

# Science, theology and the reform of economics

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## 1. PRELUDE

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From the late Attic Greek *hermeneús*, meaning an interpreter, comes the modern word 'hermeneutics'. The Ionic term was *exegetés* which has given us 'exigesis'. Both, according to Thomson<sup>1</sup>, are synonyms for the leader of the Chorus in Greek Drama, traditionally initiates of the local divinity.

Drama originates in secret significance. Exoteric or public meaning, accessible to the *Demos*, contests with esoteric or hidden meaning, available only to initiates. The *exegetés* blasphemously reveals sacred meanings, but limits damage to the social order by confining the revelation to the stage and to past events. The contrast between revealed and stated meaning is still today the root of all dramatic tension. Because the spectators have access to forbidden meaning, they understand the action better than the characters. They partake of divine attributes within the confines of performance: the laws of destiny are revealed to them.

The Chorus understands the action but cannot take part in it. Traditionally old and lacking youthful power, it parallels the modern scientist, the wise experimenter blessed with knowledge but forbidden to intervene. This primordial contract requires that knowledge be secret, expert, and inaccessible to the *Hoi Poloi*.

The late Attic word for 'actor' – *hypokrités* – is also translated as 'interpreter' and has mutated to the modern 'hypocritical', the advocacy of a principle that is not practiced, or to put it another way, serving two laws – one promulgated publicly, the other practiced privately.

This corresponds to an actual social transformation. The aristocracy's social power was represented as a special relation to alienated tribal divinities (Hera, Ares, Thetis, etc), allocated to the territorial sites settled by their tribe. It is systematically represented, for example in Homeric literature, as godlike or directly descended from the gods. To be unconstrained by law is the essence of godhead. The aristocracy were thereby elevated above the laws they themselves imposed.

Universal social laws originated with the tyrants, who wrested social power from the aristocracy and paved the way for the democracy. The *Demos* could no longer be subjugated by laws to which the law-givers were not themselves subject. The aristocracy single-mindedly fought to overturn this social conquest with enormous bitterness throughout antiquity and beyond.

Plato's *Republic*, the intellectual high point of this endeavour, solved the aristocratic dilemma by restating an ancient tradition: a *single law with two meanings*. Placing the gods in the heavens, he put them out of reach, so their laws could only be understood through interpreters. He then advanced the famous 'Golden Lie', that these same gods had segregated men into three classes, Gold, Silver and Bronze.

The function of the Gold class is to implement divine will. The function of the Silver class is to interpret divine will. The function of the Bronze class is to work.

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## 2. INTRODUCTION: THE DORNBUSCH INTERREGNUM

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When the peso collapsed, Rudiger Dornbusch circulated a proposal to rescue it from politicians. The troubled helm of the Argentine ship of state should be taken, he argued, by Science – in the shape of a committee of the world's finest economists.

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<sup>1</sup> Thomson, G. (1973) *Aeschylus and Athens*. London:Lawrence and Wishart

Two weeks later the finest came to call, in the slightly different shape of a delegation from the IMF, under whose tutelage Argentina had run up a sovereign debt of \$180bn. Economic lexicons do not contain the word 'contrition'. The IMF offered no recompense for past mistakes: it did, however, demand that Argentina repeal the 'ley de subversiones' which opened the way to criminal proceedings against fraudulent lenders, and annul its bankruptcy laws.

It was not altogether clear why these two demands were prioritised over questions like 'how did we get into this mess?' or 'how can Argentina pay it back?' Nor was it clear why the IMF chose this moment to demand changes to laws which had been the statute books for decades. The IMF planted its defences firmly on the high ground of orthodoxy. Political interference with bankers would undermine the free market, whose 'natural' course should in turn be allowed to punish poor performers. Economic science so decreed.

Until one looks below the surface. Large sums of money were missing, and many people were suing many other people. Only the financiers, it seemed, were placed by economic theory above the law. By an interesting coincidence, most banks were under American control.

By another interesting coincidence, bankruptcy loomed for companies not under American control. The income of the water and electric utilities, for example, was tied to consumer incomes – savagely reduced by the collapse of the peso and now threatened by further IMF proposals to remove the social safety net.

By a third interesting coincidence a further law, whose repeal the IMF did not seek, gave creditors the right to convert debts into shares. The practical outcome of economic science, applied to the test case of Argentina, was thus to place on a plate, in front of US finance capital, a further tranche of Argentine enterprises at knock-down prices, without the tedium of calling in unpayable debts.

Argentina is only one example of a completely general practice but serves to illustrate my central point: economics is a technical language for political proposals which are not explicitly admitted. A politician who said 'our solution to Argentina's problems is to destitute its working people and consign it to the tender mercy of the US banks' would be thrown out of court. An economic expert presents the same conclusion but couches it in such terms that the practical implications can only be deciphered by another economist. The language has two meanings: its outward, apparently objective content which appears to the general public, and a concealed, intensely subjective content, accessible only to initiates.

It is not my purpose to complain that IMF policies were 'bad economics', that it failed to implement its own theory, or that it bent its own rules to factional ends. To the contrary if this was the only problem, economics could be exonerated by indicting the IMF. My point is that economics *while implementing* its impeccable logic, in general delivers results which manifestly favour private interests.

My question is: what does this tell us about the nature of economics? We confront a body of knowledge which systematically favours one class over another, one fraction of that class over other fractions, and even one geographical subset of that fraction over all others – yet which uses a language, and methods, which is presented as neutral, objective, and positive. Moreover this is not a simple fraud. Its approach to quantification, logic and observation are widely regarded, including by most non-economists, as the hallmark of scientific practice.

We confront a special object in the spectrum of forms of knowledge: an apparently scientific body of thought which functions as a theology.

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### 3. WHAT DOES ECONOMICS DO?

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*Nature and Nature's Laws lay hid as in night;*

*God said 'Let Newton be' and all was light*

Alexander Pope

*But not for long; the devil, roaring 'Ho,*

*let Einstein be' restored the status quo*

Anon

The question 'what kind of knowledge is economics?' has been systematically sidestepped in debates on the philosophy of science. It is treated as a 'special case' because it does not fit the classifications which the protagonists in such debates find convenient. But it is not a special case: it is a test case. If we cannot include the particular case of economic knowledge in any general theory of knowledge, then we cannot claim that this theory is truly general.

Consider the late twentieth-century debate between the sociologists of science, headed up by Kuhn, and the positivist philosophers epitomised by Popper. Kuhn does not to my knowledge consider a single case history relating to economics – yet economics makes precisely the most strident and contestable claims to scientificity.

How general, then, are his results? He could perhaps justify this lack of generality by arguing that economics is not a science: he does not do so – economics is an abrupt absence in his life's work. There is no Kuhnian sociology of economics: therefore either economics cannot be a science from a Kuhnian standpoint, or the Kuhnian sociology of science is incomplete.

On the other hand positivism, though addressed originally to the physical sciences, has made its most stable inroads in the social sciences and above all economics, where every undergraduate is still schooled in the Popperian catechism as if a hundred years of philosophical controversy had never happened. No branch of knowledge is more proudly positivist. Yet throughout the twentieth century experience has consistently refuted its principal theoretical results and it has hardly batted an eyelid. What does this tell us about the programme of positivism? On this no silence is more deafening than that of the positivists themselves.

This highlights an even wider problem: 'Science' as a whole is respectfully silent about the things economists do. Why does it permit economics to appear as a science at all? Why has science – the handmaiden of secularism – failed to recognise, for what it is, a body of knowledge which to all intents and purposes functions as a religion?

Confronted with these conundrums I see only one conclusion. Following the method of Sherlock Holmes, science *itself*, improbable though this may seem, is not scientific. Economics is a mirror which, when held up to enlightenment science, reflects back its most basic limitations. These in turn arise from a Faustian contract: the scientist is freed from the constraints of the social order to study 'nature', which is defined as everything not social. In return the social order has created a new Platonic God of the Market. This in turn is analysed as the *effect of nature on humans*. Either via uncontrollable biological urges or technologically determinate 'physical conditions', in economics the behaviour of humans is explained in something beyond the reach of humans. Hence, by virtue of the very separation to which science has consented, the prime cause of society's behaviour is placed beyond social control, inaccessible to all except economic initiates.

## WHY HERMENEUTICS?

A great deal of this article is about hermeneutics – the interpretation of texts. This is the particular form, within the modern discussion in economics, of the more general question at the centre of critical philosophy, namely: where do ideas originate?

The emphasis on interpretation arises from a prolonged theoretical struggle, spanning Twentieth-century economics, between two ontologically opposed paradigms, the *equilibrium* and the *temporal* view. The precise meaning of this distinction will become clear. At this point the reader should know that these paradigms are distinguished not simply in the way they investigate phenomena but in the way they compel us to think about them. In every branch of economics, opposed theories use the same words to describe their observations – price, profit, value, and most of all, causation and determination. Superficially it appears that these theories diverge only in the conclusions they draw from observation – but in fact they differ most of all in the meaning that they give to the words that give the observations their content.

This is not to say they produce the same results. Quite the contrary. Within the temporal interpretation of Keynes, people that want to work may persistently fail to find it. Within the equilibrium interpretation, this cannot occur. Within the equilibrium interpretation of Marx, the rate of profit can never fall with cost-reducing technical change, but within the temporal interpretation it can. And so on.

The point is that these differences in observational outcome are not, in practice, what causes one theory to be adopted instead of another. In economics the succession of ideas and the triumph of one paradigm over another is not determined by exoteric meaning – capacity to explain observation – but by esoteric meaning – capacity to justify, make sense of, and hypostasise the social order. It is in this precise sense that economic knowledge plays the same social function and occupies the same role in the division of social labour as the classical religions of pre-capitalist eras.

In consequence we cannot develop a method for discriminating a true from a false economic theory merely applying it neutrally, according to the procedures of positivist philosophy, to the observed world. Society does not actually offer us that luxury. It will not let us conduct ourselves in this manner. We must therefore begin by critically unveiling what it implies, for society, to think about the observed world in the way the economist presents it.

The critical tradition, in philosophy and economics alike, asserts that valid knowledge may be acquired by making *meanings* themselves the object of enquiry, along with the real world observations to which they refer. This article takes the view one step further: it argues that for a valid scientific practice in economics the question of meaning is primary. Unless we first grasp what any economic theory *means* by price, value, profit and indeed, how it conceives of causality in economics, we will be blocked, because of the way economics functions as a branch of the division of labour, from discriminating between theories in the 'normal' enlightenment manner.

