of its different social functions, this is not the case with economics. As we have noted, the crucial social function of economics is to offer a theoretical basis for the policies that classes and politicians seek to implement, in which the subjective interests of those who will benefit from the policies are concealed, and the subjective intentions of those who promote the policies are purged, and in which both are disguised as objective social necessity.

Because of this difference in social function, the selection criterion for economic ideas differs from the selection criterion in the natural sciences. The selection criterion in economics is certainly not success in prediction; almost all practising economists agree on the unreliability of their own forecasts. The profession is instead dominated by *methodological* criteria of selection.

When economic theories are disqualified, this is rarely done on empirical grounds. Instead, the ostensible criterion of selection in economics is 'logic'. The first recourse of an economist seeking to promote her own theory, or attack that of a rival, is that her theory is 'logically coherent' but the rival's is not.

The first problem with this practice is that, while logical coherence is necessary for truth, it is not sufficient. A theory can be completely consistent but false. Most religions have an extremely logical and consistent structure, but this does not lead to the selection of predictively accurate religions. Second, and more important, economists commonly disqualify competitor theories as 'logically inconsistent' even when the latter conform to all axioms of Aristotelian or formal logic. What they mean by 'logically inconsistent' is that *their own* theoretical premises are incompatible with the conclusions of the

competitor theory. In other words, the competitor theory is disqualified on purely *method*ological grounds. This is so especially when it excludes or refutes a premise that the economist holds to be self-evident. In particular, as a hundred years of economic debate have shown, any theory that leads to the conclusion that capitalism's economic crises are endogenous and endemic – rather than the result of imperfect markets, imperfect information, exogenous shocks, and the like – provokes the most violent existential angst among those whose lives and careers are organized around the principle that this is impossible.

To take an extreme but exceedingly influential example, Reder, himself a member of the University of Chicago economics department, noted:

> Chicago economists tend strongly to appraise their own research and that of others by a standard which requires (*inter alia*) that the findings of empirical research be consistent with the implications of standard price theory. … [They shun] alteration of the theory to accommodate behavior inconsistent with [it]. … [In the Chicago PhD programme, students'] answers must conform to definite criteria which are the fundamental characteristics of [the Chicago School], e.g.: competitive markets must clear, decision makers must optimize, money illusion must be absent. However imaginative, answers that violate any maintained hypothesis of the paradigm, are penalized as evincing failure to absorb training. … An acceptable dissertation … [provides] an explanation of some empirical phenomenon. … 'Explanation' means either a demonstration that the phenomenon is compatible with the underlying theory, or the provision of such extensions of the theory as may be required. (Reder 1982, p. 13, pp. 19–20)

Proponents of rival paradigms also try to enforce such practices, albeit perhaps with somewhat less rigour. This methodological monism ensures that a dominant and successful theoretical paradigm can, and will, suppress and eliminate its rivals without the embarrassing requirement, imposed on the natural sciences, of testing the ideas of these rivals against reality. Moreover, on the rare occasions when an empirically superior theory does overcome an orthodoxy that has excluded it, closer examination generally reveals that this is not the outcome of the internal processes of economics, but the intervention of external political forces for whom the prevailing orthodoxy has become so completely dysfunctional that it no longer serves any useful purpose.

Examples of this in economics are legion. The extraordinary inappropriateness of 'official' opinion regarding Argentina's hard currency peg is only the most extreme example of the practical impact of neoliberal policies whose triumph is widely recognized to owe much more to political expediency than to empirical accuracy.

Keynes's own battle, against the empirical background of the greatest slump in history, simply to secure acceptance for the theoretical possibility of an 'unemployment equilibrium', was won only through the ascendancy of

interventionist political currents in the face of the social chaos provoked by this same slump.

A further instructive example is the course of the post-1945 debate on minimum wage legislation and the theory of the labour market. As Manning (2004, pp. 5–6) points out, early founders paid careful attention to the fact that the relation between employer and wage earner is one-sided or 'monopsonistic' – the employer functions as sole purchaser and is therefore able to set wages.

Nevertheless, the subsequent evolution of labour market theory firmly established, over a period of some fifty years, the prejudice of perfect competition in the labour market – notwithstanding such empirically absurd predictions as that a one-cent cut in the wage will lead all existing workers to quit:

> The claim that labor markets are, in the absence of outside intervention, pervasively monopsonistic, probably comes as something of a surprise to readers of labor economics textbooks. Table 1.1 documents the number of pages devoted to a discussion of monopsony and the total length in a selection of popular textbooks. As can be seen, monopsony does not figure prominently and, where it is mentioned, the discussion is generally not favourable. …The first two volumes of the *Handbook of Labour Economics* (Ashenfelter and Layard 1986) contain only two references to monopsony out of a total of 1268 pages … the three subsequent volumes published in 1999 (Ashenfelter and Card) contain three references in 2362 pages. (Manning 2004, p. 6)

This prejudice is only now being subjected to serious empirical scrutiny, at precisely the time when a growing army of casualized and poverty-stricken labourers has become a sufficient social threat for policy makers to recognize the pragmatic necessity to provide some minimum protection. In contradiction to the standard predictions of perfect competition, the UK Low Pay Commission (2005, p. vi) concluded:

> The National Minimum Wage was introduced on 1 April 1999, with an adult rate of £3.60. Its introduction benefited about one million low-paid workers and had no measurable adverse effects on employment or inflation. … From 1999–2002 the minimum wage was increased roughly in line with average earnings, reaching £4.20 in October 2002. These increases also had no significant adverse effects and indeed employment continued to grow strongly in the sectors where low pay is most prevalent
>
> … the Commission, in its fourth report published in March 2003, concluded that it was appropriate to increase the effective level of the minimum wage, increasing it faster than average earnings for a number of years, and thus benefiting more workers. In line with our recommendations, the adult minimum wage rose to £4.85 in October 2004, an increase of 15.5 per cent over two years in which average earnings increased by nearly 8 per cent. We also indicated in our fourth report that we believed that some further increase above average

> earnings would likely be required in subsequent years to arrive at an appropriate long-term level. This report analyses the impact of the significant upratings over the last two years and considers the appropriate path of the minimum wage over the next two. Our analysis suggests that the upratings have largely been absorbed without adverse effects. Employment continues to grow in most low-paying sectors and the impact on wage bills and profitability appears sustainable.

The workings of the evolutionary process are perhaps clearest of all in the economics profession's treatment of the ideas of Marx, which it systematically suppresses, and virtually never considers. This has nothing to do with the accuracy of Marx's predictions, many of which are widely conceded (though generally in private) to be rather good.[8] Marx and other Marxist theorists have contributed predictions about long-term trends in the world economy that have stood the test of time somewhat better than most neoclassical theory. The persistence of business cycles and the existence of imperialism are both now recognized modern features of the twenty-first-century world, but both were decreed, at one point or another, by the most eminent writers of the second half of the twentieth century to be antiquated dogmatic fictions.

Marx's ideas are ignored and suppressed because of their socially unacceptable conclusions, above all the conclusion that capitalism contains within itself contradictions that it cannot solve. Of course, this is rarely if ever acknowledged openly. The conventional justification for economists' suppression of Marx's work is rather that it is supposedly riven with logical inconsistencies. However, the 'logical' inconsistencies reduce to the fact that Marx's theoretical conclusions cannot be deduced from the models of his critics. Here again, the selection criterion is wholly methodological – but in this case it is wielded principally not by members of the Chicago School, but by Marx's Sraffian and Marxist critics.

Thus, theories that are perfectly internally consistent, and whose empirical predictions are as good as or better than the alternatives, will not be considered because, from the standpoint of rival theories, they appear illogical and, therefore, not worthy of consideration. This has two vital consequences. First of all, it provides no mechanism for selection against falsehood since, as we have noted, a system can be as perfectly wrong as it is perfectly logical. Second, it contains no mechanism for selecting for truth, since economics fails to conduct the most important operation that distinguishes the natural sciences: it does not test observed reality against the full range of theoretical alternatives available to explain that observation. Third, to the extent that objective external constraints influence the selection mechanism, they operate only to secure theoretical support for policy conclusions that invariably reflect – and disguise – the partisan interest of particular, and generally privileged, classes.

Why will this not be challenged from within the profession? Because

economists reap no benefit from testing rival ideas whose conclusions challenge their own beliefs and theories. Worse still, economics has successfully secured itself against the criticism of other disciplines. To the other social sciences it explains that it is unique in being a 'hard' science and can be judged only by the standards of the natural sciences. It is thereby hermetically sealed off from external audit or critique.

Because of this special status, we may describe economics as 'naturally anti-pluralistic', operating according to informal rules of conduct in practice accepted uncritically by the generality of economists whether orthodox or heterodox. These unwritten rules, what may be termed the 'accepted common-sense norms' of economics, are driven by a social process of selection and merely formalize this social process. They contain, therefore, no guarantee of truth.

The crucial issue does not concern the minor decisions economists make when studying this or that practical question – what will happen to interest rates, whether inflation will be worse or better in the next year, how many people will be employed in this or that sector, and so on. The crucial issue instead has to do with the major judgements about the principal types of theory and the principal methodologies that economists will use, in the course of making their countless minor decisions. Once a truthful theory has been 'deselected' by its proponents being denied publication, grants, resources of all kinds, it is no longer accessible to practitioners. It is precisely for this reason that the suppressive function of existing economic practice is its most consistently anti-scientific instrument; it is precisely for this reason that this suppressive function is the key thing to challenge; and it is precisely for this reason that only a consistent pluralism can actually be scientific.

The foregoing analysis lays the basis upon which, we believe, all alternatives to the present system should seek to organize.

First and foremost, it has to be recognized that the success of the natural sciences arises precisely from testing a multiplicity of theories. Economics should be no exception. Pluralism is thus not a luxury but a *sine qua non* of progress in economics. Second, success will not be achieved by setting economist against economist and school against school. It requires cross-paradigmatic engagement as the prime commitment of theoretical activity, as well as a system of practices, rewards and sanctions that promote such engagement.

The prime task of an economist, in confronting a variety of theories, is neither to ignore nor to defeat rival theories by means of arguments based on 'logic' alone, but to bring about an *understanding* of these theories and their implications, to make both the material origin of these ideas and their practical consequences available for empirical verification by external agencies. Until now, theoretical selection has preceded empirical testing. This relation has to be utterly reversed. Theories must be tested empirically

before being rejected, and they must first be understood properly before they can be tested properly.

Finally, it is necessary to organize to achieve these aims: they will not be secured spontaneously. The purpose of such organization is to combine internal critique, which provides practical institutional defence of critical pluralism, with external judgement, to place economics back where it belongs in the body of the social sciences as a whole, and to subject it – and its fellow social sciences – to the objective judgement of society as a whole.

Notes

1. Our claim that the anti-pluralistic practices of economists are rooted in material interests is broadly consonant with Martin's (1998) 'interests model' of the suppression of dissent in science generally. His analysis is informed by a quarter-century of study and firsthand knowledge; see his 'Suppression of Dissent' website, http://www.uow.edu.au/arts/sts/bmartin/dissent/. Kliman (2006, Chapter 12) draws on Martin's model to account for the suppression of Marx's critique of political economy by Marxist and Sraffian economists.
2. This is the general practice (but not the universal practice, as the spectacular failure of the Long-Term Capital Management hedge fund shows) of the best financial economists, particularly when, instead of advising governments, they find themselves and their advice accountable to employers that spend money.
3. This is one reason why, it has to be said, some of the most interesting and practical economics comes out of business schools. Of course an enormous amount of nonsense comes out of business schools, too, but this may be a price worth paying.
4. How many economists follow the precept that Darwin himself writes into his autobiography, which Freud (1938, p. 102) uses to illustrate his principle of the 'pain motive for forgetting': 'I had during many years followed a golden rule, namely, that whenever a published fact, a new observation or thought, came across me which was opposed to my general results, to make a memorandum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favourable ones'? Is it then surprising that economics conveniently 'forgets' almost every theory, however factually well-supported, that might call into question its deepest-rooted prejudices?
5. See Freeman (2004).
6. See for example Uglow (2002).
7. The more a particular branch of science impinges on our internal explanation of society, and the farther its field of inquiry is from the study of a purely external nature, the more it is threatened by criteria derived from ideology, as can be seen from the progress of controversies in biology.
8. See for example Cassidy (1997) 'Many of the contradictions that [Marx] saw in Victorian capitalism and that were subsequently addressed by reformist governments have begun reappearing in new guises, like mutant viruses. … He wrote riveting passages about globalization, inequality, political corruption, monopolization, technical progress, the decline of high culture, and the enervating nature of modern existence – issues that economists are now confronting anew, sometimes without realizing that they are walking in Marx's footsteps.'

