Scientism in the Way of Science: Hope for Heterodoxy in Modern Economics[*]

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Abstract

This paper argues that the long-standing predominance of a particular approach to science neither makes it uniquely "scientific" nor superior to rival approaches. In particular we argue this in the context of current economic orthodoxy. We first examine the dominant scientific explanation of the 17th century: "the mechanical philosophy." The constraints this approach imposed on science were eventually abandoned, but only after having stifled progress in several areas. We show how in several important respects, today's mainstream, neoclassical approach to economics is analogous to the mechanical philosophy. Historical precedent suggests that however secure the mainstream monopoly on "economic science" may appear at present, its continued dominance should not be taken as a given. Our analysis demonstrates the fragility of even the most entrenched scientific wisdoms and provides hope for heterodox economists everywhere. (*JEL* B0, B5)

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> "A school of thought is to be viewed as a single individual who talks to himself for a hundred years and is quite extraordinarily pleased with himself, however silly he may be."
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> – Johann Wolfgang von Goethe (1998: 14)

1. Introduction

Robert Oppenheimer once famously declared, "There is no place for dogma in science."[1] We agree with his sentiment that there *should* be no place for dogma in science, but historically dogma has played a prominent role in scientific theory. For instance, in the mid-19th century, believing that the North Pole was covered with thick ice was considered an "unscientific" understanding of geology. The popular scientific explanation of the time, called the "Open Polar Sea" theory, maintained that a large, tranquil, ice-free ocean surrounded the North Pole. In the same period the "miasma theory" of disease, which held that illnesses such as cholera were caused by exposure to "miasma" or "foul airs," prevailed in biological science. Yet today there is essentially universal agreement among scientists that the North Pole is covered with ice and that germs—not nasty smelling air—are responsible for outbreaks of cholera.

Explanations of worldly phenomena come and go, but acceptance of any explanation claiming to be "scientific" is constrained by the widespread belief that it should fit smoothly into the worldview prevailing in science at the time it is proposed. Taking a position at odds with current, conventional wisdom of the scientific community risks having one's ideas dismissed as unscientific and, therefore, unworthy of serious consideration. As Michael Polanyi (1962) has pointed out, for any kind of discourse to advance, there must be some generally acknowledged

[1] Quote from *Life Magazine* (1949).

boundaries about what constitutes acceptable argument. But if these boundaries become too narrow or rigid we risk straightjacketing the discussion or, worse yet, rejecting superior explanations because they pose too great a challenge to existing ones.

This paper demonstrates both the tendency towards dogmatism of a scientific orthodoxy and the possibility that it may direct scientists along paths leading to barren or even counterproductive territory. To do this we sketch the outlines of the dominant school of 17th century science, called "the mechanical philosophy." Its adherents maintained that the only valid components of a genuinely scientific explanation were the extension, place and motion of pieces of matter. They rejected the notion of physical forces (e.g., gravity) as "occult" explanations that were intrinsically unscientific, and they were obsessed with model building as the hallmark of genuine science. When faced with anomalies that seemed to fall outside the scope of explanation they deemed acceptable, they adjusted their system in such a way that it could fit any observed phenomena. Despite the many decades during which the mechanical philosophy held a near monopoly on the appellation "scientific," it was ultimately dethroned.[2] During its reign, however, it led scientific inquiry down several blind alleyways.

We contend that modern, mainstream economics is in a situation analogous to that of physical science some four centuries ago. The neoclassical approach has managed to accrue near-exclusive claim to "scientific" inquiry in the discipline and is reminiscent of the mechanical philosophy in several important respects. One

[2] We recognize that various episodes in more recent scientific history, such as Einstein explaining gravity as a curvature in space instead of a force acting at a distance, could reasonably be viewed as a revival of the type of theory favored by the mechanical philosophy. But we are not making any metaphysical claims about the ultimate status of mechanical explanations; our contention is historical: the mechanical philosophy did not provide the framework needed to make progress in a number of fields, and in fact stifled progress in several fields, within the context of its time.

important similarity is neoclassical economics' rejection of causal factors that cannot be incorporated into its models, regardless of how important they may seem for explaining real-world economies. Also like the mechanical philosophy, in the face of an accumulation of empirical facts at odds with its predictions, neoclassical economists have devised *ad hoc* modifications to their framework, effectively enabling it to "explain" any observed behavior whatsoever. Finally, neoclassical scientism in economics has had deleterious effects on the progress of economic study similar to those generated by the orthodoxy of the mechanical philosophy in the physical sciences. The history of 17th century science suggests that what now seems like an impenetrable monopoly of science in economics could come crumbling down. But like with the mechanical philosophy, in economics too, it will not be before some serious damage has been done to the progress of economic study.

The remainder of this paper is organized as follows: Section 2 traces the development of 17th century scientific theory and the mechanical philosophy. Section 3 examines modern economics' parallel situation and Section 4 explores some of the damage inflicted by neoclassical orthodoxy. In Section 5 we conclude with some parting remarks that should hearten heterodox economists of every stripe, and others, such as historians of economic thought, who find their presence and significance, in the eyes of the rest of the profession, ever diminishing.