To frame the issue more precisely let us attempt the most general and inclusive possible definition of scientific enquiry. Since Kuhn there has been a fruitful proliferation of new insights into science and religion alike – including post-Modernism, Critical Realism, and so on. However, no matter what view the reader may have of science, she must accept that it cannot simply test each theory individually against 'the facts'; it must test several theories against each other. And in fact, it must at least permit the testing of the *whole range* of available theories against each other. If we set out construct a theory of navigation, and decided,

before we started, that we would disallow and disregard any theory which assumed the earth was round, we could conduct positivist experiments until the cows came home. But the road to the truth – to any concept of truth, however postmodern or positivist it might be – would be barred, because we would have ruled out, *a priori*, an entire theoretical framework which, if so happens, is the only one of which we are presently aware that makes sense of observation.

But in order to test any theory, we must know what that theory actually is. One cannot test the assertion 'Marx was wrong about value' without establishing what Marx actually meant by the word 'value'. Moreover, if one interprets Marx wrongly, and finds out that the wrong interpretation fails to deliver Marx's result, there is actually no way, without hermeneutic analysis, to determine whether the error arises from Marx's theory itself, or in our false interpretation of that theory.

With this in mind, let us ask how economics might, if it were a science, test such assertions as 'Keynes's theory of employment has been proven inadequate' or 'Marx's theory of the rate of profit has been proven false'? Actually, it is impossible to establish the truth or falsity of either statement unless we know what Keynes's theory actually was and what Marx's theory actually was. To provide a logically sound answer to all such questions, there are therefore two bodies of evidence which must be confronted: the evidence of reality, and the evidence of the text itself.

The scientific community entertains strong prejudices against hermeneutics. This is because, since Francis Bacon's polemics against the 'schoolmen', it has identified textual analysis with the religious method of proof by scripture, which it sees as one step removed from proof by authority, and completely opposed to the 'scientific' method of proof by evidence.

However a text does contain evidence – of the true content of the theory which is contained in its symbols. The Humanities – philosophy, literature, and so on – offer well-established procedures to test rival interpretations of texts, based on this evidence. Nothing in these procedures carries any implication that truth can be deduced from these texts. To assert something about the meaning of a text is, as even positivists know, an entirely different matter from asserting that the text is true.

It may seem to the reader that all this is a diversion, since we appear to have failed in an elementary duty of objectivity, which is to distinguish between a theory as such, and the historical origin of a theory. Why should it matter at all what Keynes thought or what Marx thought, when we should be able to abstract from their writings the 'true' theoretical content which, together with its meanings, can be tested independent of the person that first wrote them down. After all, natural scientists do not care what Newton 'really thought'. His theory of gravity, for example, is usually simply termed 'the' law of gravity. What does it matter that Newton invented it?

We can answer this in general terms first: the *procedure by which* a 'theory' is abstracted from authorship is itself an act of interpretation. Because of the method which economics actually adopts when it discusses theory, there is no 'neutral' version of Keynes's theory or Marx's theory in existence. There exist only interpretations of these theories – Keynes *according to* Hicks, Marx *according to* Botkiewicz, each authored by persons who claim to present the 'true content' of these author's theories but in fact present, under this guise, a theory distinct in its own right. To invert a famous saying, the philosophers have hitherto only changed Marx: the point is to interpret him. The history of economic thought is what Samuelson has termed a 'Whig History' of theory. Each successive generation of economists rewrites past theory as if it were an imperfect attempt at their own.

This would in fact provide an objective account of past theory, *if* modern theory were an advance upon it. The reason we accept, for example, Einstein's

interpretation (for such it is) of Newtonian theory, is basically that his own theory is better, and indeed includes Newtonian theory as a special case. But if modern theory is a retrogression from past theory, this comfortable teleology no longer holds. All attempts to judge Copernican theory as *interpreted* by Ptolemaics led to monstrous injustices against truth, as would, for example, attempts to make sense of Einstein which permitted only those interpretations of him which insisted on a Newtonian view of place and time.

If, therefore, the actual course of economic knowledge is to move backwards, as did scientific knowledge in Mediaeval Europe for many centuries, then it is not only perfectly possible but highly likely that there are truths to be found within earlier writers, which are not to be found in modern thinking. The enlightenment itself originated with the rediscovery of classical knowledge which gives us the very name 'renaissance'. If – as I will argue – economics is driven by a motor which systematically takes it up blind alleys, there is no reason to suppose that it can improve itself by any other means than the rediscovery of earlier lost truths. But in that case we really do need to know what all earlier writers 'really thought', because if we merely read them through the lenses of modern ideas, we will miss the point they were trying to make.

## SUPPRESSION AND THE ROOTS OF DEDUCTIVISM

We hope the above has opened my reader's mind to the possibility that at least some new truths may be found by paying attention to the interpretation of old texts. However, if this were the end of the matter, all we need do is raise the profile of the history of thought, and otherwise carry on as before. Actually the problem is far, far greater. If we fail to place the issue of interpretation, as a method of critical enquiry, at the forefront of any economic methodology, we actually consent to the *suppression* of these earlier truths.

This is because economics applies the scriptural method of deduction in reverse. It discounts ideas with which it does not agree, not by tackling the ideas as such but by placing their *author* outside the ring. Marginalist economics triumphed in a concentrated ideological battle, beginning with Böhm-Bawerk's famous essay, not against 'value theory' in general but against Marx's value theory in particular. Neo-liberalism surfaced not with a general polemic against 'demand management' in general but from Friedman's polemic against Keynes's views on money. Moreover it had to be done this way, since value theory in Marx's hands is, in point of fact, superior to marginalism and Keynes's theory of money is, in point of fact, superior to neoliberalism. Therefore the profession, in order to regress along the path it had travelled, had no choice but to traduce those who had beaten that path.

A large part of my argument is that this method appears in relation to Marx in its most extreme form, precisely because Marx developed the temporal paradigm in the most systematic and comprehensive manner. This is why I will submit evidence of my thesis primarily – but not only – by considering the way that economics treats Marx's writings. It is however essential to understand that the same basic approach has operated, across the board, in the whole of economics for the whole of its history and has governed the actual course of the whole of its theoretical disputes.

With this in mind, consider a point made by Brewer (1995):

By any normal standard, [Marx] should not be accorded a significant position in the history of economics at all. It is not just that his ideas are not to be found in modern textbooks, but that they were never seriously discussed by mainstream economists, either during or after his lifetime ....the neglect of Marx's work by the

mainstream has been so complete and so visible that it would be a waste of space to document it.

This lacuna is as total as it is unique. Working economists take it so much for granted that they rarely notice it is unique to their profession. The excision of one of its principal founders by an entire discipline is without parallel. It is not to be found in history, in sociology or politics where, although Marx is a minority figure and under frequent attack, his presence and his influence are undeniable.

The extent of the suppression of Marx in economics is not generally grasped by other social scientists, and certainly not by scientists. In their own subjects they encounter the general hostility to Marx which prevails in society at large, and they believe that what happens in economics is simply the same kind of thing.

It is not.

In subjects such as biology, Marx's influence on figures like Stephen Jay Gould or R.C. Lewontin is well-known. Gould was a public figure, a well-known celebrity. In sociology, there is no contradiction between a distinguished and respectable career such as that of Anthony Giddens, and his careful if eclectic study of Marx's contribution to Sociology. In History, writers such as Eric Hobsbawm are widely recognised. The story is repeated in Philosophy and in Politics. In these disciplines Marx's ideas will at least figure in undergraduate programmes, albeit as a minority element. To be sure, it is difficult to promote or study Marx's ideas – but it is not impossible.

In Economics things are of a completely different order. Marx occupies the role of heretic. He is totally absent from undergraduate courses, from journals, from most graduate schools, from any serious place in the 'research' programme of economics.<sup>2</sup> To be known as a Marxist or even someone with associations with Marx is sufficient to bar any serious researcher from most promotion and nearly all publication. Even the word 'value' outside of seminar on taxation, risks raising eyebrows. Marxism exists, in economics, at the subterranean level of minor streams in general conferences, associate lectureships in liberal arts colleges, and the odd outpost, usually conquered in the seventies, and generally of such minor significance that the rightwing opposition have opted to wait until it dies, rather than unseating it.

No substantiable reason is given for this exclusion. Whatever the reason is, it certainly has little to do with empirical validity as writers wiser than most economists point out from time to time – usually during crises. Consider the following passage from Cassidy(1997) in the *New Yorker* under the heading 'the next economist'

Many of the contradictions that he[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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<sup>2</sup> There is a kind of hierarchical pyramid of acceptability in economics which depends from the US with an epicentre in Chicago. The UK is fairly near the top, with the Italians, French and German quite far behind. The global South sits near the bottom. In the murky depths where no fundamental damage can be done to the overall paradigm, Marx is more tolerated. The acid test of a genuine pluralism would be a serious space for Post-Keynesian or Marxist writings either in something like the AEA journal or in the EJ. As the saying goes, Dream On.

Nor can it be said that modern economics has shown itself in any sense empirically superior. Contrast Cassidy with Paul Ormerod's (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.

On any dispassionate view, it is hard to accept that there is a *prima facie* case for ignoring Marx's work on empirical grounds.

The point is that empirical judgement has nothing to do with the way Marx is treated. Consider the following judgement from Roemer (1979:380):

Responses to this claim, of Okishio<sup>3</sup> and others, have been of three types. These are, first, what Fine and Harris (1976) call fundamentalist positions on FRP. Second, there are empirical discussions of whether or not the organic composition of capital is indeed rising. While this sort of investigation may be useful, it does not bear upon the *theoretical* issue of whether or not the rate of profit falls due to technical change. That is, either such investigation will be consistent with the Okishio conclusion, or it will not be; in the latter case, it would show the need for a different microeconomic argument of capitalist technical innovation; it would not, however, show Okishio's *argument* to be wrong. The empirical investigations, then, are certainly necessary, but they cannot provide refutation of a theory.

This is an extraordinary statement which deserves the most careful consideration. It is a categorical declaration that *no* empirical confirmation of Marx's theory is possible. The matter is totally clear: 'Empirical investigations.. *cannot provide refutation of a theory.*'

Marx's ideas – and, as we shall see, most counter-orthodox theory in economics – are ruled out on *purely* logical grounds. Economics in practice applies a deductivist method, all claims to inductivism notwithstanding. No matter how quantitative and observational its detailed 'normal science' practice, when it comes to the 'big' paradigmatic choices it substitutes reverse-scriptural authority for proof and logic for observation.

It does so in two steps. First, it restates the theories of its targets on the basis of an understanding inferior to theirs. Then it demonstrates that the result is a logical contradiction. Having done this, it then excludes these theories from further consideration. Logic, in the hands of economics, ceases to function as an instrument of development – as a means of arriving at new theory – and is applied, just as was the theological logic of the Inquisition, as an instrument of suppression, as a means of avoiding any and all theoretical choices which undermines the primary theological prejudice that the market is inherently self-perfecting.