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11
In the Economics Classroom

PETER EARL

> And so to the University course in economics. The first task of the University teacher of any liberal art is surely to persuade his students that the most important things he will put before them are questions and not answers. He is going to put up for them a scaffolding, and leave them to build within it. He has to persuade them that they have not come to the University to learn as it were by heart things which are already hard-and-fast and cut-and-dried, but to watch and perhaps help in a process, the driving of a causeway which will be made gradually firmer by the traffic of many minds. (Shackle 1953, p. 18)

The view of the role of the economics lecturer that George Shackle offered towards the end of his inaugural lecture at the University of Liverpool comes as no surprise to economists familiar with his work. His writings on the implications of uncertainty for economic analysis shatter hopes of constructing universal thought schemes and deterministic models. They point instead towards the creative use of our imaginations to construct rival scenarios and then debate their plausibility and implications (Jefferson 1983; Loasby 1990), and to the study of the stereotypes or drills that people use to cope with the ordinary business of living (Shackle 1963, p. 18). However, over fifty years after Shackle's inaugural lecture, few economists are offering the kinds of training that Shackle advocated. What most economics students receive is not a guide to contending perspectives (Barone 1991), which highlights disputed areas, but a thoroughly neoclassical training that focuses on determinate solutions.

This probably reflects factors such as faculty politics, perceived unavailability of suitable texts, claims that intellectual standards will inevitably be lowered if attempts are made to 'cram a quart into a pint pot', and a belief that undergraduates simply will not be able to cope with such an approach to teaching. Most academic economists do not try to find out whether all these barriers really exist and are insuperable; they simply take them for granted. This may reflect incentive structures that reward success in publishing in prestigious mainstream journals (see Earl 1983; Colander 1991) rather than

time invested in innovative teaching methods. Some of the failure to teach non-mainstream modes of thought may reflect a blissful ignorance of the existence of alternatives but insofar as mainstream economists are knowingly failing to alert their classes to schools of thought that are not the 'economics equivalents of the "flat earth society"' (Weintraub 1985, p. 1118), then serious questions are raised about their academic integrity (see Parvin 1992).

This chapter focuses on how students may be expected to behave if actually presented with a non-deterministic approach to the teaching of economics: in other words, I will be exploring the practical feasibility of Shackle's vision of what might happen in the economics classroom. My interest in this issue was originally triggered by Jefferson's (1983, p. 146) reports of resistance to scenario planning within the Shell Petroleum Company: many of Shell's managers found it hard to accept the idea of a system of planning designed to highlight uncertainties, for their normal way of viewing planning was as an activity intended to reduce uncertainty. If senior managers experienced such difficulties, we might expect similar problems in the economics classroom due to tensions between a non-deterministic approach to teaching economics and the stereotypes used by students to assist their learning. I begin by showing that such fears are indeed well-grounded, but I then consider ways in which university teachers of economics might overcome these difficulties. To do this I call partly on my own sometimes traumatic experiences as well as on research associated with the work of Harvard educationalist William G. Perry Jr (1970, 1981, 1985), whose vision of the role of a liberal arts education bears an uncanny resemblance to that expressed by Shackle in his inaugural lecture.

During this chapter, a willingness to make *commitments* to particular ideas will be taken as indicating a high level of intellectual and ethical development. Non-deterministic teaching encourages students to choose for themselves particular ways of sizing up economic problems in the light of (1) their new-found knowledge of commitments made by economists who have gone before them and (2) criticisms offered by members of rival schools who have thought long and hard about what is going on in their opponents' camps. If students are offered this knowledge they are not merely being given an honest guide to the difficulties that economics presents, they are more likely to become conscious of the core notions that provide the basis for mainstream economics, not merely its rivals. For example, it may take a discussion of the idea of satisficing behaviour to clarify what is involved in constrained optimization, or an outline of 'normal cost' price theory to render transparent the market-clearing philosophy embodied in marginalist approaches to pricing (see Lee 1984).

Such an approach to teaching economics does not require that the lecturer maintains a detachment from any particular school of thought in economics.

On the contrary, as we shall see, students may benefit from being taught by economists who are candid about the factors that have led them to make particular commitments. So long as lecturers are committed to the idea that it is desirable for students to be exposed to contending perspectives and try to present alternative points of view in ways that 'do unto rival viewpoints as they would like to see the latter's proponents do unto their own', multi-paradigm teaching is compatible with lecturers having made commitments to particular approaches to theorizing about particular situations. In fact, an economist who feels particularly attracted to one school of thought may be a particularly exciting teacher not only of its strengths and weaknesses but also of those of its rivals: as Shackle (1967, pp. 295–6) argued, 'Only a theory that one has come to terms with can be taught with zest and conviction; but this deep assessment of a theory implies a consciousness of its weaknesses and possible alternatives, as well as of efficiency and beauty.'

Perry's Scheme of Cognitive and Ethical Development

When students perform poorly, their teachers often blame a lack of effort or ability. After interviewing many undergraduates at Harvard, William Perry offered a rather different perspective: the problem may be that the ways in which students are setting about trying to learn may be grossly out of line with the views of learning on which their teachers are building their courses. Perry identified a sequence of different ways of thinking through which students tended to progress *en route* to the sorts of way of making sense of the world used by their teachers. Some might be well advanced along this road even by the time they matriculated; others might fail to go far along it even after three or four years of diligent study. It may be summarized as follows:

Level 1: Dualism

The least mature stage in Perry's scheme – and one that Perry (1981) only occasionally observed – is a worldview in which students see things in dualistic terms: everything is expected to be either black or white. Dualistic students see themselves rather as empty vessels waiting to be filled with the truth and having a duty to pay attention in lectures, taking down what their teachers say and afterwards memorizing it. They expect that, if they work hard, following up every reading instruction and learning the Right Answers, they will be duly rewarded at examination time for demonstrating to their teachers their diligence and grasp of the Truth. They see their teachers as experts who know what is correct and whose role therefore is to present the Truth to them in a way that makes it easy to grasp; this is seen to

involve clearly structured lectures and assignments, with the teacher ensuring that the class is under control. Lecturers who are repeatedly interrupted by students asking questions and who cannot always deal with these questions are seen by dualistic students as poor teachers: if these lecturers knew their subjects properly and were able to present the material more clearly they would not be causing such confusion and allowing such interruptions to disrupt the process of transferring the truth from themselves to the students.

Dualistic students have a great deal of trouble seeing the point of class discussions, for they do not see their peers as knowing any more than themselves. They get very frustrated by abstract learning experiences that do not seem to have clear answers, and if asked to work out answers for themselves they aim their efforts largely at finding out what the teachers expect. They believe that better grades follow automatically if they provide more information, for grading is simply a matter of counting up the amount of correct information that they have supplied.

Level 2: Dualism questioned

It is not surprising that extreme Type 1 dualism is rather rarely observed. Sooner or later, students are likely to encounter lecturers who point out differences in opinions in particular areas, who seem to wish to challenge things that are in textbooks, and who seem happy to leave their classes with unresolved problems rather than sets of answers. Students at the second stage are alert to this phenomenon but seek to resolve its apparent contradiction with their dualistic style of thinking by inferring that some Authorities must be right and others wrong. If those that they judge not to be frauds are nonetheless giving them problems rather than answers, they are prone to infer that the Good Authorities expect them to be able to learn from these problems what the right answers are. This cognitive strategy becomes rather difficult to sustain once some of the Good Authorities admit that they do not yet know all the answers and reward highly students whose answers to identical questions differ greatly: if these lecturers are now not to be thought of as frauds, then a non-dualistic way of looking at things is needed to make sense of their behaviour.

Level 3: Multiplicity, but only for the moment?

The initial way of seeing differences in opinion as legitimate rather than as a sign of differences in competence or honesty of teachers is to see them as states of affairs that will eventually be resolved by the experts working out what the Truth is. (When Shackle wrote the words at the end of this chapter's epigraph – referring to 'the driving of a causeway which will be

made gradually firmer by the traffic of many minds' – he might be said to have been taking this perspective himself, though with a long-term time horizon.) Though this may at first restore the students' faith in their teachers, they then start realizing just how many areas are currently disputed, how long some disputes have been raging, and how far from resolution many disputes seem. This leads to a puzzle: if supposed experts can keep on arguing amongst themselves and cannot tell their students what the truth is, then why should they be seen as experts in their fields? It now becomes rather difficult to study merely by memorizing facts. Not merely is it unclear what the facts are, students also become worried that they could do poorly if they do not understand how their teachers think. Grading can no longer be seen merely as counting correct pieces of information. The Type 3 student begins to wonder whether teachers can be trusted to be fair if the latter hold strong opinions on issues about which they set questions.

Level 4: Anything goes?

Once students can no longer judge their teachers as expert authorities on the basis that they know the Truth, they tend to start thinking that there is *no* basis for saying who is an authority, and that their own views might be at least as good as those of their teachers. They grow more confident about challenging what their teachers say and see greater value in class discussions in which they can share with their peers their ideas on how particular issues might be seen. But they also start to see that although everyone may have a right to his or her own opinions, this does not imply that no one can be wrong, for some opinions may be ones whose proponents seem unable to support them with facts and reasons. They start seeing that in some courses teachers are rewarding them not for coming up with the Right Answer but on the basis of whether they are able to think about things in particular ways – whether they have mastered particular concepts, rather than whether they have memorized particular facts – and can justify the conclusions that they reach.

Level 5: Relativism

The next discovery is that this style of thinking pays off in other areas of learning and life. Students begin to see that in interacting with other people it helps to try to understand how they are thinking – where they are coming from and what is important to them. They see that their teachers try to do this too, but that the latter do not treat every point of view as equally valid: what is acceptable depends on the context and the evidence that is available. They begin to realize that their teachers can serve as models of how people

can make sense of uncertainties about their beliefs and can help them think critically about their own experiences and ways of making judgements, and the views of others. A teacher comes to be seen as a valuable resource who can provide positive and negative feedback on students' thinking. Negative evaluations come to be seen as opportunities for reframing things rather than as a bad person making judgements about one's personal worth.

Level 6 and beyond: Tentative commitments to personal viewpoints

The discovery that knowledge is relative and that teachers can be useful aides for becoming able to see strengths and weaknesses of particular ways of thinking does not solve the problem of what the individual student should believe. Individuals at the most advanced levels in Perry's analysis achieve their individuality by opting not simply to copy others but also by making commitments following much soul-searching – commitments with a capital *C*, exactly of the kind that Shackle (1967) had in mind in the passage quoted on page 195. If people have made mental commitments, they will be prepared to fight wholeheartedly for their values and yet, if operating at Perry's highest levels, they will remain open to change. They will recognize that to make a single commitment still leaves them with a need to take decisions from time to time about whether existing commitments should be revised, abandoned or augmented with new ones.

Consequences of Mismatches between Students' and Lecturers' Expectations

The minority of lecturers who have tried to teach in a way that emphasizes differences in economists' perspectives and the open-ended nature of many economic puzzles can probably remember being surprised by students who seemed so innocent as to believe that textbooks contained the Truth. I have vivid memories of my first encounter with this phenomenon: an overseas student who shared his astonishment with me at the end of a tutorial in which, mindful of Andrews and Brunner (1975, p. 32), I had criticized the idea that a firm will stay in production so long as it covers at least its average variable costs. The student found it easy to see that this might not be so if the price failed to cover average fixed costs that could be avoided by closing down (for example, rates, rental payments and outlays on overhead staff), and yet he was perplexed that this was not what was said in the textbook.

Though academics may remember such incidents they probably forget that once they were just as naïve in their expectations about the process of learning and the nature of economics. At best they probably remember the

process of becoming committed to the kind of economics that they now practise. They will not be trying, as a matter of routine, to look at their courses from the standpoints that their students might be employing.

This is potentially most unfortunate. The work of Perry and his associates leads one to anticipate major problems if a course is presented to a class on the basis that all members are at the same level of development when in fact they are not. As Perry (1985, p. 16) points out, to teach a class consisting mainly of Level 5 students as if they are still Level 2 learners is probably going to result in boredom and frustration, but at least the Level 5 students will be able to make sense of what their teacher is doing. Things are very different in the reverse case.

Consider an intermediate microeconomics course that is presented as if the class consists of students that are at Level 5 and, on this basis, includes instruction in a range of ways of thinking about topics such as consumer and producer theory, economic organization and choice under uncertainty. Some, perhaps the majority of the class, may actually be Level 2 students. During the course, their obsessive search for the Right Answers will be a barrier to concentrating on how different perspectives on the problem have been constructed, and on the difficulties in choosing between them that allow their continued coexistence. Such students will be most uncomfortable if faced with an examination question such as 'Discuss the extent to which Shackle's "potential surprise" theory of choice under uncertainty represents an advance on the "expected utility" analysis of how people decide which risks to take.' They will have trouble relating to the question if their teacher has not presented the material in a way that aims to demonstrate that Shackle's model completely dominates over the expected utility theory, or vice versa, and they will probably feel that their time has been wasted if they have been shown a model that, after all, turned out to be 'no good'. They are likely to see this sort of question as involving 'trick' wording. To avoid being caught out, they will look around for alternative questions set out in a more transparent manner. If they do attempt the question, they will probably construe it as a strangely worded invitation to present expositions of the two theories, or, if they have been taught in a style that praises Shackle's model, they may simply give an exposition of potential surprise analysis. The Level 2 students will expect that their grades will reflect their abilities to avoid technical errors as they present the theories. They will often miss cues provided by their examiner in wording the question, relating to particular philosophical differences (note, in this example, the use of the words 'uncertainty' and 'risk'). Worse still, they will not see that the question can be answered without any systematic outlining of either theory: an effective answer can readily be constructed around various criteria that might be relevant in judging the quality of a theory,

such as predictive capabilities, assumptive realism, range of compass, logical consistency, and so on.