2. 17th Century Scientism and the Mechanical Philosophy

The historical episode we discuss in this Section is generally unknown to those only familiar with the history of science as it is presented in popular works or in scientific textbooks. It is ignored because it does not advance the narrative these accounts usually hope to convey. Murray Rothbard (1995) called this common tale the "Whig

history of science," but it also might be termed the Voltarian or Enlightenment history of science. It was inspired by the triumphant success of Newtonian mechanics in the 18th century and advanced by intellectuals eager to promote the new natural philosophy.

According to the story they devised, after the decline of ancient Greek civilization and the rise of Christianity, the idea of understanding the world scientifically sunk from view. During "The Middle Ages" superstition was substituted for science,[3] and only with the work of Copernicus in the 16th century did this process finally begin to reverse. At the turn of the 17th century further scientific progress was made by Galileo and Kepler, culminating several decades later with the contributions of Newton. According to this account, once science had been released from its stranglehold by religion, it advanced along a straight and narrow course.

The chief barrier to universal acceptance of this myth was that its tellers had to excise large parts of the actual history of science to make their story plausible. One of these significant omissions was the dominance of the mechanical philosophy, which monopolized scientific theory for most of the 17th century. For our purposes it is important to understand that the mechanical philosophy, whose adherents included Descartes, Gassendi, Boyle, Hobbes, and many other major thinkers, was put forward as a rational advance over earlier schools of natural philosophy, primarily because it sought to banish all "occult explanations" from acceptable scientific discourse, such as forces acting at a distance or the attribution of any active qualities to matter.

Although the philosophy took on somewhat different forms in the writings of its various proponents, it can be summarized, without too much distortion, as being based on the "unfounded ontological reduction of the number of primary qualities to

[3] See Grant (1996) against this view.

two: 'matter and motion'" (Kuhn 1952: 18), where matter, as noted above, is forbidden from having any active principles. As Descartes, who was perhaps the foremost promoter of mechanism, summarized it: "I considered in general all the clear and distinct notions which our understanding can contain with regard to material things. And I found no others except for the notions we have of shapes, sizes and motions . . ." (quoted in Sargent 1995: 32).

The mechanical philosophers had a prominent obsession with creating models that could account for any phenomenon observed in nature purely on the basis of the size, shape, and motion of particles of matter. Little attention was paid to determining whether or not the mechanisms posited by the models really existed; the important thing was to *have a model*: "A generic kind of corpuscularism, making use of ad hoc postulated particles invented at the whim of the natural philosopher, appears in many texts dating from the middle of the seventeenth century onwards" (Dear 2001: 100). For instance, the most obvious explanation of magnetism is as an attractive force. But such occult entities were anathema to the mechanists. In order to demonstrate that his philosophy could handle such phenomena, Descartes devised a mechanical model of magnetic attraction in which the Earth and other magnetic bodies emitted streams of little screw-shaped particles, which, when they passed through the pores of any iron object, drew that object towards the magnet. Descartes' model is depicted below.[4]

[4] From the cover of Westfall (1977).

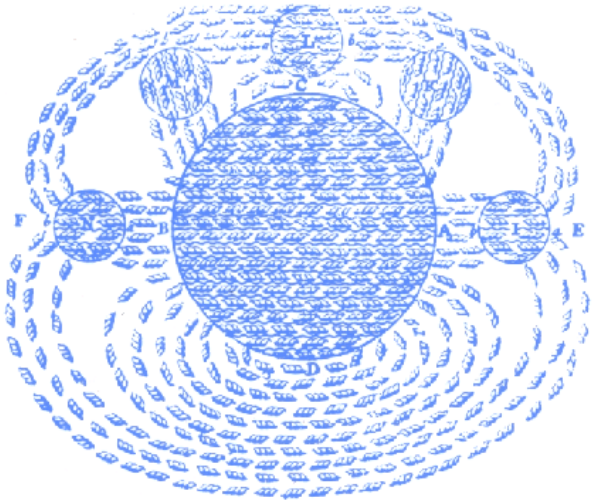

Figure 1. Descartes' Mechanical Model of Magnetic Attraction

Along the same lines, "In *De Corpore* [Thomas] Hobbes had presented a mechanical explanation of the production of cold and ice, both of which he attributed to a 'constant wind' that pressed upon bodies." A liquid freezes when this wind "raises the parts of it in such a way that the uppermost parts become pressed together and thus 'coagulated'" (Sargent 1995: 202-203). Similarly, Robert Boyle and Robert Hooke explained the relationship they had discovered between the volume of a certain amount of air and the pressure to which it was subjected by "the supposition that air consists of particles like little coiled springs, like wool, which 'consists of many slender and flexible hairs; each of which may indeed, like a little spring, be easily bent and rolled up, but will also, like a spring, be still endeavouring to stretch itself out again'" (Pyle 1995: 476).