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<sup>3</sup> The Okishio theorem is an extremely sound mathematical proof that the rate of profit cannot fall as a result of cost-reducing technical change – *if* we assign to the words 'rate of profit' and 'cost' the meaning which the equilibrium interpretation of Marx attributes to Marx himself. This is universally presented, including by Okishio himself, as a proof that 'Marx's' theory of the tendential fall in the profit rate cannot logically be true. The Temporal Single System Interpretation of Marx has definitively established, however, that the profit rate can indeed fall with cost-reducing technical change as Marx asserts, if we assign a different, non-equilibrium concept of these same words. See Okishio (1961) and the Kliman-Freeman-Foley-Laibman debate in *Research in Political Economy* 18 (Kliman and Freeman (2000)).

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## 4. TWO DOGMAS OF POSITIVISM

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### MOHUN'S CONJECTURE

In a recent reply to the position of Andrew Kliman, myself, and others which has become known as the 'Temporal Single System' interpretation of Marx, Simon Mohun<sup>4</sup> writes

The TSSI stresses that it is only an interpretation of Marx, and it stands or falls on interpretative issues. The claim is that only the TSSI definition of value, its notion of temporality, and the TSSI sign restrictions enable a replication of Marx's account of exploitation, so that the TSSI must be the correct interpretation. How should this claim be assessed?

...The recovery of Marxism as an analytical tool does partially depend on answers to the question 'what really did Marx say and mean?'. But only partially; it also depends upon what a Marxist understanding can tell us about the world in which we live. What is required is not an assessment of rival interpretations, but a theory for today's world and its use in empirical analysis. The construction of such a theory might well seek inspiration from Marx's own writings, but it is a coherent theory for today's world that is the goal.

This view represents, or rather parodies, the instinctive view of the unreflecting natural scientist, that *theoretical origins do not matter*. A theory is a theory is a theory. The only reason to ask where it came from is an antiquarian, eccentric, and more likely than not obscurantist interest in the bygone. Mohun's essential view, which is that of the general community of vague dissent, is that the option exists simply to conduct 'good' science within economics regardless of the dominant paradigm of economics or to the dominant paradigm of interpretation. From our standpoint in this article, this is roughly equivalent to a programme for creating atheist bishops.

It contains within it a concealed hypothesis, a conjecture: that the scientific method is so universal in its capacity to access truth that it works no matter the intellectual environment. Mohun's implicit assumption is that it is *intellectually possible* to do what he says: to construct a 'theory for today's world' and to 'use it in empirical analysis' quite independent of all other factors. By 'intellectually possible' we mean, and this could be a source of difference, that it is not merely possible for the single individual to make discoveries for her own satisfaction and those of a closed circle of friends, but that it is possible to put these discoveries at the disposal of the *demos*; that they can be disseminated, can survive, can become accepted, discussed, taught, and become part of the discourse of the intellectual community to which they are presented and who are materially the custodians of its promulgation. In short, it is possible to conduct what Althusser used to call 'theoretical practice' quite independent of the theoretical environment in which one conducts it.

This I deny.

Mohun's apparently commonsense view is in line with a definite intellectual tradition, arising from the battle led by the Austrian circle to establish a consistent positivism as the foundation of all scientific practice. Positivism may be outmoded philosophically but it persists as the bedrock prejudice of 'everyday' natural science and, as already indicated, it is central to the way that economists as a whole – for whom words such as 'postmodernism' or 'critical realism' are usually just so much metaphysical

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<sup>4</sup> Mohun, S. 2003. "On the TSSI and the Exploitation Theory of Profit," *Capital and Class* 81, Autumn 2003, pp85-102.

nonsense – situate their discipline within the sciences. It is, in fact, the ‘representative’ philosophy of the twentieth century – it is a core constituent element of the way in which modern science, and modern economics, were constructed. To understand modern economics, we need to go back to its logical positivist foundations.

However it has escaped notice that the positivist project is in itself normative, not positive. It sets out to define ‘good’ science: a good scientist is a neutral observer whose theories are determined only by correct experimental practice and by observation, without recourse to metaphysics. It constitutes, not a *judgement* on what science consists of, but a *prescription* about how to conduct it. If its recommended practice is adhered to, then theory is indifferent both to the personality or social context of the scientist and to the manner of expression of the scientist’s ideas.

The key word is ‘if’. If such a practice is socially impossible, positivism is nothing more than a good idea. It is not a recipe for scientific practice, because it contains no means to conduct science in a religious environment. It contains no provision for the social survival of individual advances or discoveries, for translating isolated good ideas into social acquisitions. It contains no defence against the suppressive mechanisms of any branch of knowledge whose selection procedures are not themselves positivist.

Many scientists are instinctively aware, if only through the horror and disbelief which their first encounters with it inspire, that economics has a positivist philosophy, but does not have a positivist practice. Their instincts lead them to believe, however that is possible to practice ‘good science’, in economics as in natural sciences, simply by testing individual ideas against the evidence of the senses. The primary direction of scientific enquiry, they instinctively feel, should be to adopt working theories, devise hypotheses within them, and conduct tests of these hypotheses to accept, reject or modify them. Any enquiry into the origin, history, or interpretation of these theories, is secondary or irrelevant and in particular, cannot influence the outcome of the tests. In short ‘good science’ is at least possible without critical thinking, and certainly possible without hermeneutics.

The principle thesis of this article is that this view is radically false. Mohun’s conjecture is not a proven thesis but an unproven dogma. Furthermore on the evidence of the actual conduct of economics, it is empirically and inductively false. Economics in fact restricts the production of reproduceable knowledge (ie knowledge that disseminates and survives) to ideas which are determined by the esoteric requirement of explaining and justifying the social order, not ideas which are determined by the exoteric requirement of explaining what is observed.

I will argue that a scientific economics (more correctly, political economy) therefore cannot be reconstructed on a scientific basis without critical method, that is a primary focus on the meaning and origin of theory, and in particular on its esoteric, not its exoteric, functions.

I assert moreover that the survival of scientific theory within economics is possible only as a secondary outcome of a reform project, a radical transformation of economics in which critical analysis – that is, an assessment of the origin of theory as a necessary instrument in its development – is displaced from the margins to the centre.

This in turn is possible only to the extent that the reform project is adopted, and indeed, imposed, outside of economics; economics will not reform itself. I hence address this text to non-economists (of whom, fortunately, there are some within the profession of economics); to people that have sufficient distance from the essentially theological practices of the discipline to understand that it cannot go on as it is, and maintain any pretence to be scientific.

## THE DOGMA OF INEVITABLE PROGRESS

Positivism has a travelling companion: the doctrine of irreversible progress. Mohun's conjecture in fact reduces to this doctrine, and is an outcome of it.

If the subjective origin of a theory has no influence upon its content, then knowledge can only move forward, at least as long as positivism is practiced. Since modern science, beginning with the enlightenment, espoused a rationalism which after Galileo carried all before it like a tidal wave, it is at least absurd and almost certainly obscurantist, at any given point in the evolution of theory, to refer back to a previous body of theory, since modern theory must necessarily encompass the entire body of prior knowledge, plus everything that the subsequent experience and intellectual endeavour of humanity has added to it.

Newton's often-quoted<sup>5</sup> dictum that he had seen far because he stood on the shoulders of giants conveys an implicit iconic message: all we must do to penetrate nature's mysteries is stand on the shoulders of those who went before.

The pervasiveness of this implicit progressivist teleology is clearly evident in such throwaway expressions as 'we now know' (as opposed to 'we now think'). But it is absolutely explicit in the extremely general idea that any interest in *what Marx actually said*, or *What Keynes actually meant*, is necessarily obscurantist. Indeed Ben Fine, in coining the now widespread term 'fundamentalist' for the defenders of Marx's Law of the Tendency of the Rate of Profit to Fall, explicitly asserts that the only motivation for an interest in the texts is the substitution of 'Marx's word' for empirical evidence. This is the entire thrust of an important contribution of Laibman<sup>6</sup> who, arguing that 'the road to the 21<sup>st</sup> Century lies through the 20<sup>th</sup> Century', insists that any attempt to set aside the re-interpretation of Marx by his successors can only lead to an impoverishment of knowledge.

The accumulation of knowledge is thus equated to a kind of intellectual geology. Each generation is assumed to lay down a stratum of knowledge, a kind of compost which crushes beneath it all that precedes into a fossil record of a subject whose only importance is its surface topology. Hermeneutics is from this standpoint an exercise in paleontology. It may lay bare the embryonic forms of our present knowledge but cannot unearth anything with evolutionary superiority.

Let us pause to consider whether or not the unceasing advance of knowledge is in fact a universal law of history or whether, indeed, the progress of any knowledge may accurately be treated as a geological accretion. Let us begin with Copernicus. Actually, he did not invent his system. It was laid down in the second century B.C. by Aristarchus of Samos. It came, not just from a previous century but from a previous millennium, entombed by fourteen centuries of obscurantism. Moreover Copernicus was perfectly aware of Aristarchus's theory and refers to it in his theory; the Copernican movement was in essence a component part of the Christian Humanism of the early Mediaeval period, awakened to knowledge of the earlier, and more scientific, classical works made available to the West by Arab scholars from the 12<sup>th</sup> Century onwards.

The following one and a half millenia perfected, with a complexity so great that it is still hard to follow today, an alternative system invented by Eudoxus, sanctified by

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<sup>5</sup> And now questioned as, in its intentions, an ironic reference to Newton's opponents: see (TBA)

<sup>6</sup> Laibman, 'Rhetoric and Substance in Value Theory: an Appraisal of the New Orthodox Marxism' in Freeman, Kliman and Wells (eds) 2004, *The New Value Controversy in Economics*, Aldershot and Vermont: Edward Elgar

Aristotle and perfected by Ptolemy. The movement of all heavenly bodies was explained by fifty-three concentric spheres, complete with epicycles, each turning on a different axis. This system predicted almost all the observed positions of the stars and planets, as well as eclipses, with very great accuracy. Except for comets, it was only when Galileo turned his telescope on the moons of Jupiter that any really serious conflict between theory and observation emerged.

Yet progress did not come through a forward development of this dazzling system, now unknown and forgotten. It arose in a return to an *earlier* system of thought.

A simple reaction, the 'simplified enlightenment' outlook, is that this was a one-off case which arose because of a generalised regression – the so-called 'Dark Ages' – during which the whole of classical antiquity was obscured by religiosity. This constructs a mythical view of scientific progress: in the Dark Ages everything was bad, with the progress of history everything gets better.

There are three problems with this naïve reaction. First of all, it is clear from modern scholarship that both Copernicus and Galileo were profoundly religious. In no way did they consider their system in conflict with the teachings of the bible and, indeed, Galileo to his death believed (rightly) that only by adopting his system could the Catholic church survive. Nor was this an impossibility: his ideas were well-received within the Catholic hierarchy and before the reaction, the Pope adopted the habit of having Galileo's works read to him in his bath.