When Level 2 students are presented with questions that invite them to 'compare and contrast' particular pairs of theories or problem situations they are again likely to try to look elsewhere for more clear-cut questions; if they cannot do so, then they will tend to answer in terms of paired expositions of the two theories or pairs of lists of features, rather than in an integrative manner that displays critical thinking (Level 3 students may often produce similar types of answers, but will feel rather more at ease with such questions). Level 2 students will feel comfortable with questions that enable them to show that they have memorized successfully various definitions (for example, 'What is an indifference curve?') or set-piece applications of theory (for example, 'Use the indifference curve/budget line framework to show that a benefit in cash is better for poor people than a benefit in kind of equal cost to the government.'); they are much less able to cope well with applying tools in hitherto untried contexts.

Level 2 students are obviously going to run into trouble if their teachers set examinations that require them to display skills in synthesizing ideas, to marshal evidence to back up particular conclusions, and make personal judgements about which characteristics of a problem area are significant before they decide which theoretical tool might be useful for dealing with the problem. Their problem is not necessarily one involving limited ability to cope with particular technical issues or lines of thinking. If they tackle the questions in Level 2 style they are likely to be penalized simply because they have not even *tried* to do the things their teachers expected to see them do: their worldviews stop them from answering the question.

Such students sometimes become aware, well before their final examinations, that there is a mismatch between their expectations of what will happen during the learning process and those of their teacher. Yet they may have great trouble facing up to the possibility that they should try to change the way they try to learn, rather than their teachers changing the way that they try to teach. For example, I well recall a distraught student who came to see me and said 'Look, I'm a commerce student, I like seeing things down as figures, in black and white; that's why I enjoy accountancy. I work hard and normally do well, but I'm really having trouble with your microeconomics course. I just don't know what I'm expected to be learning from you, it's all grey right now; I can't see where I am supposed to be going with all this fog.' This student was not yet aware that subjective elements could actually cloud issues in accountancy as well as in economics (see Buchanan and Thirlby (eds) 1973; Wolnizer 1987). He had no idea that the kind of economics training he was receiving might prepare him for coping with life as an accountant in a world where matters are not either black or white, and he

made it clear that he was merely looking for a way of getting by in his micro-economics course while he kept applying his cherished Level 2 approach to learning elsewhere in his studies.

Such a reluctance to make a major change of outlook is entirely analogous with the resistance offered by neoclassical economists to suggestions that in some contexts they should embrace satisficing theory and abandon their core idea that *all* choices can be reduced to acts of constrained optimization. They will have applied the neoclassical hard core in many contexts but they will have little experience in using satisficing notions. Until they learn how to think in the alternative style they may risk making a bigger mess of things by trying to embrace it than by continuing to try to fit all decisions into the constrained optimization framework. We are grossly overoptimistic if we expect that Level 2 students and/or neoclassical economists will jump at the promise of the greater insights that will follow if they invest in an alternative, more complicated way of viewing the world. Perry (1985, p. 16) even suggests they will need to go through something akin to a process of grieving: 'I believe that students will not be able to take a next step until they have come to terms with the losses that inhere in the step just taken.'

Three alternatives to progression to a higher learning level may be observed when a student finds it altogether too daunting to try thinking about the learning process in a new way recommended by a teacher or peers (see Perry 1981; Baxter Magolda and Porterfield 1988). Some students pursue a *temporizing* strategy, avoiding commitment while trying to gather mental strength to face up to the challenge of moving on in, say, the next academic year. Others *retreat* from looming complexity (for example, Level 4) to a simpler position (for example, Level 2 dualism). Yet others seek to *escape* from the task of choosing which ideas to embrace because they notice that any commitment to a way of thinking may seem to have inconvenient implications. Escape strategies involve either of two ways of achieving detachment from current challenges: one is dissociation, in other words, letting things drift so that fate determines the outcome; the other is encapsulation, whereby the student plunges into being busy in familiar ways.

Both escape strategies can manifest themselves at a variety of levels. Least alarming to a teacher may be instances of students avoiding commitment in their essays by making no attempt to argue their way towards a conclusion after putting down conflicting views on the topic in question. As an example of how bad dissociation can get on a grand scale, I will explain what happened when I tried teaching price theory in terms of a history of thinking about pricing and competition from Alfred Marshall's work in the 1890s to contestability theory in the 1980s. My intention had been to demonstrate to the class that economists can spend decade after decade stumbling from one puzzle to another, misunderstanding each other and going round in circles

reinventing each other's ideas, with key questions remaining unresolved. I then expected to show the class how, despite the mess, they might begin to feel comfortable with most of the ideas, not simultaneously but in different applied contexts. With hindsight, my strategy amounted to an attempt to lead my class at high speed first on to Perry's Level 3 and then up to Level 5. However, since I was then still oblivious of Perry's work, I had not spent enough time explaining to them what they might experience.

Much to my dismay, lecture attendance soon dropped off dramatically. The students' representatives informed me that a large part of the class simply 'could not take all the different names contradicting one another' and had 'decided to spend their time concentrating on other courses where it was clearer what was wanted and where they were supposed to be going'. To win back the attention of the class took a major effort in terms of supplying handouts summarizing the individuals' key contributions and demonstrating that many of the prime sources that I had been discussing were actually footnoted in their texts (Marshall, Robinson, Chamberlin, Hall and Hitch, Sweezy, Baumol, and so on). The experience made very clear to me just how desperately my students wanted me to present a bland, finished product to them rather than show them potential building blocks and what use had been made of them; many students simply fled once I started showing them with chapter and verse how their textbook misrepresented or misinterpreted primary contributions. They were unwilling to derive self-confidence from being shown how even famous economists sometimes end up following questionable lines of logic or failing to see the point of what someone else has written.

It is possible to observe signs of thinking below the committed relativism level even among master's degree students. If they have not been used to having their opinions and expertise respected by their teachers they appear prone to interpret, for example, an instruction to 'prepare a critical review of' as merely to mean 'prepare a summary of'. Indeed, they often write up their work under the latter heading even though the former is what is written in their course outline. It is not that they cannot be critical if they try but that they still expect the published word to be free of problems. When they read something that does not seem to make sense, they tend to fudge what they write about it on the basis that they are failing to understand it (which certainly *may* be true) rather than criticize it as not making sense for a particular set of reasons (which may well be the case, from particular standpoints).

Phases of Development among Academic Economists, Too?

A non-deterministic approach to teaching economics obviously has major potential to turn out disastrously if presented to an audience that consists

largely of dualistic thinkers, for it highlights areas of difficulties rather than presenting the Truth and it invites students to make personal commitments when dealing with problems. I will shortly explore some ways of trying to help students move towards making commitments to relativism; I thereby hope to make it harder for entrenched interests to argue that such courses cannot work at the undergraduate level. But before I do this I think it may be useful to raise and relate to the process of economic discovery a further theme in Perry's work, namely, that in different parts of their lives people may operate at different levels of thought. If this is so, it is conceivable that academic economists may implicitly lament the fact that their students are answering questions in a Level 2 manner when the questions were set up in the hope that Level 5 or 6 answers would be offered and yet, in their own work as academics, they may in some areas actually be operating rather as if they are, say, Level 2 or Level 3 thinkers. This being so, an awareness of the Perry progression may enable them to advance intellectually.

One area in which we may expect to see the Perry progression at work is in different strategies that scholars use for coping with critical reactions of referees, for though they may have ceased being students, academics do not cease having to undergo examinations. Suppose the submission of an article to a journal leads to a pair of opposing referee reports, one quite supportive and the other fairly critical. A relatively inexperienced academic may not be at the stage where the automatic reaction is to try to see how the critical referee may have come to see the paper as he/she did and then look for opportunities in the critical comments for improving the paper so that it will both satisfy the referee and extend its author's own line of thinking. Instead, the inexperienced academic may be prone to take strength from the positive report and dismiss the critical report as coming from someone who has not spent enough time seeing what the article achieves. The trouble is, such behaviour is not going to be very helpful towards ensuring that the article is revised in a way that will be acceptable to the presently hostile referee. What will probably be needed is a revision (and an accompanying letter) that shows the author's understanding of how the critical verdict was arrived at and argues the author's case (if it is possible to do so) in a manner that will make sense to the critic and, ultimately, to others who think in much the same way.

In outlining the Perry framework I tended to highlight problems that could arise if students were working at lower levels than their teachers: I did not consider what might happen if, say, a Level 5 student sat an examination set by a Level 2 teacher. However, the latter kind of situation is one that may well arise when academic economists are having their work examined by their peers, whether in the process of screening contributions for possible publication or as part of critical discussions of works accepted for publication.

(I suspect it is by no means unknown in the teaching context.) The contrasting works of Caldwell (1982, 1989) and Dow (1985, 1992) provide a very good example of this.

Caldwell ends his excellent examination of philosophical debates about methodology and the methodological practices of economists by making a commitment to methodological pluralism on the basis that 'the quest for a single, universal, prescriptive scientific methodology is quixotic' (1982, p. 244) and 'that results obtained within specific research programs which of necessity follow particular methodological precepts are program specific' (1982, pp. 250–1). This sounds very much like Level 5–6 thinking. However, while Caldwell believes that it may be helpful if economists are conscious of the diversity of research programmes when they present their work, he stresses that he is only proposing methodological pluralism 'as a program for *methodologists*' (1982, p. 251, emphasis in original). Dow (1985) likewise argues that economists have much to learn from trying to understand the different methodological perspectives from which arguments are being constructed. But the way in which she encapsulates these differences is striking: she suggests that mainstream neoclassical economists have a dualistic view of the business of doing economics, whereas post-Keynesians embrace a diversity of methods each subordinate to their overall worldview. Dualistic thought is manifest in neoclassical work not merely in terms of tendencies to claim that economics that does not conform to the neoclassical way of doing things is 'not scientific' or is 'economic poetry, not real economics' (the latter duality is one that I have seen Frank Hahn use in respect of authors who write from subjectivist and behavioural standpoints); it is also evident in terms of a concern with, for example, a strict separation between endogeneity and exogeneity (Dow 1985, pp. 119–21). By contrast, the Level 5–6 context-specific style of thinking in post-Keynesian writings is manifest via, for example, their emphasis on the importance of institutions and their attitudes to econometrics (Dow 1985, pp. 75–7).

Caldwell (1989) finds it very difficult to come to terms with the post-Keynesian methodology. For example, he inquires how Eichner's attempts to build an empirically grounded approach to economics can be embraced by the same scholars who feel drawn to those of Shackle's writings that have raised major questions about the possibility of predicting an undetermined future. As Dow (1992) points out, Caldwell's critique of post-Keynesian thinking is based on dualities such as *a priorism*/prediction and prediction/explanation that are not actually part of the context-dependent way in which post-Keynesians think. From the standpoint of Perry's framework, it appears that while Caldwell argues in Level 5–6 terms that different areas of inquiry may need different types of methodologies, he uses something more akin to a Level 2 mode when he tries to argue that a particular way of thinking

about *economic* issues should not involve the use of a range of related methodologies in a context-dependent way. If one of the most highly regarded economic methodologists of our time can fail to appreciate a key aspect of the post-Keynesian version of committed relativism, then what hope should we have that students will be able to cope with a multi-paradigm text such as Dow's (1985) *Macroeconomic Thought: A Methodological Approach*?

Strategies for Assisting Progression Towards Committed Relativism and Beyond

In an article that both advocates a 'contending perspectives' approach to the teaching of economics and makes explicit reference to Perry, Barone (1991, pp. 21–2) suggests that one of the payoffs to this kind of teaching is that students appear to move more rapidly from dualistic towards relativistic modes of thinking. This should not be seen as inconsistent with what I have so far been arguing, namely, that attempts to teach economics in a non-deterministic manner have the potential to result in major teaching disasters on account of mismatches between teachers' and students' expectations and learning strategies. The key thing is how one sets about presenting this kind of learning experience. Before I outline possible strategies, however, I think it is important to note that Barone is misrepresenting the teaching process when he reports (1991, p. 22) that, via the contending perspectives approach to economics, 'we have moved our students to a more advanced intellectual plane'. As Perry points out:

> [W]e cannot push anyone to develop, or 'get them to see' or 'impact' them. The causal metaphors hidden in English verbs give us a distracting vocabulary for pedagogy. The tone is Lockean and provocative of resistance. We *can* provide, we *can* design opportunities. We can create settings in which students who are ready will be more likely to make new kinds of sense. (Perry 1985, p. 16, emphasis in original)

If students have not yet created for themselves views of what learning might involve that are similar to those of their teachers, they will not be able to see what their teachers are getting at when the latter try to explain to them what they hope to see them do in their courses.