Second, reaction again Aristarchus began not in mediaeval times but in Aristarchus' own circles. Cleanthes called for him to be publicly burned for the sin of 'displacing the earth from its rightful place at the centre of the Universe'. The triumph of the Ptolemaic system arose not because of the way science was conducted, or because 'science was rational, and religion was not' but because the *particular scientific conclusions of Aristarchus were incompatible with the social order*.

Third, it is clear from a much more modern example that regression – or at least the absence of progress – is not a feature of a particular *age* but of a particular *type of knowledge*. This example is the doctrine of evolution itself, which was being theorised two generations before the Voyage of the Beagle (originally by Darwin's grandfather Erasmus) and for which there was ample evidence through the work of geology and the discovery of fossils, without any need to travel to foreign parts. This well-evidenced body of theory was suppressed, and vanishes from history, for over fifty years despite an overwhelmingly generalised 'positivist' practice in the area of the mechanical sciences. It reappears only when it becomes interesting and useful to begin theorising economic competition itself as 'natural' as a justification for the rapacious barbarity of capitalism.

Moreover the debate around evolutionary theory is shaped and dogged to this day by this essentially ideological constraint. Biology as Gould and Lewontin have so pointedly observed is virtually paralysed by preconceptions – for example the very polarisation of 'nature and nurture' – which have little to do with experimental observation itself and, to the contrary, arise because of the need to think about human society as a manifestation of the animal kingdom – that is, biology works within a methodological infrastructure determined, again, not by positive practice but by the requirements of the social order.

Thus not only can 'science' go backwards, it actually has done so for large periods of history and is *still doing* so in definite cases. There is no universal progress of the sciences. Some branches of knowledge, particularly Catholic cosmology in the high middle ages, are profoundly retrogressive in their overall impact on thought. Others, notably enlightenment cosmology, are profoundly progressive.

If, then, regression in knowledge is characteristic not of an age but of a *branch of thought*, we need to enquire what it is about a given branch of thought that gives it its capacity for regression or progress. On what basis does it make its fundamental choices?

Most superficial explanations offered for the retrogressive impact of pre-enlightenment Catholicism are highly superficial and on close examination, do not hold water. This is particularly true of sweeping generalisations which seek to assign to all religion the functions of the Mediaeval Catholic Church and to all sciences the functions of the enlightenment.

It is *not true* that 'science is rational, whereas religion is superstitious'. To the contrary, all three monotheistic religions<sup>7</sup> have made fundamental contributions to modern logic and indeed, modern deductive logic (not to mention modern mathematics) originates not so much with the work of Descartes as with the studies of Aquinas, of the Cabbalists, and the Islamic algebraists. It is *not true* that scientific discovery prevailed only with the enlightenment. To the contrary, many of the most fundamental technological advances on which later capitalist society based itself were creations of the mediaeval age (the clock, the windmill, modern agriculture, etc etc etc). It is *not true* that sciences advances continuously from the enlightenment onwards.

It is not at all my intention to offer an apologia for religion. My point is that the 'naïve' view of science does not, on closer study, hold water and we cannot therefore assume *a priori* that just because a body of knowledge (eg economics) applies a body of methods it is necessarily incapable of playing the same role both in its relation to society, and in its impact on knowledge, as Mediaeval Catholicism. The real task is to look, concretely and critically, at what each such branch of knowledge, whatever its subjective claims to scientificity, *actually does*.

## TESTING MOHUN'S CONJECTURE: HOW DOES ECONOMICS ACTUALLY CHOOSE ITS THEORIES?

Mohun's conjecture may be tested empirically, by observing how economics has actually behaved, at crucial junctures in its history.

In the course of this article I will specify and explain two generic types of system: an *equilibrium* or simultaneous system (1) and a *temporal* system (1a). Systems (1) and (1a) provide different predictive systems yielding alternative quantitative predictions of reality. This suggests a simple discriminatory principle to determine what constitutes scientific practice: the principle I propose is, I hope, non-contentious: a practice is not scientific unless, faced with two alternative theoretical accounts giving different quantitative predictions of observables, it conducts research to verify which theory provides the best predictions of the observables. Note this is a necessary, but not sufficient condition. It is however clearly necessary; I take it merely from the literature. I have never encountered any meaningful conception of scientific practice within which a *refusal* to test a theory that is definitely available is accepted as scientific.<sup>8</sup>

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<sup>7</sup> I cite these only because I am more familiar with them, not because of any implication that other religions are less rational.

<sup>8</sup> Recall that we are discussing normative concepts of scientific practice; that is, what we think scientists *ought* to do, in order to produce good science. Many people such as Kuhn have discussed whether scientists actually conform to this ideal and has pointed out that in practice, they spend a lot of their time working out the consequences of a single theory, rather than testing new ones. I have, however, never heard anyone claim that a refusal to

Even if we do not take the above as a 'standard' definition of science, it should be clear that Mohun's conjecture is refuted by any body of theoretical practice that does not conform to it. If, confronted with a certain set of theoretical propositions (heliocentrism, evolutionary origin of species, etc), the community responsible for developing the theory systematically refuses to subject them to empirical testing, and if these theoretical propositions are true, then knowledge cannot be 'generated' from within this community, and Mohun's conjecture falls.

Well, what does economics actually do? The answer is, *in every case and in every school and in every subject*, faced with the choice between (1) and (1a), it either adopts (1a) immediately, or it gravitates rapidly towards it without testing (1), or even worse, *having tested* (1) it *nevertheless* excludes it from consideration. Thus

1. Ricardo himself; in my view, his profit rate determination is simply ambiguous. If determined by the value approach, profit rates are not equal; if determined by the 'price of capital' approach, they are. Ricardo himself never comes down on one side or the other. But the Ricardians *always* come down on the equilibrium side
2. Marginalism; Böhm-Bawerk considered simultaneous (equilibrium) analysis 'a mortal sin'. Walrasian general equilibrium was a minority tradition really until Marshall finally plumped for it on a basis that Dobb (studies in value theory) identifies very accurately. Criticising Jevons, he argues that Jevons has a causal chain which does not match the temporal chain. Jevons wishes to say that final demand causes value which causes labour demand. Marshall berates this 'catena of causation' on ideological grounds (cf second Plato citation); he says that since labour demand comes before final demand in time one will be driven to admit that labour is the cause of value. Therefore one must liberate economics from its 'successivist' prejudices and substitute 'mutual causation' – that is write down a set of simultaneous equations from which time has been eliminated.
3. Marx (subject of separate paper); there is a clear and specific project to 're-invent' Marx as a simultaneous equation theorist. Bortkiewicz is completely explicit about the Walrasian and Marshallian necessity for this:

Alfred Marshall said once of Ricardo: 'He does not state clearly, and in some cases he perhaps did not fully and clearly perceive how, in the problem of normal value, the various elements govern one another *mutually*, not *successively*, in a long chain of causation'. This description applies even more to Marx ... [who] held firmly to the view that the elements concerned must be regarded as a kind of causal chain, in which each link is determined, in its composition and its magnitude, only by the preceding links ... Modern economics is beginning to free itself gradually from the successivist prejudice, the chief merit being due to the mathematical school led by Léon Walras.<sup>9</sup>

4. Bortkiewicz corresponded with Walras from the age of 19 and saw his contribution to economics as, essentially, doing to Marx what Walras did to marginalism. As for Walras himself:

A Truth long ago demonstrated by the Platonic philosophy is that science does not study corporeal entities but universals of which these entities are manifestations. Corporeal entities come and go, but

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test a new theory is a good thing or that their suppression constitutes improved scientific practice.

<sup>9</sup> Bortkiewicz (1952:23-24). I am indebted to Michele Naples for pointing out this passage.

universals remain for ever. Universals, their relations, and their laws, are the object of all scientific study. (p61)

5. Keynes/Kalecki: the real power of Keynes' analysis is a non-equilibrium framework. Quite apart from many considerations which many more expert than I can explain, his whole point was to say that one may not assume full employment. But full employment is an equilibrium condition. Yet within a few years of the publication of the *General Theory*, Hicks had already re-theorised it as an equilibrium model, and since then every generation of students is basically told that Keynes 'is' ISLM.
6. One of the principle instruments for inserting non-equilibrium analysis into Keynes' framework is the *uncertainty of the future*. Rational expectations is nothing more than a means of putting this genie back in the bottle, by the unbelievable device of simply supposing that whatever agents believe about the future, is in fact what will actually happen.
7. Another personal story: Most startling of all is the evolution of 'non-equilibrium Walrasian' approaches which had a promising beginning. Five years ago I asked one of the founders why nothing more is being published in this school. 'because unless the words "Rational Expectations" are in the title, no-one will publish it – came the answer.
8. Perhaps the only promising recent development has come in recent econometrics with a real recognition that time-series analysis on simultaneous equation lines introduced insurmountable problems of serial correlation. But this was known eighty years ago! 'Process theory' gave way to Haavelmo's simultaneous equation approach, which was standard until very recently.

Empirically, the history of the last century and a half in economics is, I think, an emphatic refutation of Mohun's conjecture. Moreover this refutation is not vitiated by the fact that a great deal of apparently objective quantitative research is conducted claiming to reproduce or draw on the 'insights' of Keynes or the 'insights' of Marx, but within a type (1), equilibrium system. The point is what economics does when faced 'large choices' between paradigms. A 'large choice' corresponds to a paradigm change in the sciences.

Economics, having made a large choice, may conduct a great deal of very practical work, applying a great deal of standard scientific objectivity including much collection of data, production of regressions, testing of hypotheses, and so on – just as Mohun proposes. However, I contend, if the large choice excludes a scientifically superior paradigm, there is actually no way that such busy quantitative activity can arrive at any conclusion within this superior paradigm. Thus imagine that in the history of cosmology thought moved in reverse, replacing Copernicus in 1650 by Ptolemy. There is simply no programme of quantitative astronomical research, no matter how objective or how attached it might proclaim itself to the 'insights' of Copernicus, that would be capable of restoring the Copernican paradigm. One can even imagine a diligent band of 'Copernicans' filling up token posts in liberal arts colleges, churning out monographs claiming to reproduce 'Copernican' conclusions in a heliocentric universe. Such monographs would not advance the cause of science a single iota.

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## 5. DEEP IN THE HEART OF PRAXIS: HOW RELIGIONS CHOOSE THEIR PARADIGMS

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What actually makes a theory 'go forward' or 'go backward' – indeed, what do these so far rather vaguely-formulated ideas actually mean? To clarify the issue I will return to the primary thesis of this article, the distinction between scientific and religious modes of thought. I will argue that the distinction is valid but, following Kuhn, cannot be made simply on the basis of the *form in which the knowledge is expressed* but the *practice through which it is adopted*.

We therefore depart quite radically from the enlightenment view of religion itself, which takes as the defining characteristic the primary theoretical assumptions which are made by a body of knowledge. From this point of view, for example, what defines a religion is the fact that it assumes Gods.

But actually, this leaves a lot to be desired. Science is, and was, practiced by many very reverent people who for their own part at least, believed and preached an intimate connection between their religious beliefs and their scientific conclusions, not least Galileo and Newton. Moreover there are very major religions that do not suppose a God, such as Buddhism.

A more developed form of the enlightenment view is that religion is distinguished by its adoption of unexplained and unverifiable hypotheses, based for example on faith. The critical realist tradition has questioned this enlightenment prejudice in some depth, beginning with Ian Barbour's<sup>10</sup> claim that science cannot justifiably assert that it is the only source of knowledge. Curiously enough, these insights have largely focussed on enlightenment concept of science. A far less significant amount of effort or resources have gone into re-examining the enlightenment concept of religion. Science's view of itself may have changed, but its view of religion is little changed, and still more or less matches Ian Barbour's description:<sup>11</sup>

Many people view science as objective, universal, rational, and based on solid observational evidence. Religion, by contrast, seems to be subjective, parochial, emotional, and based on traditions or authorities that disagree with each other.

Actually, this is not a bad description of much economics. But where is the God of economics? Can one have a religion without Gods, a religion that *looks like* a science? We shall pursue this enquiry by returning to a well-researched but legendary episode in enlightenment history. The archetypal enlightenment hero is Galileo. So let us consider the *actual* argument advanced by Galileo's principal scientific antagonists, Clavius

If the position of Copernicus involved no falsities or absurdities there would be great doubt as to which of the two opinions – whether the Ptolemaic or the Copernican – should better be followed as appropriate for defending this kind of phenomena. But in fact many absurdities and errors are contained in the Copernican position – as that the earth is not at the centre of the firmament and is moved by a threefold motion (which I can hardly understand, because according to philosophers one simple body ought to have one motion) and moreover that the sun stands at the centre of the world and lacks any motion. All of which conflicts with the common teaching of philosophers and astronomers and also seem to contradict what the Scriptures teach.<sup>12</sup>

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<sup>10</sup> Cf Barbour (1996, 2004)

<sup>11</sup> Barbour (xxxx) *religion in the age of science*

<sup>12</sup> From Clavius' commentary on Sacrobosco's *Sphaera*, cited in Lattis (1994:249)

What is the actual basis on which Clavius rejects Copernicus' theory? *Not* because he chooses to ignore empirical reality. To the contrary it took two centuries of development before Heliocentric theory could produce consistently superior empirical predictions. *Not* because Clavius rejects the application of rational logic. His objections are framed in terms of an impeccable logical argument, given the premise. *Not* even primarily because it fails to conform to the scripture. This is added by Clavius as a secondary clincher, in a very qualified form ('seems to' contradict the scriptures). Moreover the Scriptural evidence against heliocentrism is extremely scant and almost reduces to a single passage in which Joshua holds up the movement of the sun in order to secure victory in battle.

All these conventional interpretations of the anti-Copernican camp's standpoint are a mythology constructed by Viviani, the devoted young protestant acolyte of Galileo's declining years. They are a simplified propaganda 'spin' constructed by ascendant Protestantism in its ideological battle with the church, later modernised by Brecht as part of the second International's reconstruction of Marxism as a simple continuity of enlightenment battling the dark and ignorant forces of reaction.

Clavius's central objection is the 'absurdity' that the earth is not at the centre of the universe. The specific word 'absurdity' demands our attention. A proposition can be empirically wrong without being absurd, eg if we say a body of water is curved when in fact it is flat. A proposition can be a false logical deduction, as when we say that because lakes are a body of water, every body of water must be a lake. Absurdity is beyond this. It means the very proposition cannot be sustained because it requires us to think in an impossible way: for example, to conceive of a tiny giant or an honest liar. Kuhn conveys this point very well:<sup>13</sup> the problem is not just that the Ptolemaics denied the earth was empirically anywhere other than the centre of the universe: 'centre of the universe' is part of what they *meant* by the word 'earth'.

The dispute between the Galileans and the Ptolemaics was thus in essence a dispute about meaning – about ontology. The Ptolemaics simply thought about the universe in a different way. For them, the earth *had* to be the centre of it because it could not be in the heavens. Otherwise, the heavens would be imperfect. But their explanation for almost everything observed was an outcome of departures from perfection. The ideal form of the world was its true reality. The further away from the world of human experience, the more perfect. As perfection descended into the terrene domain it became gradually more and more corrupt, so that the actual world of experience was a distorted manifestation of this distant perfection.

This is made absolutely explicit by Plato himself, the unchallenged father of all cosmological social theory, of all accounts spanning the natural order of the universe and the social order of humankind.

"Anyone can see that [astronomy] forces the mind to look upwards, away from this world of ours to higher things", says Glaucon. "Anyone except me," says Socrates. "I cannot think of any study as making the mind look upwards, except one which has to do with unseen reality. No-one, I should say, can ever gain knowledge of any sensible object by gaping upwards any more than by shutting his mouth and searching for it on the ground, because there can be no knowledge of sensible things...These intricate tracteries in the sky are, no doubt, the loveliest and most perfect of material things, but still part of the visible world, and therefore they fall far short of the true realities – the real relative velocities, in the world of pure number and all perfect geometrical figures, of the movements which carry round the bodies involved in them. These, you will agree, can be

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<sup>13</sup> Kuhn, T. S. (1962) *The Structure of Scientific Revolutions*. Chicago and London: University of Chicago Press

conceived by reason and thought, not seen by the eye ...Accordingly, we must use the embroidered heaven as a model to illustrate or study these realities...If we mean, then, to turn the soul's native intelligence to its proper use by a genuine study of astronomy, we shall proceed, as we do in geometry, by means of problems, and leave the starry heavens alone" (*Republic* 7:529A cited in Sambursky (1987:44))

The central idea in this is the following: the spheres are not something in which the planets travel: they *are* the planets; they are the true reality, not accessible directly to the sensations, of which the perceptible visible sky is merely an imperfect manifestation.

The ideological purpose is perfectly explicit; Plato requires that the gods should reside in the heavens and should therefore be inaccessible to the common people: "When you and I try to prove the existence of the gods by pointing to these very objects – sun, moon, stars and earth – as instances of deity and divinity, people who have been converted by these scientists [Anaxagoras -AF] will assert that these things are simply earth and stone, incapable of paying any heed to human affairs" (*Laws* 10:886D cited in Sambursky(1987:54))

From this point of view, the earth simply had to be at the middle of the thing because otherwise, the heavens could not be an empirical manifestation of perfection. Thus one of the greatest disturbances caused by Galileo's observations, inexplicably from the modern standpoint, was the discovery of mountains on the moon. Pre-Galilean theory held that superlunary material was adamantite and incorruptible – and could not depart from perfect circularity.

The problem facing the pre-Galileans was that in order to conceive of the earth as being in the heavens, they simply could not go on *thinking* in the same way. All concepts, all ideas, all meanings had to be re-assigned: causality, place, motion, everything. Galileo, whose universals were constructed from his painstaking observations of weights, pendulums, and all manner of terrene bodies, did not start from the heavens. He started from the earth. He did not suffer from his opponents' conceptual difficulties because he had long ago stopped thinking like they did.

This, however, explains everything and nothing. It does not explain, in particular, why some people could make Galileo's leap of understanding and others could not. It does not explain Galileo's eventual rejection and condemnation. In particular it does not explain why people such as Bellarmine, who were indeed capable of understanding Galileo's viewpoint and was even a childhood friend, found no contradiction in leading his persecution.

Plato's answer to Anaxagoras makes it clear that the answer to this problem lies elsewhere than in the domain of logic and elsewhere than in the domain of observation. What prevented further advance was that the cosmology of the time was not merely a theory of nature but also a theory of society. It was an account of human conduct, of the social order. To displace the earth from the centre of the universe did not just deprive Galileo's peers of an explanation of material movement; it deprived them of an explanation of why they were there to observe it. If perfection did not reside in the heavens and corruption on earth, where did the power of the papacy derive from? From where did the rights of kings descend? Why should some men rule, and why should others accept this? If the heavens contained 'mere lumps of rock' then there is no proof of the astral gods, and if there are no astral gods then human beings cannot be their descendants or delegates.

These matters were not regarded as separate by Galileo's contemporaries any more than by Plato. It was a theoretical requirement of any theory of the universe that it

should simultaneously function as a theory of society. Humans were (and still are) part of nature.

A theory of natural existence which contains, within itself, a natural place for authority, must provide a natural explanation of why that authority is in authority. This is not simply a cynical manoeuvre. The authority itself must be confident of its own position, its own place in the universe, or it will be consumed with self-doubt and fall into an ontological and faithless vacuum.

The problem, in summary, was not that a completely logical, rational and positivist account of the world ran headlong into confrontation with an alternative account ruled by superstition in alliance with cynically corrupt power. It was that both the theory of the Ptolemaics and the theory of Galileo were simultaneously theories of the social order. They were cosmologies. The concepts that they deployed were part of a universal apparatus of thinking applied without distinction to things and to people. The two functions of this apparatus, as a means of thinking about people and as a means of thinking about things, could not be arbitrarily separated in though without a parallel separation among the thinkers themselves, and their polarisation into two opposed camps.

The distinction between the religious and scientific mode of thinking, as this arose in the actual historical formation of enlightenment science, appears therefore as a distinction between *those for whom the esoteric ontology is primary in the selection between competing paradigms, and those for whom the exoteric ontology is primary.*

The defining feature of the religious mode of knowledge, as with the scientific, arises not in its 'normal scientific' or 'normal religious' every day practice but at those points when it has to make a choice between 'big theories', between paradigms. The scientific mode of thought chooses that which best explains *nature*, regardless of the contradictions this creates for understanding the social order; the religious mode of thought chooses that which best explains the *social order*, regardless of the contradictions this creates for the understanding of nature.

From antiquity until the ownership of land and labour became generally alienable and hence monetised, aristocratic social power was rooted in the *person* of the aristocrat. This personal power is what had to be explained. This essential intellectual function was played by cosmology, because it explained why monarchs and aristocrats existed, and why they had power over others. With the rise of money and the commodity form, *personal* power fades and power arising from *abstract wealth* rises. Hence, it is the power of the owners of commodities, and above all money, that requires justification. it is no longer the monarch who obstructs the course of history but the financier, and the place of cosmological religion is taken by monetary religion, which provides just as absolute a justification for the actions of financiers as pre-Copernican cosmology did for the monarchy

## WHY DO THE SOCIAL SCIENCES EXIST?

How did this separation arise at all? To put it another way, what made science possible? Essentially, a peculiar and rather cowardly escape route taken by the enlightenment, which with a very few, rather inevitable exceptions (cosmology, evolution) avoided all direct conflict with religious authority by creating an artificial division between man and nature.