I know this only too well from the frustrating experience of trying to teach business economics to both undergraduate and Master of Business Administration classes. I frequently use case studies of actual firms as the basis for tutorials, assignments and examination questions, with the typical task being to make a critical appraisal, on the basis of economic theory from the course, of the strategy of a firm or of several firms whose strategies partly

overlap but partly differ. (I normally warn students which companies will be the subjects of examination questions several weeks before the date of the examination.) Many students set out to answer such questions by searching for articles that have already been written on the firm(s) in question by academics or business journalists. Such students then *report* what these 'authorities' have found but make no attempt to *do analysis* of their own by choosing which of the components of their theoretical toolkit from the course might be well suited to the context in question. They also tend to fail to consider the pros and cons of strategies the firm(s) could have selected instead of the one(s) actually chosen, unless this is done in existing work that they discover. In other words, they operate at a Level 2, rather as if they are journalists, rather than operating at Level 5 and showing that they could be left to work out solutions for themselves as business analysts (a particularly worrying situation in the case of the MBA students!). This happens despite me giving warnings about this being not what I want to see and urging them not to spend their time researching what others have written about the companies because I will only be giving marks for their own analysis. It also happens in assignments and exam answers despite me showing them in preceding tutorials what it is possible to do with similar questions using just the theoretical materials from the course and historical material from the firms' websites.

One lesson that we might take from the behaviour of such students is that practice is likely to be needed to ensure that potential for thinking at a higher level is realized. This begs the question of the scale of teaching resources that may be required. If teachers give frequent opportunities for experimentation with writing answers to questions that do not give prompts as to exactly how answers might be constructed, they are likely to spend much more of their time marking essays and being visited by students who demand advice on how to tackle questions. To liberate time for providing feedback in the sort of detail that will assist students in improving future assignments – in other words, not merely grading the paper and adding a brief comment at the end, but painstakingly going through it rather as if refereeing an article and pointing out precisely where redundant material lies, where alternative viewpoints are being ignored, and where *non sequiturs* are present – it may be worth adopting the rule that absolutely no consultations are allowed about an assignment prior to the date at which it is handed back in marked form. This rule would need to be communicated with colleagues if it were not a department-/faculty-wide policy, for otherwise students from one course might simply increase their visits to other instructors.

The 'no prior consultations' rule would be likely to come as a shock to many students, particularly Level 2 thinkers, who expect to be spoon-fed by their teachers. However, there might be major benefits from explaining to

the class why the rule was felt necessary and the effects it might have – perceptions of the role of teachers and the nature of learning may be changed by students if they are told that the rule is designed to give tutors more time to spend on giving feedback on students' experimental attempts at using their own ways of thinking in particular contexts. It is a chance to try to convey the message that students' own ways of looking at things are taken seriously and that students are expected to take responsibility for the ideas they commit to paper. The teacher can also emphasize that the rule is aimed at increasing fairness: it guards against rent-seeking behaviour by those students who think nothing of trying to get three or four consultations with an instructor even over a minor assignment. To ensure consistency with the emphasis on the experimental nature of written assignments, it would appear necessary to allow the falsification of expectations not to be catastrophic. One way of encouraging relatively safe experimentation is to inform students that while they might tackle all the suggested assignments, only their best, say, three papers would count towards their coursework grades. Having adopted precisely these strategies and explained their rationale to my class, I have been pleasantly surprised to see how the students have accepted them in the spirit that was intended, and have started to take feedback on their essays very seriously indeed.

If economics teachers are trying to facilitate the recognition by their students of more advanced patterns of thinking, they can augment the provision of feedback on written work by providing in lectures, as handouts or as library resources, detailed information about how questions might be tackled in ways that would be seen as commendable and on the characteristics of answers that are awarded poor or mediocre marks. This can be done not only for assignments currently being used (after they have been marked and returned to the class) but also for previous examination questions, mock examination questions and assignments from previous years (for examples, see Earl 1995, and the companion website of Earl and Wakeley 2005). It would be inappropriate to call such collections of information 'model answers', given that the goal is to encourage students to think for themselves. Ideally, these learning resources could be provided to students as integral parts of their textbooks (as in Earl 1995), in contrast to the prevailing situation whereby they tend to be confined to instructors' manuals designed to accompany particular texts. After all, if teachers are trying to get their students to see them as 'people who have been this way before and can help us to achieve new understandings' rather than as the 'them' who examine 'us', then it is clearly inappropriate for instructors' manuals to exist offering kinds of resources that are not available to students.

Wherever possible, it seems advisable to try to relate the course materials to students' existing repertoires of experiences for, as both Perry and Shackle

often remind us, creative thinking involves a re-sorting of existing elements. Lecturers can do this not merely by keeping theoretical discussions related to problems of economic indeterminacy that students are likely to be experiencing in their lives and encouraging students to try to relate such experiences to the theoretical material. Teachers should also try to be alert to things that students say that reveal an ability to think at higher levels than they are currently thinking in economics. They should not be downhearted if they discover that their students are, say, Level 2 thinkers in economics but Level 5 thinkers when they argue amongst themselves about the merits of various sporting teams, consumer durables or artistic works, for the latter can be used as metaphors that might be applied to economics.

When and How to Bring Indeterminacy into the Economics Classroom

When designing a non-deterministic three-year university-level programme in economics it might be tempting to view the first-year course as the vehicle for getting students familiar with basic economic concepts and terms, the second-year course as one that opens students' eyes to the range of competing worldviews that exist in economics and areas of unresolved debate, and the third-year course as one that involves an emphasis on students thinking for themselves in a variety of applied contexts. In terms of Perry's progression, the temptation is to build the introductory course on the assumption that most students are Level 1 or Level 2 learners, the intermediate course in the hope that students switch over to Level 3 or Type 4 thinking styles, and the advanced course on the expectation that students will be ready to switch to the Level 5 or Level 6 styles of thinking. There are a number of reasons why this could prove an unfortunate strategy.

First, it downplays Perry's message that different students in a typical classroom are likely to be at different stages in their intellectual development. If lecturers merely offer a thoroughly dualistic approach to teaching introductory economics, they will fail to captivate students in a first-year class who are already getting towards relativistic thinking as a result of experiences in other parts of their lives. The work of Perry and his associates suggests that courses may be better designed so that they present opportunities for a range of approaches to learning to seem rewarding. This not only helps to reduce the risk that students will become bored or retreat from involvement in the subject, it also gives opportunities for students to discover that different strategies for coping with learning may be required for coping with different kinds of problems. No one ends up feeling excluded.

Second, if introductions to economics are presented in a thoroughly cut-and-dried manner, students will not be finding problems with applying dualistic thinking to economics: they may be scoring highly and perceive no need to try to come up with other views of what is going on. A dualistic introduction to economics would involve no change from the present typical introductory course involving mainly formal lectures to huge classes and 'objective testing' via multiple-choice examinations. This seems economical in terms of its resource requirements but it has several disastrous consequences for those who try to introduce contending perspectives at the intermediate level. The conventional introductory style of teaching does nothing to falsify expectations that students bring from their high schools based on years of dualistic training. Even if university were expected to be different from school in some vaguely imagined way, then dualistic teaching is likely to signal to the first-year students that it is, in fact, little more than a continuation of school: minds that were open to change are thereby closed. No signal is given that students ought to be expecting to rethink their views of the nature of the learning process.

All this makes a shift to a relativistic style in the second-year course much more of a shock. Resistance to change in this direction is more likely the more that expectations have been firmed up around the idea of economics as a series of diagrams to be learned according to textbook gospel, and lectures as occasions for copying down PowerPoint slides rather than interacting with the lecturer or trying to make sense of a discursive presentation rather as if listening to and taking notes from a radio programme. If students see their role in lectures as consisting mainly of copying material down from a screen rather than *making sense of material* on the spot, we should hardly be surprised that they feel out of their depth when confronted with a lecturer who does not use this mode of delivery. Nor should there be surprise that they do not *build up* an increasingly detailed picture of the subject as the semester goes by and instead try to 'learn' the material in the last few weeks or days before the final examination. Dualistic teaching does nothing to demolish the idea that economics is nothing more than a set of tools that can be stored in student folders until needed.

It is not merely these deductions that lead me to advocate an early sowing of seeds of doubt amongst fledgling economists. My experience has been that it is easier to win a first-year class than a second-year class over to the idea of economics as a non-deterministic subject. First-year students did not panic at the idea of neoclassical, institutionalist and Marxian economists having different things to say about burning issues of the day when I taught an elective course on Australian Political Economy in this style; my experience was very similar to that reported by Barone (1991). Yet each time I have taken over a dualistic second-year sequel to a dualistic first-year core unit and tried to reshape it along

non-deterministic lines I have found myself dealing with conspicuous student resistance and a desire on the part of students for me to convert everything to diagrams that they could learn. Deterministic first-year teaching in terms of a multitude of diagrams, often with a remarkable emphasis on mathematics by lecturers fresh out of North American doctoral programmes, seemed to have made it difficult for students to keep sight of the relevance of economic theory to making sense of practical problems. It must have been difficult for the first-year students to find time to read and think about contemporary economic issues if they were struggling to keep abreast of technical ones in their introductory courses (compare Siegfried et al. 1991, pp. 21–2).

The resistance that I encountered was partly associated with difficulties that I had in recommending a single textbook that bore any obvious relationship with the range of materials being covered in the lectures. Given their lack of experience in taking notes in lectures that emphasized ideas and how they might be used and appraised, students craved a convenient printed source that they could compare with their own notes. My attempt at providing a solution to this difficulty was to write and distribute detailed lecture summaries (which were eventually turned into books: see Earl 1990, 1995). However, this strategy seemed counterproductive until I was introduced to Perry's work and in turn began to introduce my classes to the Perry progression at the outset and to remind them from time to time that in addition to teaching economics I was trying to help them advance along the Perry progression.

Without the Perry background, the problem was that students – particularly those from countries whose school systems reinforce dualistic thinkers – tended to memorize entire summaries and regurgitate them in the examination. Such students would home in on particular words in the questions as signs of which lecture summary they should regurgitate, rather than make any systematic attempt to answer the question in the way that one would expect of someone thinking at Level 5 in Perry's progression. All my warnings that this would be a disastrous way of tackling the examination went unheeded, for that was the only way they had. None of these students took up my invitation to discuss with me the results of experimenting with additional practice essays from past examination papers, to see whether they were succeeding in getting out of this habit prior to the examination.

By contrast, Barone and his colleagues built their course around a set of half a dozen or so key books from different schools of thought in economics and a 'contending perspectives reader' (which I presume consisted of Xeroxed articles and extracts). In the absence of suitable multi-paradigm textbooks, this strategy has much to commend it, particularly since it may help students to get away from thinking of a textbook as a bible. However, texts may still be indispensable for large classes unless small groups of students

can be persuaded to pool their resources, for it is probably unreasonable to expect them each to purchase several books (and unreasonable to expect that libraries would purchase very many sets of the key texts) unless they happen to be, say, cheap paperbacks, each of which is representative of a particular school of thought. At the minimum, what is likely to be needed is a text that provides an overview and thereby enables diverse prime sources to be tackled with confidence.

Pluralistic economics texts already exist. At the intermediate to advanced level, I sought to fill the gap in microeconomics over a decade ago (Earl 1995), and was followed by Himmelweit, Simonetti and Trigg (2001). More recently Tim Wakeley and I (Earl and Wakeley 2005) have offered a business economics text that covers both mainstream and heterodox (mainly behavioural, evolutionary and institutionalist) microeconomics and mainstream and post-Keynesian macroeconomics. Over two decades have already passed since an exemplary multi-paradigm text on macroeconomics was first published, namely Dow (1985). Dow's pioneering book is most unusual in beginning with a careful analysis of methodological issues and then presenting, in as unbiased a way as possible, guides to neo-Austrian, mainstream, post-Keynesian and Marxian thinking on macroeconomics. Dow deliberately avoided making any attempt to appraise these rival worldviews and was criticized for this by Weintraub (1985) on the basis that she was leaving unanswered the question of why at least 98 per cent of economists in the US are members of the mainstream school. Although Dow's text came under fire rather on the basis that it is an unduly relativistic (Level 4) treatment of macroeconomics, it may also be seen as a device for helping students to make informed choices between the rival paradigms or between these existing paradigms and ones they might themselves be trying to put together. Dow teaches students about different ways of thinking and then leaves them to make commitments of their own.

From the standpoint of Perry's work, two rather different comments appear to be in order about Dow's text. First, it is perhaps unfortunate that she chooses to label one paradigm as 'mainstream' rather than 'neoclassical', for she may thereby be encouraging students who do not feel confident about making appraisals of their own to end up conforming with 98 per cent of economists in the US on the basis of nothing more than what the remaining 2 per cent might prefer to call the 'million lemmings can't be wrong principle'. Such users of the text would not be making a commitment in Perry's and Shackle's senses. Second, she might have helped her readers to make commitments (with a capital *C*) if she had presented some personal accounts by macroeconomists of each school who had examined a variety of approaches to macroeconomics before ending up making a commitment to a particular worldview. In the absence of such exemplars, student readers who

have previously had mainly dualistic training in this area (via a typical IS–LM/'aggregate supply and demand'-based text) may tend to retreat towards a Level 3 view of macroeconomics ('really, these debates just come down to discovering the underlying parameters') rather than advancing towards Level 5 and Level 6 viewpoints.

Neither of these comments applies to a more recent, excellent text by Snowdon, Vane and Wynarzcyk (1994). None of the eight schools of macroeconomic thought covered by these authors is labelled as mainstream and the book features interviews with leading members of each school. However, this text has less coverage of methodology than Dow offers. Taken together, the texts by Snowdon et al. (1994) and Dow (1985, republished in revised form in 1996) provide a splendid basis for teaching macroeconomics in terms of contending perspectives. The 'no textbook' excuse for not engaging in pluralistic economics teaching is looking increasingly feeble.

Conclusion

In this chapter I have explored tensions between three things that George Shackle advocated: the construction and debate of rival scenarios, studies of the stereotypes that people use for coping with uncertainty, and the teaching of economics in a way that encourages students to develop for themselves effective ways of thinking about economic problems. Studies of stereotypes used by students for coping may be taken to suggest that economists who are committed to undertaking the sort of teaching programme that Shackle advocated may be unwise to think that they are 'home and hosed' once they have overcome opposition from colleagues who would prefer to see students being taught the gospel only according to neoclassical precepts. The problem then becomes one of dealing with the range of expectations that one's students have, which may largely have been formed in the light of experiences in courses taught in a non-pluralistic, deterministic manner. Lecturers cannot force their students to change their ways of thinking even when they have been given the opportunity to provide students with materials that may be construed as showing that the Truth is not obvious and that context-specific commitments may nonetheless be made to particular ways of looking at things. But they may be able to help their students to progress to higher levels of learning in the following four ways:

1. by challenging those of their colleagues whose styles of teaching involve spoon-feeding and who reward primarily skills in dealing with 'objective tests';
2. by spending more of their time giving feedback after requiring students

to construct for themselves experimental answers to relatively risk-free assignments;

3. by looking for instances of higher-level analysis outside of economics that may be used as metaphors for the kind of thinking styles that might be helpful in economics;
4. by using their own experiences and the experiences of other economists to show that they can understand the struggles their audiences are undergoing.

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12
Some Practical Aspects

THOMAS MAYER

> Truth is so important, however, that it behooves us not to jump to conclusions about it. (Samuels 1997)

Warren Samuels's contribution to our thinking about pluralism has been recognized by his selection as the author of the article on methodological pluralism in the *Handbook of Economic Methodology* (Samuels 1998). This chapter supplements his treatment by presenting a version of epistemic pluralism that is not grounded in postmodernism, and is not subject to the objection that in its strong version it amounts to an 'anything goes' relativism, while in its weak version it amounts to no more than the platitudinous mandate: 'be open to ideas that differ from yours'.

I try to counter the above objection to the weak version of pluralism by presenting a version of epistemic pluralism that focuses on our limited knowledge and our uncertainty about many important aspects of the economy, while accepting the tradition in economics of offering answers to most practical questions that come up, even if such answers have to be based on evidence that is far from compelling. As recent discussions of global warming illustrate, natural scientists sometimes do the same.

I do not presume to contribute to the large philosophical literature on methodological pluralism. The argument is localized to economics – and to the current situation in economics – since it does not discuss whether ultimately we may possess sufficient knowledge to pick the one correct theory. Most of it is therefore consistent with an anti-pluralist position as this is understood in philosophy. Hence, it bypasses some of the problems faced by pluralism that Caldwell (1988) discusses, such as its relation to a theory of truth. And unlike Caldwell it looks at pluralism in economics as a whole, instead of focusing on pluralism in methodology.

On many issues in economics only zealots hold their conclusions with certainty. Most, while they may think that the evidence for their conclusions is strong, admit at least the possibility that they may be wrong. Yet in

practice they usually ignore this possibility. Thus, some methodologists – following the lead of many philosophers – seem to argue that some particular method, such as methodological individualism, is the only correct way to do economics, so that any results reached by some other method are worthless. Similarly, in welfare economics economists start (implicitly or explicitly) with a particular value judgement, for example the importance of egalitarianism, and usually proceed from there without, at least explicitly, allowing alternative value judgements any weight at all. Likewise, only rarely do those who offer policy recommendations discuss whether these recommendations are robust with respect to errors in their model, despite the familiar saying that 'all models are wrong – but some are useful'.

Such a procedure differs sharply from the way economists describe the behaviour of agents. In simple models agents are assumed to calculate certainty equivalents rather than to operate just on the basis of the most likely forecast. And even halfway sophisticated models go beyond certainty equivalents and allow for risk aversion. I will therefore explore the implications of assuming that economists should operate with as much sophistication as they ascribe to agents, so that they realize that they, as well as the agents whose behaviour they model, have to live with uncertainty.

My target is therefore segmented decision-making, that is, the standard practice of first selecting on one's reading of the best – but perhaps far from conclusive – evidence, a certain proposition (that is a value judgement, a methodological rule, a theorem, or an empirical finding), and then to use this proposition in further reasoning as though it were definitively established. This procedure and its questionable nature are both most evident in empirical work. Typically, an econometrician presents not only her point estimate, but also its standard error. But in the next step of the argument she ignores the standard error, and plugs only the point estimate of the coefficient into the subsequent equation. One should therefore not be surprised if the resulting conclusions are not robust.

A more reasonable alternative is often, though not always, not to accept as the truth a particular proposition because the evidence for it seems strongest, but to carry in one's mind two or more conflicting propositions, while attaching unequal weights to them. (In deciding on these weights one should usually take account of the relative losses from accepting the wrong proposition.) As Mäki (1997, p. 43) has remarked: 'it may be that at some point in time, such as now, the epistemic standing of economic theories is such that we had better tolerate a number of strong ... [substitute theories] at the same time'. More generally, Richard Foley (1983) has shown that it is not always irrational to hold at the same time two logically inconsistent theories.[1]

Such a procedure might be called 'probabilistic pluralism', since it attaches different weights to various alternatives. It therefore differs sharply from

relativism with its notion that all theories are equal. On the contrary, it assumes that we can – and should – distinguish between theories that are more likely to be true and those that are not. Such probabilistic pluralism is hardly new. Thus it seems to describe how the Federal Reserve, and presumably other policy makers, often behave. Thus as monetarism obtained more academic respectability in the 1960s and 1970s, the Federal Reserve Bank in the US shifted towards monetarism – but only part of the way.

I first discuss probabilistic pluralism with respect to economic policy, and then discuss theory, methodology and value judgements. I then respond to the argument that pluralism on the level of the individual economist is not needed, since truth will emerge from the debate of various economists each of whom presents just one point of view and disregards the contribution of other views.

Policy

Choosing a policy is analogous to choosing a portfolio of securities. Economic theory tells us that rational investors do not allocate all of their net worth to the asset that they expect to have the highest yield, but diversify in a way that takes into account various expected states of the world and their relative probabilities, as well as risk aversion. Compare that to the article on economic policy. Here we are told that a certain policy would improve the functioning of the economy, and the risk inherent in the adoption of this policy is either ignored, or the reader is given reasons for thinking that it is minor. But the probability that these reasons are mistaken, and that the risk is substantial, is usually ignored. Moreover, while some specifics of the analysis are often spelled out in excruciatingly detailed mathematical form, we are told little if anything about the loss function implicit in making the policy recommendations, or about the assumed degree of risk aversion. To be sure, not all policy literature proceeds in this way. Thus there exist a series of models showing that the optimal size of a stabilization policy depends, in part, on the standard error of the GDP forecast (see for instance, Brainard 1967, Mishkin 1998). Moreover, in many cases – though far from all – it would make little sense to diversify the policy 'portfolio' by adopting the policy only in part. But even in all-or-nothing cases, to indicate the riskiness of the policy would be helpful. To be sure, saying that the author should point out all the weaknesses of the analysis he or she knows about may be a counsel of perfection, because it would require pointing out these weaknesses not just to the reader, but also to the editor and referees. But even leaving aside the ethical problem in hiding weaknesses, there are sources of risk that could safely be pointed out. For an example of how risk is disregarded consider the debate on whether banks should be allowed to merge

with nonfinancial firms, or to hold stock in them along the lines of the German and Japanese 'universal banking' systems. One might agree with the proponents of such a change that it would *probably* improve the American financial system. But 'probably' is not 'certainly'. Hence, one has to look not just at the size of the gain if they are right, but also at the size of the loss if they are wrong, and at the relative probabilities, as well as the appropriate coefficient of risk aversion. Another example is the argument (see Calomiris 1990) that the Federal Deposit Insurance Corporation (FDIC) in the USA should be abolished, and that instead banks should insure each other. One can make a plausible case that this would probably make the financial system more efficient by reducing moral hazard, but there are also risks. The obvious one is that without the FDIC, massive bank failures could set off a major recession.[2] It is not irrational for someone to agree with Calomiris that such a contingency is unlikely, and that the reduction in moral hazard that would result from the abolition of the FDIC is highly desirable, and yet to oppose this change because massive bank failures would be so damaging that, when multiplied by even a small probability, the expected loss outweighs the gain from abolishing the FDIC. And even someone who believes that the mathematical value of the benefit from abolishing the FDIC exceeds the mathematical value of the cost, might reasonably oppose it due to risk aversion.

Disregarding the risk of error could be defended by arguing that the researcher's task is to develop a particular line of reasoning, and it is up to the reader to keep in mind that this reasoning may be wrong, and to evaluate the resulting risk. There is certainly something to this response, but the reader may not be in as good a position as the researcher to evaluate the probability of error. I return to this topic in the penultimate section.

Theory

The choice of a theory presents a similar situation. Accepting a particular theory is often (like adopting a policy) an action that has consequences, such as influencing the views of other people, or changing one's research procedures or research agenda. To be sure, the loss function is often hard to specify because we do not know all the implications that acceptance of the theory will have, but that does not justify ignoring the loss function altogether. One reasonable response is to say, for example, that although I accept theory T, I am aware that there is a significant probability that it is wrong. Consequently, when undertaking some action, such as choosing a research project, I am reluctant to select one that is worth doing only if T is correct. And my reluctance will depend not only on this probability, but also on the costs and potential pay-offs of the research project.

It is, of course, not possible to calculate these probabilities precisely, but one can obtain some rough idea, particularly if one is willing to take other economists' opinions seriously. Thus, if I know that someone who is just as well informed as I am, disagrees with me and thinks that T is false, should I not tell my students that, though I accept it myself, there is considerable doubt that T is true? And if I write an op-ed article predicated on the truth of T, if there is much dispute about it, shouldn't I warn readers who are unaware of this?

Moreover, theories have specific domains. Not only is this so in the formal use of theories as logical systems whose conclusions depend upon the validity of certain assumptions, but also in their informal use relevant to answering empirical questions, where the assumptions are not intended to hold strictly, but only approximately. If the domains of theories are properly specified, then there need be no conflict between different theories. But if, as is usual in economics, the domains of theories are not clearly specified, then there is likely to be an area in which their claimed domains overlap, or at least an area where it is unclear which theory applies. Hence there is often room for an eclectic theory that combines elements of two or more conflicting theories, or for maintaining both theories. We know that theory A is better for one problem, and theory B for another, but are uncertain for a third problem, where we may want to look at the solutions given by both theories, and if they differ admit our uncertainty.

To a formalist who would like to model economics on geometry such vagueness may seem 'unscientific'. But not to those who prefer to model economics on the natural sciences. Ian Hacking (1983, p. 264) reports that:

> Even people in a team, who work on different parts of the same large experiment, may hold different and mutually incompatible accounts of electrons. That is because different parts of the experiment will make different uses of electrons. Models good for calculations on one aspect of the electrons will be poor for others. Occasionally a team actually has to select a member with a quite different theoretical perspective simply ... to get someone who can solve those experimental problems.

Such conduct is not inconsistent with the belief that there exists only one given reality (that is, ontological realism) which we may be able to grasp eventually, but for now we have to make do with what Mäki (1997) calls 'temporary pluralism'.

Methodology

In the 1950s an anti-pluralist view of methodology was much easier to justify than it is now. Then, one could argue that there is a delineable set of

disciplines known as 'science', that we know this set's defining characteristics, and that this set is the sole reliable path to genuine knowledge. To be sure, there were disputes even within the mainstream philosophy of science, but with hindsight they were mere family squabbles. The main task of the economic methodologist was therefore to decide which of the prevailing economic methodologies best captures the defining characteristics of science as set out by philosophers, or if none do so, to develop one that does.

But the received view is no more; the old certainties are gone. Even if one rejects (as I do) most of the postmodernist turn, the search for the one true methodology seems quixotic. Now we need to look not for a demarcation criterion, but for heuristics that advance the growth of knowledge. And these heuristics may have features that seem strange. For many years biology made substantial advances even though it used the crutch of teleological argument. Inconsistency between quantum theory and relativity theory has not prevented physicists from working with both and making progress.

One should therefore be sceptical about statements that economists *must* do one thing or another, for example that they must not make statements about aggregates unless these can be shown to be necessary implications of rational utility maximization. That claim seems particularly hard to accept when it comes from those who in defending the unrealism of assumptions take refuge in Friedmanian instrumentalism, and it is worth considering in some detail, because it provides a good example of the flaws of such a monistic methodology.[3]

If physicists could get by for so long by taking gravitation as an unexplained given, why should macroeconomists, too, not be allowed to take observed regularities as given?[4] Even in the case where macrotheory is actually inconsistent with microtheory, instead of just being not derivable from it, we need not, as the example of relativity theory and quantum mechanics illustrates, drop either. If we want to claim complete knowledge something will have to give, and consistency between macro- and microeconomics may have to be established. But until we reach the point where such a claim is realistic we can use both theories. It could be that rational choice theory is a sufficiently close approximation to be useful when dealing with most microeconomic problems, but not when dealing with certain macroeconomic problems.