(TBA evidence from Uglow)

This created the division of labour which permitted the engineers and physical scientists, from the men of the *ponts et chaussées* to the Victorian engineers, to get on with the practical business of transforming nature whilst not only avoiding confrontation with the business of running the world. It also created the environment in which hermeneutics could apparently cease to matter. Newton entertained a passionate belief in astrology. So what? He propounded a theory of matter which could be perfectly adapted, with almost no change, to the practical business of building machines independent of its theological consequences. Faced with the very contradictions that Plato feared, natural science appears at every serious point to have given way to authority on religious questions, in order to preserve its dominion over the material. Laplace's answer to Napoleon's question 'where does God fit' is archetypal: 'je n'avais pas besoin de cette hypothèse'.

This is not, however, without its difficulties. How should we suppose that a truly universal theory of things is not also a theory of society, of how knowledge passes into action and indeed, how knowledge is itself formed? And society has exacted its revenge on 20<sup>th</sup> century natural sciences, in the form of that most mysterious of creatures, *the observer*, the alienated incarnation of the social reality of the scientist. First through relativity, and then through quantum physics,<sup>14</sup> the dualistic structure of late nineteenth century materialism, with its neat division into society on the one hand, and nature on the other, has collapsed onto a sickbed of its own making.

The social sciences exist, in short, because the natural sciences renounced responsibility for society. It follows that *neither* branch of knowledge can adequately advance without a synthesis. In the critique of social science that follows, above all economics, it should not be supposed that we follow the standard assumption that the natural sciences offer a paradigmatic or universal model for the attainment of knowledge. Rather, we consider the natural and social sciences as equal victims of their artificial separation.

Equal victims though they may be, they are not travelling in equal directions. The natural sciences continue to advance; economics continues to regress. Let us ask why.

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## 6. TWO WORLD VIEWS: ACCIDENT, REALITY AND MOTION IN THE SOCIAL AND NATURAL SCIENCES

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We now turn to the core concept and, we will show, the core debate within economics. The rest of this article is dedicated to developing the distinction between what we consider to be *two world views* which arise within economics at each point where it faces a paradigmatic choice, which we term the *equilibrium* or simultaneist view, and its alternative the *temporal view*.

We encounter an initial problem already alluded to: by the same word, those with different world-views mean different things. Not least, they mean different things by the word 'equilibrium' itself. Therefore we cannot take the word 'equilibrium' outside of its specific contexts as if it meant the same thing to everyone, for example by assuming that because statistical mechanics uses the word 'equilibrium' or because the word 'dynamic equilibrium' appears in the language of chemistry, and because this same word appears also in the vocabulary of the economist, that this word means the same thing to a chemist, a physicist, and an economist. Nor can we get

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<sup>14</sup> Out of respect for Alan Sokal I refrained from calling this article 'the hermeneutics of quantum gravity'. The temptation was great.

around the problem by simply establishing, in the positivist manner, a 'correct' definition of equilibrium and demanding that the economists apply it.

Instead we have to get inside the heads of the economists and ask how what it means to them, how it fits in their system of concepts. That is, immediately, we need to enquire as to the origin and structure of their whole theory, before we can understand any part of it on its own.

In order to do so, we need to understand how the concept of equilibrium sits in relation to all other concepts of economics. I will show that in fact, in the way that economics conceives of equilibrium, it determines the entire ontological structure of economic thinking. It results in a specific concept of price, of value, of profit and, not least, of what is meant by the idea of *cause*, which is quite distinct from that the temporal world view, which also dominates in most natural sciences. The main thing the non-economist has to get used to, when trying to understand how economic theory works, is that the economist does not mean the same thing as anyone else when she or he uses the same word.

Our aim is to establish the social content of this conceptual usage; we need to establish how it relates to the social order within which it resides. How does it facilitate the governing of society? Which social classes, which elites, and which social orders are served by thinking in this way, and which are not? Which would suffer if society did not think in this way, and which would gain? In this way we can investigate the real 'selection criteria' of economics and the relation between their esoteric and exoteric content.

This way of approaching things can be crudely applied or misinterpreted. The purpose is not instrumentally to develop an 'alternative theory that serves the right people'. The goal of an objectively valid theory is not abandoned. The point is, however, that the theory we now possess can neither be understood, nor improved on, without understanding where it comes from. And this in turn cannot be understood without understanding how it permits people to function as social agents; that is, how it reinforces or impedes them simply living their lives.

Behind this lies a deep question which we have until now only alluded to tangentially. In the case of the social sciences, it is impossible to distinguish subject from object in the way that the natural sciences have tried to do, above all in the 19<sup>th</sup> Century. What is being thought about is the same as what is doing the thinking; one cannot suppose a neutral experimenter *over here*, and a society *over there*; the experimenter is the society itself. In economics, one in fact encounters the very same problems which have plagued 20<sup>th</sup>-Century natural science in a different context: how to frame scientific theories (which essentially deal with the relation between observer and observed) when the observer herself is necessarily part of what is observed. The problems encountered by relativity and quantum physics are, we think, in some deep sense the same as those encountered much less successfully by economics, namely, how to speak meaningfully about an object that is simultaneously a subject.

## TWO CONTRADICTIONARY IMPULSES OF RICARDIAN DYNAMICS

In one of his earliest works on political economy, written in 1845, Marx makes an observation which contains a root-and-branch critique of the Ricardian concept of 'economic law':<sup>15</sup>

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<sup>15</sup> I am indebted to Julian Wells for drawing my attention to this excerpt.

Both on the question of relations of money to the value of metal and in his demonstration that the cost of production is the sole factor in the determination of value Mill succumbs to the error, made by the whole Ricardo school, of defining an *abstract law* without mentioning the fluctuations or the continual suspension through which it comes into being.

If e.g. it is an *invariable* law that in the last analysis – or rather in the sporadic (accidental) coincidence of supply and demand – the cost of production determines price (value), then it is no less an *invariable law* that these relations do not obtain, i.e. that value and the cost of production do not stand in any necessary relation.

Indeed, supply and demand only ever coincide momentarily thanks to a previous fluctuation in supply and demand, to the disparity between the cost of production and the exchange value. This is the real movement, then, and the above-mentioned law is no more than an abstract, contingent as one-sided moment in it.<sup>16</sup>

Underlying this difference is an entirely different view of the relation between *accident* and *reality*, between chance and ignorance. This is a preoccupation for 20<sup>th</sup> century science, particularly quantum physics, and is thus not a particular problem of economics but a general concern of science. What is interesting is that, without anyone paying much attention to it, the question surfaced far earlier in the social than in the natural sciences. In Hegel's words, becoming is the Truth of Being. From Marx's point of view the *reality* – the 'fundamental law' consists of accident, movement. If we abolish this movement in order to fix our ideas, we arrive at an 'abstract, contingent and one-sided moment in it.'

The alternative view, already implicit in Ricardo's predecessors, is that 'fundamental laws' are what we get when we assume that accidents cannot happen: or, which is the same thing, that we may study an interconnected structure by supposing that it *does not change in time*. These are the temporal, and the equilibrium view, respectively.

With this distinction, political economy bifurcates. Let us try and see why.

First, let us relate the question to what is probably the most well-known thing about Ricardo, which is that he could not make up his mind. Developing the concept of value to its highest point so far, he concluded that there were two definitions of the value of a commodity which were incompatible with each other. This in turn arose because it could be determined, by which Ricardo means 'calculated', in two ways: either as the magnitude of value contained within it, or as its price, given by the assumption that profit rates are equal in all branches of the economy.

Ricardo derives the equal-profit-rate assumption from his idea that profit is in some sense the 'price' of capital, the return that an investor receives for parting with the use of her money. But in fact, the view that there is a single profit rate and a single price of capital requires a further, prior assumption: that the economy is in some sense 'at rest', that there are no further impulses tending to lower or raise the profit rate in one particular branch of the economy, such as an imbalance between demand and supply.

TBA LONG RUN and 'natural price' in Ricardo

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<sup>16</sup> Marx (1975:260). The emphasis on the first two sentences in the third paragraph is mine. According to the translators, in the second paragraph the bracketed words are written above the word that precedes them, that is the word 'accidental' is written above the word 'sporadic' and the word 'value' is written above the word 'price'

We do not need any such assumption to determine the magnitude of value by the quantity of labour in it. If we say, for example, that a jacket contains ten hours of labour because the cloth in it contains five hours, and the tailor works five further hours to produce it, then we have made no assumption whatsoever about whether the jacket will be sold, what profit it makes or indeed, what price it will actually sell for. The idea that value is equal to the magnitude of past value does not require us to assume equilibrium or a balance between demand and supply.

Ricardo's 'two definitions' of value thus in fact bifurcate in a way that the modern interpretation of Ricardo (and Marx) has obliterated. The modern interpretation assumes that these are two alternative 'equilibrium' definitions of price, one of which assumes that profits equalise perfectly whilst the other assumes they do not equalise at all. In fact, the first is not a definition of price at all but of its content, value. Ricardo sees a contradiction only because his attention is fixed on the quantitative determination of price, and believes it must be equal to value. Once price is allowed to depart from value for any reason whatsoever – for example, as a result of the normal fluctuation of the market – this relation cannot hold. Ricardo's problem is therefore completely solved if, as did Marx, we treat value not as quantitatively equal to price but as its *content*, as the substance of which it is composed. Looking at things in this way makes perfect sense of the statement, for example, that the coat has a value of ten hours but a price of twenty. This statement simply asserts that because the market has produced a temporary shortage of coats, it permits the tailor to extract twenty hours in exchange even though she has worked only ten.

However to see things in this manner, Ricardo's 'first' definition of value has to be treated not as an alternative equilibrium concept, but as an approach from which equilibrium is simply absent. The value of the coat is ten hours completely independent of whether it exchanges for ten, three, or twenty hours in the market. There is no implicit prior assumption that supply equals demand. Marx notes this very carefully

although price, being the exponent of the magnitude of a commodity's value, is the exponent of its exchange-ratio with money, it does not follow that the exponent of this exchange-ratio is necessarily the exponent of the magnitude of the commodity's value. Suppose two equal quantities of socially necessary labour are respectively represented by 1 quarter of wheat and £2 (approximately ½ ounce of gold). £2 is the expression in money of the magnitude of the value of the quarter of wheat, or its price. *If circumstances now allow this price to be raised to £3, or compel it to be reduced to £1, then although £1 and £3 may be too small or too large to give proper expression to the magnitude of the wheat's value, they are nevertheless prices of the wheat, for they are, in the first place, the form of its value, i.e. money, and, in the second place, the exponents of its exchange-ratio with money. If the conditions of production, or the productivity of labour, remain constant, the same amount of social labour-time must be expended on the reproduction of a quarter of wheat, both before and after the change in price. This situation is not dependent either on the will of the wheat producer or on that of the owners of the other commodities. The magnitude of the value of a commodity therefore expresses a necessary relation to social labour-time which is inherent in the process by which its value is created. With the transformation of the magnitude of value into the price this necessary relation appears as the exchange-ratio between a single commodity and the money commodity which exists outside it. This relation, however, may express both the magnitude of value of the commodity and the greater or lesser quantity of money for which it can be sold under the given circumstances. The possibility, therefore, of a quantitative incongruity between price and magnitude of value, i.e. the possibility that the price may diverge from the magnitude of value, is inherent in the price-form itself. this is not a defect but, on the contrary, it makes*

this form the adequate one for a mode of production whose laws can only assert themselves as blindly operating averages between constant irregularities. (Marx 1979:196, my emphasis)

As already noted, this counterposes two radically different treatments of the same issue: since value does not appear openly but in the form of price, and since value is unaffected by supply and demand whereas price itself fluctuates with their movement, what possible relation can there be between value and price? In the last words of this carefully-considered statement from volume I of *Capital* Marx repeats the statement which we have already cited, from his earliest work on political economy, and he in fact repeated and expanded it throughout his life.