The insistence of new classical economists that we have to reduce all macroeconomics to microeconomics is therefore hard to justify at this stage of our knowledge, so long as one views economics as an empirical science, that is, as an attempt to explain and predict regularities, rather than as a branch of logic. It would be correct only if three conditions hold. The first is the claim that we may not tolerate – even for now – contradictions that

we hope to resolve in the future. That claim is hard to justify, though many new classical theorists seem to treat it as obviously correct. The second is that we have sufficient grounds to be more confident in rational choice theory than in the relevant macrotheory – and that is by no means obvious. Third, even if these two conditions hold we would be justified in dropping the macrotheory only if it is *inconsistent* with the microtheory, not merely if it cannot be derived from the rational choice theory. Biology is consistent with quantum theory, but nobody would try to reduce biological statements to quantum theory statements.

How then can we understand the new classical insistence on rigorously deriving all macrotheory from microtheory? One possibility is to view new classical theory not *primarily* as an attempt to explain observed economic phenomena, but as an attempt to derive the logical implications of rational choice theory, thus in effect defining economics in a Robbinsian rather than a Marshallian way.

Maartan Janssen (1998, p. 308) has made the interesting suggestion that it is a unity-of-science argument that provides the valid justification for the reductionism that the new classicals insist on, writing:

> Two distinctively separate disciplines such as microeconomics and macroeconomics can only coexist in a fruitful way if they have different domains of application. However, as ... both study aggregate phenomena, it is not clear when to apply one (and not the other). It is thus natural to study the compatibility of the two disciplines, and as microeconomics has a better developed analytical structure, the reason for investigating the possibilities for microfoundations becomes clear.

Janssen is right in saying that the interrelation of microeconomics and macroeconomics is worth studying, and that it would be desirable to relate them more closely. But that is very different from saying that we *must* avoid macroeconomic statements that cannot be derived from microeconomic foundations, even if it means having no answers to certain questions, or giving answers that have a much less secure empirical foundation. Moreover, that microeconomics has 'a better developed analytic structure' does not necessarily mean that it is superior to macroeconomics. Other criteria, such as empirical confirmation and applicability, matter too, particularly if one thinks of theories as instruments for either prediction or explanation.

Yet none of this means that being rigorously derivable from microtheory is not a desirable attribute of a macrotheory. It does add an additional way of testing a macrotheory, and furthermore, if the macrotheory is well confirmed, it provides a way of testing the microtheory. But there is a distinction between being a *desirable* attribute and being a *necessary* attribute. Hence, it may well be useful to have two or more macrotheories (or versions

of macrotheory). One version is at least loosely derivable from microtheory (even if it does not meet the strict conditions of reducability).[5] The other version, though not inconsistent with microtheory, is not derivable from it, but can solve some problems that the derivable theory does not, or has better empirical support. Moreover, the consistency of microtheory and macrotheory should be treated as a two-way street.

Another instance where a pluralistic attitude towards methodology is appropriate is the debate about *Verstehen* (the understanding we have about human activities). Obviously, *Verstehen* does not provide evidence that meets the criterion of interpersonal knowledge; what makes you understand why twelfth century peasants did not revolt may not necessarily make me understand it. But if good empirical evidence that is interpersonal is not available, evidence derived from Verstehen is preferable to mere guesses. Moreover, Verstehen can add to the usefulness of a theory in satisfying our intellectual curiosity, and by making the theory easier to work with on an intuitive level. So, instead of treating Verstehen as either totally useless or as a necessary attribute of any adequate theory, why not treat it as a desirable but not a necessary feature?

Value Judgements

In making policy recommendations economists must make value judgements. Even the application of the Pareto criterion is a value judgment, though an unusually vacuous one that might even be called hypocritical.[6] Usually much less restrictive value judgements are needed. Ideally policy makers or philosophers could provide them. But policy makers are often reluctant to be that explicit, and philosophers do not speak with one voice.[7] Thus, some philosophers tell us that individuals have an inherent right to their property, so that a just state will not redistribute income. Others say that the state should redistribute income because equality is the moral default setting and inequality of income can be justified only insofar as it benefits the poor as well as the rich. Both sides present coherent arguments.

So what should the economist do? One alternative is to confine herself to purely positive economics. But that is hard to do, both because value judgements tend to seep unnoticed into positive analysis (the assumption of a quadratic loss function is one example), and also because the economist's clients, that is the general public and policy makers, want definite answers to questions that combine positive and normative elements. Another alternative is for the economist, despite her lack of training in philosophy, to take the plunge and make the required value judgements. But that adds an arbitrary element to her conclusions. A third alternative is to study ethics and political

philosophy in depth. However, that suggestion not only comes up against a time constraint, but also seems insufficient. To those who have at least *some* positivistic tendencies, the various resolutions provided by political philosophy, despite their great sophistication and substantial value-added over untutored common sense, often seem tenuous.

Hence, in making value judgements – regardless of whether economists take an untutored plunge, or devote much study to it – they are on insecure grounds. A related and serious problem is that one's value judgements may be inconsistent. I may believe in the importance of property rights, but also in the need to provide more help to the poor than private charity can make available. Should I favour a progressive income tax to finance a welfare programme?

Such problems suggest a fourth possibility, straddling. Given uncertainty, straddling, that is going part of the way with each of two or more contradictory principles, is appropriate if there are increasing costs to errors.

Suppose the issue is whether to tax Peter $10 to pay to Paul, and that we believe that this redistribution is appropriate, but are not sure. Hence, we may tax Peter only, say, $5. If we are right in believing that a $10 tax is appropriate, then by imposing only a $5 tax we commit two injustices, depriving Paul of $5 that are due to him, and permitting Peter to retain $5 that should not be his. And if our belief is not justified, then we also commit two injustices. Alternatively, if we impose the $10 tax, then if our belief is correct we commit no injustice, but if it is incorrect we commit an injustice that in dollar terms is twice as large as in the first case.

Obviously, our decision should depend, in part, on how confident we are in our belief, as well as on our degree of risk aversion.[8] But it should also depend on our evaluation of the relation between the magnitude of this injustice and the loss that this injustice creates, that is on whether there are increasing, constant or decreasing costs to injustice (see Johnson and Mayer 1962). And that is not clear. Neither increasing, constant nor decreasing costs can be dismissed out of hand. Some might argue that there are increasing costs to injustice: that imposing a $10 unjustified tax on any one person has more than twice the moral cost of imposing a $5 unjustified tax on two persons, because unjustified suffering should be spread as thinly as possible rather than concentrated on one person. At the other extreme some might argue that the very existence of an injustice represents an evil whose undesirability has nothing to do with its size – that it is meaningless to quantify injustice. This is the extreme case of decreasing cost to injustice. A more moderate position is that there are both fixed costs and rising variable costs to injustice, in the sense that the very existence of any injustice, however small, is offensive, and hence imposes a fixed cost, but that there is also an additional cost to injustice that varies (probably more than proportionately) with its magnitude.

In the first case (increasing cost) there is a role for straddling, while in the second case (decreasing costs) there is not. In the third case the answer depends on whether the variable costs are increasing at a fast enough rate to outweigh the fixed cost component. Thus in at least one, and perhaps two of the three cases, the pluralist's tendency to straddle may be justified.

Probabilistic pluralism in value judgements can also be justified in another way. Theories of ethics are grounded in the basic principles that the public perceive to be ethical. In turn, much of the public's perception is based on its evaluation of particular situations (which may be either actual or hypothetical). And since these evaluations do not necessarily form a coherent, logical system, but have a strong emotive element, it would not be surprising if sometimes there are contradictions. When confronted with a specific situation I may decide that X's rights have precedence over Y's, and derive a general rule from this. But when confronted with a very different case I may decide that Y's rights have precedence and derive a rule that conflicts with the previous one.

A pragmatist might well argue that we simply have to make our peace with a world in which certain moral imperatives have domains that are not adequately demarcated. A decent society needs to think of human life as in one sense sacred, not as something whose value is measured in dollar terms. We do not permit someone to sell to another person the right to kill him. At the same time, in making practical decisions, society sometimes has to weigh human life in dollar terms. Few would advocate spending, say, $100 million on a medical procedure that would prolong one person's life by only one day.[9] And we permit risky activities like coal mining.

Individual or Collective Pluralism

A speech community may act in accordance with pluralism, even if no individual member does so, by allowing an adequate hearing to diverse points of view. Is that all that one should ask for, or is it desirable that on many issues individual scientists keep several divergent views and their probabilities in mind? In other words, should economists follow lawyers and act as advocates, or should they try to resemble as best they can the picture of the dispassionate scientist found in idealistic discussions of science. There is something to be said on both sides.

On the one side, acting as an advocate rather than dispassionately weighing the evidence and admitting weaknesses in one's arguments comes naturally because it avoids the discomforts of cognitive dissonance. Hence, regardless of whether economists (or other scientists) *should* behave this way or not, in practice they are likely to do so, and, one might argue, it is better

to do so openly, and put the audience on notice. Moreover, economists, like other flesh-and-blood scientists, need the stimulus that comes from motives like status enhancement; perhaps there is some truth to the frequent confusion of 'disinterested' and 'uninterested'. An attitude of 'my theory, right or wrong' is likely to motivate them to work harder than is an objective search for the truth. One might therefore argue that the advocacy model of economics represents a useful and almost necessary division of labour.

But there is a stronger case to be made on the other side. Even if full attainment of the ideal of the dispassionate scientist is out of reach, maintaining this ideal points economists in the right direction. Moreover, the advocacy model has its comparative advantage in situations where the public or policy makers can exercise reasoned judgement about who is right, or, putting it less positively, where experts can do little better than the lay public. Thus we let juries decide the facts of the case, while judges decide the law. In economics the public and policy makers find it difficult to decide who is right. And not only the public, but also other economists face this difficulty. Authors of empirical articles often have many opportunities to skew their results by, for example, leaving out observations. If readers are to take such articles seriously they must have some confidence that the author is motivated, or at least more or less constrained, by the dispassionate-scientist model.[10] Furthermore, the public is less likely to take the views of economists seriously, even on issues on which most economists agree, if on other issues it sees economists occupying immovable positions that concede nothing to the other side. In addition, if economists, because they see themselves as advocates, are in the habit of ignoring the evidence on the other side when they present their case, they are likely to slip into the same habit when deciding which side to choose, and then to stay with it even if strong contrary evidence comes along. This is hardly a formula for swift progress.

Conclusion

Theorems are subject to demonstrative reasoning. Theories and the evidence for or against them generally are not. Insofar as economics models itself on empirical science with its focus on theories rather than theorems the principles about behaviour under uncertainty that economists proclaim with regard to agents should apply to economists themselves. This provides an opening for pluralism. Hence, in giving policy advice economists should seek diversification and consider the probability of error, and also the loss function and risk aversion. And since advocacy of a theory is in a relevant way like a policy decision, the same applies to theories. If this means employing several

contradictory theories, then that is consistent with rational behaviour. In choosing a methodology, too, a sharp dichotomy of right and wrong is not useful, as the example of new classical reductionism illustrates. Similarly, there is a role for probabilistic pluralism in making value judgements.

Notes

1. Foley (1983) gives an example of rational belief in two conflicting propositions. Suppose that a persuasive experiment shows that there is a 99 per cent probability that each one of a set of a hundred hypotheses is true, while another, conclusive experiment shows that not all of these hundred hypotheses can be true at the same time. It is then reasonable to accept each of these hypotheses, while also accepting that at least one of them must be false.
2. There is also the problem that banks might use membership in their insurance organization as a tool for collusion.
3. For a powerful and general critique of reductionist claims see Kincaid (1997)
4. To be sure, macroeconomic regularities are not as reliable as the regularities that physicists deal with. But then economic theory is also not as solidly established as physical theory. It is therefore far from obvious that in economics observed regularities should play a relatively lesser role relative to theory than in physics. And while, in principle, the Lucas critique makes macroeconomic regularities suspect, there is not much empirical evidence to suggest that in most situations it is empirically important.
5. I am using the term 'derivable' in a loose, common-sense fashion that does not imply that the strict conditions for reduction (see Kincaid 1997) are fully met.
6. Sheltering behind the Pareto criterion is hypocritical because the probability that any change would make nobody worse off is so small that if economists take the Pareto criterion seriously they could make virtually no policy recommendations.
7. Presumably policy makers are reluctant to make value judgements in part because they do not want to admit to themselves that they are making some people worse off. Moreover, in democratic countries policy makers are selected in part on the basis of their skill in papering over differences.
8. This assumes that committing an injustice has no further consequences.
9. Along these lines see Hausman and McPherson's (1996, chapters 1 and 14) discussion of rich countries exporting pollution to poor countries.
10. For a survey of the extent to which economists do take econometric evidence seriously see Mayer (1995, Chapter 9)

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13
Islamic Economics: A Case Study

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As an undergraduate student at the International Islamic University Malaysia, my experience in studying economics was unique in the sense that while being exposed to mainstream neoclassical economics, there was explicit mention that economics was to be taught in a 'comparative' and 'critical' manner. At the same time, due to events of the late 1970s and early 1980s, developing 'Islamic economics' was one of the goals in a few Muslim countries, including Malaysia. I discovered that there was also a 'mainstream school' among those writing on Islamic economics, modelled along neoclassical lines, working almost within the boundaries of neoclassical theory, with some adjustments to incorporate teachings, norms and values that reflected certain requirements of Islam.

What worried some of us was the almost total neglect of non-mainstream views in the literature and the very few serious attempts to identify and address 'foundational' issues seriously. Writings in Islamic economics began to be dominated by those in the area of banking and finance, with almost the sole objective being to develop alternative financial instruments and products to be used in the 'alternative banking and financial system' that has now become globally known as Islamic Banking and Finance. I could not accept the almost exclusive direction taken by the 'mainstream' literature and, for me, coming across the post-autistic economics movement at the turn of the century was almost a revelation.

Looking through the issues of the *Post-Autistic Economics Review* (PAER), I realized that the writings went to the core of neoclassical economics, attacking its theoretical foundations. It is imperative to address these foundations and to point out their relevance to reality. What we know, how we can know it, and the criteria to evaluate what and how we know are central foundational issues in any approach to economics. If we claim to have an alternative approach to economics, it must necessarily be projected from a worldview or vision that represents that approach. It must also be developed

based on knowledge and using the sources of knowledge in a way determined by that approach.

The first section of this chapter briefly shares the experience at the Department of Economics of the International Islamic University, Malaysia, in introducing a course called Foundations of Islamic Economics (FIE). The second section then discusses issues related to the relationship between economics and religion and puts forward arguments for why a religious-based economics is possible, at least in the case of Islamic economics. The author welcomes comments and constructive criticism, in the hope that religion-based economics can be accepted as part of heterodox economics, in line with calls for pluralism in economics.

Background of Course ECON 1710: Foundations of Islamic Economics (FIE)

The FIE course has been offered as a faculty required course to all students since 1993, with adjustments made in 1995, 1997 and 2003 to the course content. It is meant to provide students with a rationale for and an introduction to Islamic economics. The course is presented in a comparative manner and all students would have taken Principles of Microeconomics and Principles of Macroeconomics as prerequisites.[1] FIE is generally meant to provide a conceptual introduction to economics as stated in the course outline:

> This course introduces students to the basic premise that the study of Islamic economics proceeds from the Islamic worldview and has to be developed according to a methodology that is founded upon this worldview. Therefore the 'foundations' that need elaboration are the Islamic worldview, Islamic economic methodology, and features of an Islamic economic system. Since economics deals with the production, consumption and distribution activities of man, these areas will be addressed in this course. Other more prominent areas of contemporary Islamic economics such as the prohibition of *riba* and issues in Islamic banking are also discussed.

Six main topics are covered in about fourteen weeks:

1. Islam, Its Worldview and Islamic Economics
2. Methodology of Islamic Economics
3. The Islamic Economic System
4. Allocation of Resources in an Islamic Economic System
5. Distribution
6. Issues in Islamic Financing: *Riba*, Contracts and Islamic Banking

For topics 3–6 of the course, articles written by writers (and occasionally

critics) of Islamic economics who address the subtopics and issues discussed are used as references. Most of the topics and subtopics are known to students as they are topics in economics. For example, Topic 3 discusses the characteristics of economic systems used in comparative systems textbooks. An attempt is made to situate the Islamic economic system *vis-à-vis* capitalism and socialism, while pointing out that all systems have their own underlying philosophical foundations and goals in terms of their meaning and means of achieving them. Topic 4 discusses consumption and production, focusing on the possibilities of different normative frameworks that govern the positive/technical side of decision-making. Topic 5 discusses distributive justice as seen by writers of Islamic economics and elaborates on goals of distribution and measures implemented to achieve distributive justice. Topic 6 very briefly surveys some important issues in Islamic banking and finance and is included in this course more for the business and accounting students who may not take other courses in Islamic economics.

As pointed out in the introductory class, Topics 1 and 2 are in actuality the main topics of the course since they (and to a lesser extent topic 3) are the 'foundations' of economics. Any attempt to present alternatives to standard textbook economics discussions (in topics 4, 5 and 6) can only be fruitful if these foundations are presented and understood in a meaningful and acceptable way that opens up the minds of students to the possibility of alternatives and gives arguments to support these alternative foundations. Students are encouraged to refer to articles in the PAER as additional references in the course. What follows is a brief elaboration of topics 1 and 2 in terms of 'what is covered' and 'how it is covered'. In presenting this, I have attempted to connect the discussion to issues found in alternative economics literature, especially in the PAER.

Islam, its worldview and Islamic economics

Contemporary Islamic economics is presented as a twentieth-century response of Muslim societies to calls for indigenous solutions to their development problems since political independence. Although some writers like Kuran (2004) have traced the term *Islamic economics* to the 1940s, it was only in the 1970s that Islamic economics was officially born. Is there a rational basis for an Islamic economics? Topic 1 tries to present this rationale.

The course is based on the premise that no human endeavour is value-free. Economics in this case is preceded by what Schumpeter (1954) calls a 'vision'. As far as the conceptual framework of economics is concerned, what needs to be done is to derive or 'systematize' a certain economic vision within the overall worldview that will involve certain 'core' economics-related concepts in the overall worldview. This process of deriving an

economic vision, made up of selected interrelated concepts, is 'ideologically' based in the Schumpeterian sense, since choosing, and later ordering, defining and interpreting these concepts are undertaken within the overall worldview of the person doing it. While Schumpeter still considered this ideological element as something that needed to be 'neutralized' through proper, universal methods of analysis, we do not agree that these elements are necessarily 'bad' or that they can and should be neutralized. In this sense, we agree with Heilbroner (1988) who sees ideology as a part of economics since its 'motivations are not only powerful, inescapable, but legitimate'. Since alternative worldviews/ideologies exist, different economics are not only possible but, one can argue, natural and legitimate as well. In fact, according to Heilbroner, without a vision or a 'belief system' (ideology) there can be no economic analysis because there will be nothing to analyse. In this sense, economic analysis works within an ideology.

Hence, the universality of the Western experience of economics, dominated by neoclassical economics today, cannot be accepted as a rule of law. This view is supported by John Stuart Mill ([1836] 1948) thus: 'Political economy, therefore, reasons from assumed premises which might be totally without foundation in fact, and which are not pretended to be universally in accordance with it.'

Topic 1 tries to discuss the Islamic economic vision and tries to make comparisons to the standard neoclassical economic vision that underlies textbook economics. In this, much of the criticism used by alternative economics can be, and is, given as support for the position that alternative views of religion, man, nature, knowledge, aim in life and their implications for economic pursuits can lead to 'different economics'. For the purpose of this chapter, I will highlight the discussion on religion.

Religion and economics

The very idea that religion can be a major influence in determining economic activities would be dismissed as incoherent, irrational and emotional by the majority of contemporary economists. This is primarily due to the historical experience of Western Europe with Christianity and the result of the secularization process that has taken place in Western Europe since the seventeenth century. However, as presented by some writers, this secularization process is very much a Western European experience and may not be universally applicable (see Elliade 1987). In the religious perspective of Islam, human beings are asked to address secular pursuits, that is, to deal with the here and now. Hence 'shunning this world' was never a religious teaching for Muslims as it may have been to Christian Europe in the Middle Ages.

However, the ideology of secularism that underlies much of contemporary science and Western society today is more problematic. This ideology, if interpreted to mean that 'only the here and now' is relevant and even 'real', would not be acceptable to most religions and their adherents. Reference to a life hereafter, to matters that are not strictly observable or comprehensible to pure human reason, are irrelevant for economic decision-making in standard economic reasoning. However, if one believes in the 'unobservable', this has tremendous implications for what rational decisions mean. This 'extended time-horizon' would certainly affect the choice of individuals and societies in allocating scarce resources.

Religion for Muslims is not accepted as being a 'human creation' of, or for, 'infantile' man, but is a representation of a 'way of life'. The term used to denote 'religion' in Islam is *din* and does not limit itself to the personal rituals and faith/dogma as usually understood by the term religion. As mentioned by Watt (1979, pp. 3–4), the term *din* refers more to a

> whole way of life … [covering] both the private and public/societal lives of man, it permeates the whole fabric of society, and includes theological dogma, forms of worship, political theory and a detailed code of conduct, including even matters which the European would classify as hygiene or etiquette …

As far as Islam is concerned, it is argued that the concept of *din* provides an all-encompassing ethos for man, including that of economics. Religion is seen as a source of ethics for economic behaviour. This seems to be at odds with the experience of Western society, where even attempts at bridging the gap between religion and economics have ended up with more opponents than proponents. Other important elements of a worldview that are discussed in order to justify the possibility of an Islamic economics are the elements of man, nature, and aim in life, since these have direct relevance for making comparisons to mainstream neoclassical economics (see Haneef 1997).

Man, who is at once the vicegerent on earth and servant of God, has to play the role of a trustee, utilizing nature for the benefit of humankind. Both these roles have to be lived simultaneously, and any neglect of either one would not enable man to function as his true self. In fact, as stated by Nasr (1990): 'There is no more dangerous creature on earth than a khalifah (vicegerent) who no longer considers himself to be an *abd* (servant).'

While these views are shared by other religious teachings, they have huge implications for ethical behaviour. Motivation is both extrinsic and intrinsic (Goodwin et al. 2004). Man is at once endowed with physical, intellectual and spiritual potentialities that must all be nurtured and developed. This acceptance of the spiritual aspect of man is of fundamental importance in the Islamic economic vision (and in other religious traditions) and has far-reaching

implications for the epistemology and methodology of Islamic economics and in relation to human welfare and needs.

Nature is not seen as something that must be 'overcome' or 'conquered' in man's pursuit for development in this world. It is, rather, a bounty from God that must be utilized in the 'best way possible' for all. This 'best way' could be interpreted differently by different scholars at different times and places while still being within the parameters set by the Islamic worldview and, more specifically, the economic vision. This view of vicegerency shares many commonalities with the Christian view of stewardship and even with some social economics views discussed in the *Post-Autistic Economics Review*.

The primary aim of life for humans as described by Islam is to achieve happiness, or *falah* (ultimate success). Man is urged to use the resources in this world to achieve success not only in this world, but in the next world as well. In the mould of Aristotelian ethics, one of the most prominent scholars in Islamic scholarship, Al-Ghazali (d. 1111), mentions four means to achieve this ultimate success: goods of the soul, goods of the body, external goods and divine grace. Material pursuits (wealth) would be considered external goods that are part of the provisions to achieve success provided they do not contradict the other categories, especially the goods of the soul (faith and good character or knowledge and right action). The ethical imperatives of this requirement are again very profound. Justice is the observing of moderation in all things. All virtuous qualities of the soul are thought to stem from temperance and justice, while selfishness/greed is not seen as a virtue.[2]

Methodology of economics: is there only one scientific method?

Topic 2 on methodology is probably the most difficult topic to teach as well as to receive (on the part of students). In Malaysian universities, this may be due to local type of secondary education system which focuses primarily on examinations and on the expectation that economics must deal with 'practical matters'. (The author welcomes feedback from participants on whether similar problems are faced in other programmes and how they have been overcome.)

First and foremost, it must be stressed that in Islam, knowledge is possible and that certainty, at the human level, is attainable. The central point of departure of Islamic epistemology from that of post seventeenth-century Western Europe is in the prominence and centrality of revelation in the pursuit of economic knowledge. It is revelation that provides the foundation upon which the senses and intellect function. Thus in Islamic epistemology, and I believe in other religious traditions, there is a higher authority than the senses and human reason. This higher authority provides the stable 'vertical axis' (the transcendent) to which the 'horizontal axis' (efforts of humans) can refer as a point of reference. This paradigm of knowledge sees human reason

as aided or rather *guided* by revelation. As stated by Al-Ghazali, 'prudence alone does not guarantee restraint and moderation and requires the aid of revelation'. This proposition is at present considered 'unscientific' in neo-classical economics. Would it be accepted by those who promote 'pluralism' in approaches to studying economic phenomena?

Islamic tradition accepts varying methods of scientific inquiry in accordance with the nature of the subject in question and modes of understanding that subject.

> Muslim scientists, in their cultivation and development of the various sciences, have relied upon every avenue of knowledge open to man, from ratiocination and interpretation of sacred scriptures to observation and experimentation. (Bakar 1991, p. 15)[3]

Definitions, logical clarity and semantic analysis were some of the early disciplines that developed from this religion-based scientific spirit. However, as stated by Bakar (1991):

> (logic) was used extensively from the 11th century but did not lead to the kind of secular rationalism experienced in the west during the enlightenment and renaissance. Similarly, the empirical studies employed by Muslim scholars did not lead to the kind of empiricism in the west beginning with Roger Bacon. This was because reason was always linked to revelation and sense perception was never made the source and verification of all knowledge.

Outlines of an Islamic economic methodology

Methodology discusses the process of building models, developing theories, testing hypotheses, as well as establishing and using criteria to evaluate our process. It deals with theory appraisal, with standards and benchmarks that determine the authority of our 'scientific' statements. Islamic economics or any other religion-based economics, just like any other disciplines of study requires proper methodology in order to develop theories which then can be 'verified', or at least 'not falsified yet', by practice.