The Ricardian answer, from which Marx demarcated himself for the rest of his life, is to enquire in what ratio goods must exchange in order to equalise demand to supply. Value then reduces to a special kind of price. Marx's answer, the polar opposite, is to ask what relations make exchange possible, regardless of supply and demand. Price is then explained as a form of appearance of value.

We thus see that the simple bifurcation between accident as primary or secondary, we arrive at a second bifurcation between value as the *persistent substance* of price and value as the *quantitative magnitude* of price.

The two conceptions stand opposed as the imaginary to the real. The Ricardian abstract law, supposing a relation that never happens, imposes ideal prices and values on reality. Marx's 'no less invariable law', insisting on a motion which always happens, imposes real prices and values on the imagination.

This distinction opens a violent separation between two potential world-views of economics. The 'Ricardian abstract law' presupposes that all factors driving supply apart from demand have been eliminated and may be set aside before we even begin thinking about what price consists of. That is, the market is *presupposed to have worked*. Price is thereby *conceived of* as what happens in a perfect market. This is not a calculating device; it is an ontology. It is a definition of price. Ricardo's second conception of profit, which is the foundation of almost all subsequent development in economics, is that as a price (the price of capital) *there can only be one rate of profit*. No sectoral variation is possible, and indeed, no variation between capitals within the same branch.

Even in order to sustain this idea in one's head, one must suppose that all possible reasons for profit rates to diverge have *already* been removed from the system, before we begin to think about what profit really is.

But the only way in which this could occur is if the market was functioning so perfectly that there was no longer any tendency for any profit rate to diverge from this single uniform rate, or any reason to suppose that it should. In short, the presupposition is that the market is *already perfect*. Deviations from this perfection can, therefore, no longer be explained by the workings of the market, because these workings were eliminated in order to deduce profit. It must, therefore, be a consequence of something exogenous to it.

Marx's point is that this constitutes a definition of reality which corresponds to no real situation. Like Plato's ideal heavens and ideal planets, it defines ideal prices and an ideal rate of profit which never in fact occurs.

If the circumstances never in fact occur which permit Ricardo to suggest how profit could be calculated, things are far worse than possessing no *measure* of profit; there is in fact no *definition* of profit – just as, if the earth is displaced from the centre of the universe, there is no *definition* of earth. Profit, following Ricardo's second definition is not just a quantity which exists but cannot be known. It is a quantity that does not exist.

## CAUSE AND EFFECT IN THE TWO WORLD VIEWS

However there is a further requirement. To return to our coat with its five hours cloth and its five hours labour, what determines the value in the cloth? Marx's answer, ignored by the economists as often as it is repeated by him, is the *past* labour in the cloth, that is, the time which, on average, society found necessary to produce it. The production of value is therefore a process in time, which adds the live present to the dead past. The very word 'dead' labour makes it clear what is involved.

Modern interpretations of Marx, beginning with Bortkiewicz, broke with this definition – for ideological reasons we shall make clear – and substituted an entirely different view, which is that the labour in the cloth is given not by the past but the future – by the labour which will be necessary to produce the cloth, once the coat is finished.

If, while production is going on, the labour needed to make the cloth falls, this idea gives rise to an absurdity, since it requires that the value of the cloth should be determined *after it is used*. This is resolved by the device which TSS researchers have designated 'simultaneism'. It is assumed that the value of the cloth and the coat are given by the values which *would* hold, *if* there were no change in either during the course of production. Technically, the value of inputs and the value of outputs are determined 'simultaneously'. It is, I hope, not difficult to see that simultaneous determination is simply the mathematical form in which the assumption of equilibrium is introduced. In order to carry out the calculation, it must be supposed that nothing changes. That is, it must be supposed that the economy contains no forces tending to change it – that it reproduces itself in an unchanging state.

This idea took economic thought along the second branch of Ricardo's bifurcation, heading off in an entirely different direction from Marx. If, says equilibrium economics, we can quantitatively determine prices by supposing that nothing changes – by presuming for the sake of calculation that the market works so perfectly that there is no variation in a single price, or a single proportion between used and produced goods, then we can calculate the magnitude of prices *without needing to worry* what they consist of. The 'metaphysics' of Marx's analysis is just so much irrelevant hot air. Since we can conduct the 'practical' business of predicting prices without it, let us dispense with it, and let us announce that bar a few minor details, *we can calculate prices by supposing that the market works*.

It is at this point that the problems begin. *Real*, observable prices are not equal to the ones we arrive at by this means. Nor is the real, observable rate of profit, the real, observable rate of interest, the real, observable rate of unemployment, or, in fact, *anything* measurable and quantifiable. The world as imagined in suburbia simply does not coincide with the actual world of the streets.

To this, equilibrium economics gives the following reply: yes, but the real world is 'close' to equilibrium. It is equal to equilibrium, plus a random disturbance. If  $x$  is reality and  $x^*$  is the ideal equilibrium then

$$x_t = x_t^* + \varepsilon$$

$x$  is equal to the value it *would have* if the market were perfect, plus a 'disturbance term'  $\varepsilon$ , the unfortunate distance between the ideal and the real. Indeed, this gives practical content to the activity of most economists, whose lives are spent happily quantifying and analysing  $\varepsilon$ , the difference between theory and reality, to whom it never occurs to wonder why there is any difference at all, or whether this might involve some revision of our ideas about  $x^*$  itself.

When it does worry about such matters, economics resorts to the metaphor of a pendulum, oscillating about a static position.<sup>17</sup> This is a very important example of a case, which exists in many physical systems, where because there is a regular or cyclic disturbance about a rest position, we can think of the movement as the rest position and a disturbance term.

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<sup>17</sup> See Francisco Louça's very thorough treatment of this idea in the work of Frisch and his contemporaries in Louça(1996)

But there is a vital difference. The pendulum can survive without moving, but the market cannot. Because it must move to exist, its existence can only be explained by its movement. *So rast ich, so rost ich*. A tornado in stasis is no longer a tornado. Like a tornado, an avalanche or indeed a living being, the market exists as a self-sustaining entity only in and through movement;

It is not just that markets fail: the point is that this is how they work. Only when supply differs from demand do the mechanisms which equate them come into play; moreover the very process of equalisation in one sphere disequilibrates all others, whereon eternally restless capital disrupts all balance as it scrambles for gains and stampedes from losses in its thirst for surplus profit, the life force of the market.

There is no economic pair of scales. A market is never in a state which permits exchange at equilibrium prices. From the very outset we have to follow Newton and Galileo, and study its motion with general concepts, the equivalent of mass and energy, which can express all possible exchange relations in terms of unifying concepts, so that we can compare them and say things about them which apply to all of them.

We need, in short, a way of talking about price which *does not depend* on the relation between supply and demand. We need to enquire qualitatively what the value of a commodity consists of, external to and independent of any subsequent exchange relations it enters into, *before and in order* to study the quantitative phenomenon of real market prices.

Gives rise to two different views of cause TBA

## Origins of the equilibrium ontology

We now come to the core of the matter. What does Economics actually assert? It does not, of course, simply assert that equation (1a) predicts the real behaviour of the system, that is, it does not predict that the market is perfect. It asserts

(1) that (1a) is a *valid abstraction*.

(2) that it is a *quantitatively close approximation*.

But, as we have seen, it never actually tests (2) because it never compares it with the most important potential alternative. *Actually*, therefore, economics rests on a philosophical proposition – (2) above.

In short, it rests on a method of abstraction. This in turn does not make it wrong. But since we know that it is not subject to empirical verification in a scientific manner, we must use a different instrument – we must critically assess the philosophical consequences of the abstraction method used. In this way we will find out *what types of conclusion it can, and cannot, generate*; that is, we will establish the limits of its possible validity. Of course, we must also subject the temporal, and the statistical dynamic, approach, to the same critical analysis.<sup>18</sup>

We now therefore consider the ontological properties of these two systems, temporal and equilibrium.

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<sup>18</sup> I am running so close to time in the production of this draft that I do not have space to amplify a critical point I wish to make, namely, the statistical dynamic approach is *compatible* with the temporal approach and *incompatible* with the equilibrium approach. In my view the necessary direction of progress is a critical synthesis of temporalism with statistical dynamics; in my view also, this is the entire direction of Marx's thought (considers all key variables as statistical averages over time and space, etc).

## Neoplatonism

(TBA expand this whole section?)

Equilibrium is not an innocent abstraction. A legitimate scientific universal is *part of reality*. When we speak of 'horses', we employ an abstraction to think about concrete horses. But the properties of this abstraction are *true* of actual horses, with actual proud hooves. If someone falsely claims that horses really have horns, we can eke out this imperfection by noting that no-one has ever seen a horned horse. If someone insists that horses are in fact imperfect unicorns, then they take a step outside science, because no real unicorn exists.<sup>19</sup> There are of course imperfect horses, which perhaps do not trot or do not neigh. But this is an accidental departure from an observed universal. *Most* horses trot. *Most* horses neigh. A dumb crippled horse is a departure from an observed reality. We enter an entirely different way of conceiving reality if we insist that *all* actual horses are imperfect representations of a reality that *no* actual horse inhabits.

Yet such an idea is exactly the method of modern, equilibrium, economics. A perfect market has never been seen. Yet all *actual* capitalism is explained as an 'imperfect market' – as a deviation from a magical entity which does not and cannot exist. This method of abstraction dictates that *if* reality diverges from economic theory, this *must* be because reality is imperfect. In philosophical terms, it is laboratory-pure neo-Platonism. It presupposes that reality itself is inherently flawed, and only in thought do we find perfection. Therefore reality itself is a distorted emanation of theory.<sup>20</sup> Thus economics is riddled with concepts such as 'imperfect competition', 'natural rate of unemployment' or 'market failure' whose function is to explain reality as a deviation from a theoretical construct which is never actually seen and indeed, cannot actually exist. The real world is reduced to a fall from capitalist grace.