In Islamic economics (religion-based economics), we have the central position of revelation (the Quran or the other holy books) as the ultimate authority. The challenge that is faced is how to use revelation, including reason and observation, to develop Islamic economics. The Quran is not an economics textbook but it does provide guidelines and general principles to guide human beings in their economic life. To develop Islamic solutions to economic problems entails a process of 'deriving' the answers based on the Islamic sources; as agreed by all scholars, most of the economic applications will have to be derived, and hence the development of an Islamic economics is an intellectual challenge.

From an epistemological perspective, Islamic economics was broadly defined as: 'an approach to interpreting and solving man's economic problems based on the values, norms, laws and institutions found in, and derived from all sources of knowledge'. These sources of knowledge include revelation, reason and the universe (observation) and must be used 'appropriately'. Judging from the writings of contemporary writers on Islamic economics, there is no unanimously agreed-upon formula as to how the sources will be interpreted and followed, especially in all details. Very little has been written in this area. Priorities differ among schools and scholars, hence different economic views and policy prescriptions are potentially possible. Both deduction and induction are accepted methods of analysis in Islamic economics and this has never posed a methodological problem for Muslim scholars in the past. In this sense, pluralism has always been a part of Islamic scholarship.

Model/theory building

In building models/theory, the stages involved are as follows:

1. *Establishing assumptions, relevant variables and their tentative relationship; understand your worldview; establish your economic vision*. This stage may happen 'naturally' as everyone has a worldview, even if they do not realize they have one. Also in today's world, most starting points are not from zero but rather from the accumulated work of previous scholars. In conventional economics, many scholars also accept the fact that all economic analysis has to start with a 'vision'. The issue is what this vision is, its sources and how it is formed.

Revelation, being a legitimate source of knowledge, will certainly be a source of this vision and of modifying the vision. What revelation has to say about economic behaviour and concepts including those related to man, nature, man's relationship to nature and other humans, as well as those relating to consumption, production, distribution, finance. will form a preliminary conceptual framework of Islamic economics. This framework will have to be 'systematized' into principles, postulates, hypotheses, precepts and assumptions that will be investigated and validated or otherwise. As mentioned earlier, pluralism is natural to Islamic scholarship since the interpretation of revelation and the systematization process can still vary within the parameters set in Islamic scholarship.

For example, the prohibition of *riba* in Islam has been interpreted in the mainstream to mean the prohibition of interest while others have seen it as the prohibition of 'unearned gain', while others see this as the prohibition of 'exploitation'. If differences occur at the conceptual level, then certainly they would have significant implications at the policy level.[4] Here again, one could

easily see parallels in the spectrum of views in economics as propounded by alternative schools. It is clear from this example that while juristic interpretations are fundamental in developing Islamic economic thought and policy prescriptions, moral and ethical issues relating to 'different possible interpretations' can and do vary within the spectrum of Islamic economic thought.

In establishing an Islamic economic vision, works of past and present scholars would be referred to and the economic vision would be adequately modified. This stage is similar to how accumulated knowledge and 'authority' are dealt with in Western economics. Since no human being is infallible, all the views of past and present scholars are subject to critical evaluation. Since in the last four hundred years, Muslim scholars have generally not played a leading role in developing economics/science, critical interaction with modern economics, *with the full spectrum of views from both orthodox and heterodox schools*, is called for. Just as the post-autistic economics movement calls for a critical view of standard economics, so too would Islamic economics. It would be totally naïve and unrealistic to believe that an Islamic perspective would have a unique view on every matter and issue, distinct from the whole range of Western economics. *Selective assimilation* cannot be ruled out as a valid ingredient in economic theory building in Islam.

2. *Establish principles, laws, assumptions, hypotheses, models, that is the tentative theory (which should be normative).* This step is found also in standard economic model/theory building. Based on the assumptions founded on the Islamic economic vision as revised and modified, empirical studies would naturally constitute part of the process of developing an acceptable body of economic thought.

3. *Test hypothesis and models, that is, empirical studies (which is positive).* If empirical tests verify (or do not yet falsify) the hypothesis, they are accepted, and if repeatedly observed they become ultimately the building blocs for further theory. From an Islamic perspective, this step would also be acceptable.

If empirical tests do not support the hypothesis, all aspects of steps 1–2, including the technical aspects of the survey etcetera, are rechecked. This would also be acceptable in Islamic economics. It would be necessary to ensure that our assumptions, variables and their 'intuitive' relationships correctly reflect the economic vision. Since there should be no contradiction between the position of revelation and that of sense observation, this double-checking would be very important. If this review manages to solve the divergence, then the problem is solved.

If after this review, there is still a divergence between the tentative theory and the practice observed, conventional economics would move to change

theory to suit practice since 'practice determines theory'. For religion-based economics, however, sense experience does not provide the absolute proof for 'truth'. In Islamic methodology, facts must be distinguished from truth. While 'proofs' from sense experience have a certain authority, in Islamic epistemology, secondary sources cannot escape the criteria and proofs from revelation. 'Reality' will include revelation. If 'correct' interpretation of 'revelation' is established, that is, the economic vision is correct (something that is prone to debate and difference of opinion), it could be equally possible that experience should be what undergoes change (via policy).

Can There Be a Religion-based Economics?

From the preceding paragraphs, an affirmative answer has been put forward giving Islamic economics as a case study. Many may not agree with this and see it as unacceptable primarily from ontological and epistemological perspectives. This is very much a product of the Western European experience from which sciences that we know today have been developed and classified. The same may not be true in many parts of Latin America, Asia and Africa where religion is still a very important determinant of not only individual life but social/public life as well. If the seventeenth century witnessed the advent of the secularization process, the last quarter of the twentieth century has seen events that have been termed the 'unsecularization of the world'.[5] Even so far as the 'Western' world is concerned, the World Values Survey has shown the USA as not following strictly the modernization/secularization model of development.

Therefore, trying to propose a religion-based economics as part of an alternative economics movement like the Association for Heterodox Economics needs more discussion/debate between economists and religious scholars. Some efforts in the past have been attempted. Brennan and Waterman (1994) report a meeting between liberation theologians and a group of economists, a few of whom were known to have religious beliefs. They report that the meeting was one of 'no engagement' in which members of the two groups were almost talking on 'different wavelengths'. They conclude that 'a useful distinction between economics and religion is possible – and indeed necessary', since economics was 'freed from religion' and had its own 'rules' since as far back as the late nineteenth century'.[6] In the second volume of the series, Dean and Waterman (1999) also viewed economics as 'autonomous' with regard to theology and saw it as futile to talk of a 'Christian', 'Islamic' or 'atheist' economics.

However, there was by no means unanimity in this view. Some, like Dow,[7] argued that since economics deals with human beings and human

behaviour, religion has a role to play in understanding the subject concerned. In fact, looking through the *PAER* and other alternative economics literature, many proponents of 'social economics' (formerly described by some as Catholic economic thought) see the possibility of a religion-based economics, albeit cautiously. Nevertheless, based on the overall evaluation of the limited references on the relationship between economics and religion (at least so far as the West is concerned), there seems to be a preference to maintain separation. It seems to be easier to talk of 'ethical economics' rather than religion-based economics in the West. Many, like Charles Wilber (2002), state that since economists and economic actors have values, and since social institutions and society in general add ethical dimensions to supplement economic evaluations, ethics has to be part of economics.

This is certainly a possible way of finding common ground since proponents of religion-based economics would cite religion as the source of ethics. Earlier writers such as Sen (1987), have pointed out that while economics had two 'origins', namely ethics and 'engineering' (meaning the technical side), it has been the latter that has dominated. He argues, like many in the PAER, that economics has become impoverished due to this separation between ethics and economics. Sen suggests that economics can actually be 'more productive' by paying greater and more explicit attention to the ethical considerations that shape human behaviour and judgement.

The numerous petitions of students and academics calling for 'reform' in economics education all call for pluralism. Could this pluralism include a religion-based economics? Looking through the PAER, there seem to be divided opinions on the matter. Most proponents of alternative economics would agree with King (2004) that there is not one single and correct alternative to neoclassical economics. Many reasons are put forward. Human beings are complex, economics is complex, economic theories are time-specific or context-based, etc. However, it can also be agreed as stated by Hodgson (quoted in King, PAER) that being a proponent of pluralism does not mean support for 'unqualified relativism' and logical incoherence. There is indeed a need to ensure 'logic, coherence and consistency' in arguments (see, for example, Cole et al. 1991).[29] Dow's categorization of modified pluralism (King 2004) is probably a good basis on which to proceed with our discourse on a religion-based economics:

> Worldview and theory of knowledge cannot be eradicated; yet recognition of differences at this level allows for reasoned debate over appraisal criteria and analysis of different methodologies. (Dow 1996, pp. 45–46).

It would seem, at least in the Western experience, that economics and theology/religion speak different languages, have different goals and have

different criteria to judge outcomes (Dean and Waterman 1999). These differences can be acknowledged, as indeed has been clear from much of the literature. However, for there to be any 'reasoned debate' by proponents of the two sides as proposed by Dow, there is a 'need to have some common ground for differences' (Brennan and Waterman 1994, p. 4). This may be a more difficult area to get agreement on as seen by the views expressed in the two volumes mentioned earlier. This point is expressed in a different context by Davidson (2005) who argues that there must be 'one' benchmark (Keynesian economics, according to him) that acts as the foundation of all other alternative models.

The present writer does not see a problem with religion-based economics being argued 'rationally'. However, the criteria to judge this rationality may pose a problem for Davidson and others who have certain set views on this. I would reject the view that somehow a religion-based economics (and its theories of human behaviour) would be able to claim absolute authority; this is because in my view theories are also a product not only of revelation but also of the human intellect as they have to undergo a process of theory/model building. Many of the natural constraints quoted by proponents of alternative economics would also be valid for religion-based economics. As stated very clearly in the first section of this chapter, Islamic economics can be and has been presented as a 'spectrum' rather than a monolithic body of knowledge (see Haneef 1995).[21]

Finally, proponents of alternative economics have consistently argued that neoclassical economics is all about a mythical creature called rational economic man. Numerous articles in the PAER have taken to task neoclassical economics for being oblivious to reality, that is, to the practice of economics and to more 'holistic' explanations of human behaviour. If we accept this call to look at reality, to see economic behaviour and the way people in different societies perceive economic life and explain the factors that affect economic decision-making, then certainly there is a case to accept the possibility of a religion-based economics.

In many parts of Asia and Africa, religion is still seen as an important part of the individual and social life of communities. This is certainly true for Muslim countries as diverse and pluralistic as Malaysia and Indonesia in Southeast Asia as well in the more central regions of the Muslim world. As stated by Stauth (1998), Islam seems to have entered a new position of 'relative centrality' that has become the motor of an 'Islamic' modernity. Alternative economics proponents need to be aware of this fact.

Conclusion

This chapter has attempted to put forward a case for religion-based economics as part of the call for pluralism in the teaching and development of alternative economics. In many parts of the world, religion is still an important part of public life. Trying to understand and explain human economic behaviour then must take into consideration this fact. While it is accepted that the economics that we inherit today is an economics that has been moulded on the experiences of mainly Western European society, the vast spectrum of views found in economics can certainly be open to the possibility of a religion-based economics. If the economics discipline, inclusive of all its diverse schools of thought, can break free from the constraints of seeing economics as a product of the 'European project of modernity' and if the proponents of pluralism can connect to the various alternative responses to the 'universality of Western knowledge' thesis, heterodox economics could certainly accommodate a religion-based economics. Referring to religion does not have to be seen as 'irrational' or backward. While we in no way are supporters of an unlimited 'anything goes' view, it must be equally unacceptable to insist that only certain views are within the ambit of 'economics', when more than two thirds of humanity may have a 'religious' orientation *and want to see this orientation expressed in their way of understanding and practising economics*.

Notes

1. While students are exposed to some 'Islamic perspective' in these two courses, it is very minimal as the emphasis is on providing the standard textbook economics to students. Any comparison and critique are provided in Foundations of Islamic Economics, in another course called Issues in Islamic Economics, and to a lesser extent in the intermediate courses in Microeconomics and Macroeconomics.
2. There is an important distinction to be drawn between *self-interest* as defined by Adam Smith and *selfishness* which seems to be used synonymously today. The former may be considered a virtue. See Lux (1990).
3. See Bakar (1991). This view is also shared by some Western scholars with their view of 'adequatio', namely that different levels of being/knowledge require different modes or channels of knowing. See, for example, Maritain (1937) and Schumacher (1977).
4. While the first interpretation would call for the abolition of interest rates as a fundamental requirement in an Islamic economic system, the second and third may neither consider it fully necessary, and they definitely consider it insufficient, and would also require some form of structural reforms, for example involving land reform and redistribution. While the first group may not disagree on the need for structural reforms, they do not see it as proceeding from the verses on the prohibition of *riba*.
5. Weigel, cited in Huntingdon (1995). Scholars like Peter Berger also discuss this phenomenon.

6. This liberation is even seen by some to go back to Adam Smith. See, for example, Minowitz (1993).
7. See Brennan and Waterman (1994).

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