### WHAT EXISTS? TWO WORLD-VIEWS IN ECONOMIC THOUGHT

The relation between accident and permanence is one expression of a philosophical issue which has been debated since the time of Heraclitus's assertion that 'no-one may stand in the same river twice'. Once it is admitted that all things change, and that nothing is eternal, the question 'what exists' is posed in an entirely different way. The basic problem is that of *persistence through change*. Although 'I', a person, may live from 0 to 100, from any given moment to the next, I am in constant flux, down to the very atoms which compose me which are continually being shed, eliminated, and replaced by new atoms that are still part of the same essential being, myself. Therefore the question 'what exists?' from the outset requires abstraction: it requires that define any object, and any social relation, by asking *what remains unchanged*

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<sup>19</sup> We are indebted to John Weeks for this analogy.

<sup>20</sup> It may be thought this is an abusive exaggeration. Recall what Plato writes: "These intricate traceries in the sky are, no doubt, the loveliest and most perfect of material things, but still part of the visible world, and therefore they fall far short of the true realities – the real relative velocities, in the world of pure number and all perfect geometrical figures, of the movements which carry round the bodies involved in them. These, you will agree, can be conceived by reason and thought, not seen by the eye ...Accordingly, we must use the embroidered heaven as a model to illustrate or study these realities" (*Republic* 7:529A) Here is what Walras writes: "A truth long ago demonstrated by the Platonic philosophy is that science does not study corporeal entities but universals of which these entities are manifestations. Corporeal entities come and go, but universals remain for ever. Universals, their relations, and their laws, are the object of all scientific study."

despite change? or, which amounts to the same thing, what is preserved when an object changes?

Marx's insistence on the qualitative aspect of price, as opposed to its quantitative aspect, is the outcome of this radically different approach. What matters about the price of a bushel of apples is not what causes it to be exchanged for one coat or two, but what the coat and the apple have in common which makes the exchange possible at all.

There is, of course, room for ample discussion about whether or not Marx was right to argue that 'labour' – as opposed to, say, money or the natural prolixity of the earth – are the 'true' content of price, the true substance of value. The point is, however, that once one departs the comfortable world of equilibrium we cannot avoid asking what value 'consists of', what is its 'substance'.

Marx's well-known ontological starting point – value is a *social relation* – has exactly this character. The point, for Marx, is to provide the link between the behaviour of the market as such, and the more general behaviour – whether inside or outside the market – of the humans that make it up. The 'substance' of the market is – society. Its magnitude is quantified social effort – labour. The decisive point about this is that it provides a theory not just of market perfection but of market breakdown. It enables us to analyse what is really going on when the market is *not* reproducing itself perfectly, which does not end with minor fluctuations but includes crisis, permanently growing polarisation, Kondratieff waves – in short all the 'big' phenomena of the market that arise not from its perfection but from its movement.

Marx's concern to reduce incommensurate magnitudes to a common denominator is often portrayed as metaphysical. And indeed, if supply did equal demand, the qualitative aspect could be safely ignored. If scales balance, does it matter why? The measurement itself connects the measured objects: what they have in common is 'being weighed'. We can build scales without a concept of mass, which is why scales existed long before Newton.

The problem is what happens when they don't balance. With any piece of machinery, you only need the manual when it breaks. If two *unequal* weights are placed in a pair of scales, awkward questions arise like: how fast does the pan move? What force does the imbalance exert? What impact does the motion have on the requirements of balance in any case? We then have to connect what is sitting in the scalepans, which until now balanced unquestioningly in the self-contained suburb of stability, to the raw chaotic urbanity of nature. Only when Galileo, Newton and their followers asked such 'metaphysical' questions as 'how fast to objects fall' or 'how does the moon get from where it was yesterday to where it will be tomorrow?' could physics formulate general laws of nature with the general concepts of mass, energy and force.

## APPENDIX: A MATHEMATICAL DIGRESSION

In this article, we have explained the equilibrium-temporal distinction in qualitative terms without using mathematics and this explanation is embedded in the present text; however we also have to do three necessary things. The first is to address an audience of natural scientists. The second is to give a very precise definition to these qualitative descriptions. It would be foolish to miss the opportunity of an axiomatic statement of the questions at issue. The third, and most important, is to give a formal proof of the main statements made in this text, in particular the assertion that the temporal and the equilibrium paradigm give, in every case where there is secular change in the exogenous variables, quantitatively different answers and hence offer to economics the opportunity of a Popperian test which, in fact, it does not take.

There follows, therefore, a short mathematical summary which non-mathematicians may ignore, except the conclusions. As to the validity of the conclusions, they will, I am afraid, have to bend (as will I) to the judgement of the mathematicians.

Let us suppose that some general dynamic system contains variables of two types: exogenous and endogenous.

The endogenous variables are all those that the economist considers intrinsic to the market – those which define what a market consists of and which are to be treated as determined by its state of being or its motion. This consists of magnitudes like prices, quantities produced, labour inputs, profits, the interest rate, wages, and so on. Let the state vector of all these variables at time  $t$  be  $x_t$ .<sup>21</sup>

The exogenous variables are all the rest – those the economist leaves out when considering what the market actually is, which are not intrinsic to its definition. These might be (in the Sraffian framework) physical quantities of inputs and outputs or (in the marginalist framework) they might be consumer preferences and production functions. More generally they can include just about anything,<sup>22</sup> which accounts for the very large market within economics for vacuous theoretical innovation. Let the state vector of all these variables at time  $t$  be  $a_t$ .

Now let us write down a general dynamic equation for the system:

$$x_t = f(a_t; x_{t-1}) \tag{1}$$

The function  $f$  constitutes the economists' theory: that is, it tells us in what state the economy will be at any given time, as a function of the exogenous variables, and what it was in the past. This is, we should note, perfectly determinate for a wide variety of functions  $f$ , given an initial condition at some time  $t=0$ .

Such a system provides a *temporal* determination of the endogenous variables  $x_t$ .

Now consider how the economist proceeds. S/he supposes that the *market works*. In consequence, it may be supposed that  $x_t$  can have no motion that is proper to it.  $x_t$  may differ from  $x_{t-1}$  because the exogenous circumstances have changed – consumers have got more picky about GM foods, wages may have gone up because of the trade unions, oil prices because of OPEC, technological progress may increase output, and so on. In that case, the market would of course adjust to changed circumstances and a new set of  $x_t$  would emerge. While it is changing, there will moreover be 'dis-' equilibrium.<sup>23</sup> Supply will not equal demand. But we are not interested in this, because it is not the 'true' reality. The 'true' reality is what we observe when all such movement has ceased or, to be precise, what we would observe were all such movement to cease.

We can therefore rewrite the equation on the assumption that  $x_t$  has ceased to change: that is,

$$x_t = x_{t-1}$$

This gives us a *fixed-point* equation

$$x_t^* = f(a_t; x_t^*) \tag{1a}$$

<sup>21</sup>  $x_t$  may contain differences or derivatives of its other components eg  $x_t = \{p_t, p_{t-1}\}$  or  $\{p_t', p_{t-1}'\}$ . This convention makes it easier to express dynamic relationships of order greater than 1.

<sup>22</sup> Prominent candidates include: the weather, education, sunspots, historical backwardness, the pleasure of leisure, differential gender preferences for childcare, entrepreneurial spirit, risk preference,

<sup>23</sup> In our own writings, TSSI authors systematically use the term 'non-equilibrium' in preference to 'disequilibrium'. The term 'disequilibrium' implies that there is an equilibrium to be dissolved, which is like defining the natural sciences as Godless knowledge. Like, excuse me, who said God was invited?

A very general theorem in mathematical topology tells us that for almost any  $f$ ,  $a$  we care to choose, a solution  $x_i^*$  exists to this equation.<sup>24</sup> Thus it solves Ricardo's 'quantitative' problem – it allows us to calculate the variables.

This formal system of equations subsumes within it pretty well all the mathematically formulated economic problems of which I am aware. To be precise, it encompasses all those which make claims to *predict* or *model* the actual course of development of the economy.

Thus for example in the Keynesian determination of macroeconomic equilibrium we have something like

$$x = \{\text{consumption, investment, employment, output}\}$$

$$a = \{\text{marginal propensity to consume, liquidity preference, government spending}\}$$

In the most simple presentations of the determination of value we have

$$x = \{\text{unit values}\}$$

$$a = \{\text{labour inputs, consumed use-values, produced use-values}\}$$

In the Sraffian determination of prices we have

$$x = \{\text{unit prices, the uniform profit rate}\}$$

$$a = \{\text{technological coefficients, the wage}\}$$

and so on.

The economist may 'endogenise' variables by moving them from  $a$  to  $x$ . Thus we may assert that government spending is in fact determined by the market and construct an equation showing how it is related to savings, exports, and so on (or even, like the Chancellor, construct a 'Golden Rule' that is explicitly built into the government's reactions). Or we may, in the Sraffian system, make the profit rate exogenous and the wage endogenous, or some combination in between. This doesn't affect this part of the analysis since the *same* move from  $x$  to  $a$  will be made both in the temporal system (1) and the equilibrium system (1a). The point is this: the behaviour of (1) and (1a) is *not necessarily the same*.

## Pendulums, centres of gravity, and averages

There are a very definite set of circumstances in which (1) and (1a) are, whilst not identical, *equal on average*. This occurs, in general, if there is no *secular* movement of the exogenous variables. This occurs, to fix ideas, with a pendulum held in a fixed position, or for example with a windblown lake. Although the pendulum only passes its fixed point (the position at which it comes to rest) twice in each cycle, and although a choppy lake is never quite flat, nevertheless on average the pendulum's position is the same as its rest position and the lake's average level at every point is, over time, what it would be if the lake were perfectly flat.

This has led to a defence of the equilibrium view based on the idea that it is a centre of gravity.

(etc. expand)

What conclusions follow?

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<sup>24</sup> The theorem is colloquially known as the furry ball theorem, because it proves that you cannot comb a ball covered in fur without leaving at least one hair sticking up. There may be more than one fixed point (many hairs may stand up) and this is the source of a lot of controversy in economics (sunspot equilibria, etc). **(TBA stable and unstable)**

- (1) the temporal and equilibrium determination of quantitative results are *not the same*; they give rise to *different quantitative predictions* and hence to testable hypotheses, in the Popperian sense.
- (2) Nevertheless, they give rise to two sets of *meanings* for all those variables which are endogenous to the system.  $x$  and  $x^*$  are not merely different numbers, they provide *different ways of thinking* about the objects to which they refer
- (3) In particular the two systems give rise necessarily to a different account of *causality* (TBA expand)

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