Such models seem absurd to us today. But at the time they were put forward, they were generally viewed as the cutting edge of science, replacing the primitive and unscientific explanations of natural phenomena that had been offered by Aristotle and his disciples. Even in physics, where the mechanical philosophy was most successful,

dogmatic adherence to its precepts crippled research in certain areas. For example, "By the end of the 17th century, the mechanical philosophy, which encouraged optics early in the century, and which furnished the idiom in which all students of optics . . . discussed the science, had become an obstacle to further progress . . . [O]ptics stagnated for a century" (Westfall 1977: 64).

In other sciences the effect of its supremacy was even more disastrous. Westfall describes "the story of chemistry in the second half of the [17th] century [as] the story . . . of its subjection to the mechanical philosophy, since the growing role of mechanisms in chemical literature appears less to have sprung from the phenomena than to have been imposed on them by external considerations" (1977: 69).

"The mechanical philosophy did not in itself offer a chemical theory. On the contrary, it was potentially adaptable to almost any theory" (Westfall 1977: 71). Rather than searching for the fundamental causal factors underlying the multiplicity of chemical phenomena, the focus of the mechanical philosophers was on devising some model, any model, which appeared to explain each particular phenomenon with which they were presented using only mechanical elements. Westfall comments, "Like his fellow mechanical chemists, [Lemery, the leading French chemist of the 17th century,] seemed possessed by a mania to explain every property and every phenomenon" (1977: 73). He contends, "In no area of science was the tendency to imagine invisible mechanisms carried to such extremes" as it was in chemistry (1977: 81).

For example, Lemery's theory of why acids dissolved metals suggested that the particles making up acids had little dagger-like points, which skewered the smooth particles composing metals and then carried them away from their comrades. Metals could be precipitated back out of a solution by adding another substance whose

particles moved in such an agitated fashion that they would break off the points of the particles of acid, thus setting the particles of metal free.

The most prominent, and arguably best, chemist of the 17th century was Robert Boyle. However, "the development of a satisfactory chemical theory as such was not Boyle's goal. Chemistry represented to him a means to demonstrate the validity of the mechanical philosophy of nature" (Westfall 1977: 77). Indeed, "his mechanical philosophy appears to have operated to thwart the most promising aspect of his chemistry" (Westfall 1977: 79). As Thomas S. Kuhn puts it, "the form of atomism developed by philosophers and applied to physics in the seventeenth century embraced concepts inconsistent with the development of such fundamental chemical notions as element and compound. These impediments to chemistry are manifest in the chemical theory of . . . Robert Boyle" (1952: 13). Westfall sums up the situation thus: "[S]ince there were no criteria by which to judge the superiority of one imagined mechanism over another, the mechanical philosophy itself dissolved into as many versions as there were chemists . . . It is difficult to see that the mechanical philosophy contributed anything to the progress of chemistry as a science" (1977: 81).

The reign of the mechanical philosophy had a similar effect on the advancement of biology. One of the most significant biological discoveries of the era, that the heart is a pump serving to circulate blood throughout the body, was made not by a mechanical philosopher, but by an animist, William Harvey. Descartes, instead, explained the heart as a heat engine that expanded the blood, forcing it out through the circulatory system.

The mechanists, when confronted with the discovery of mammalian eggs by investigators employing the microscope, and unable to accept the existence of anything such as a "formative virtue" that could transform some simple substance into

a complex body, responded with the theory that a fully formed animal was contained inside every egg. Of course, if the body was a female, she would already have more eggs inside of her, containing her fully formed children, each of whom, if female, would bear the eggs for her children, and so on. As a consequence, it seemed probable "that the entire human race was present already in Eve" (Westfall 1977: 100). The idea that an embryo developed in stages of successive differentiation could only be defended by "occult organizing forces . . . but the mid-eighteenth century, with its strong preference for the mechanical ideas set forth by Descartes . . . was not the ideal time to propose intangible driving impulses" (Pinto-Correia 1997: 171).

Such theories were not the result of any serious contemplation of biological phenomena by mechanical philosophers. Before addressing biology they had already decided that *only* mechanical explanations could qualify as scientific, and so they tried to force living processes into their pre-formed moulds. "[Mechanistic biology] did not arise from the demands of biological study; it was far more the puppet regime set up by the mechanical philosophy's invasion" (Westfall 1977: 104).

The mechanical philosophy lost its hold on the scientific imagination during the 18th century, due both to the unsatisfactory nature of many of the explanations it offered, and to the considerable success of Newton's distinctly non-mechanical theory of gravity. It is worth noting that in many cases science advanced by going "backwards" to concepts that had been rejected by the mechanical philosophers as "unscientific."

Newton's theory of gravity, for instance, maintained that one material body was somehow able to influence another without any physical contact between the two. When his theory was published it was widely derided by Cartesians as "manifest stupidity" (Pinto-Correia 1997: 171), a throwback to the positing of "occult forces"

characteristic of the superstitious views of Renaissance naturalism. Similarly, Newton attributed his mathematical advances to revisiting the works of the ancient Greek geometers, and dismissed the recently developed Cartesian geometry as "the Analysis of the Bunglers in Mathematicks" (Westfall 1980: 379-380).

Furthermore, some "unorthodox" theories developed during the reign of mechanical philosophy that were long rejected by its practitioners as hopelessly flawed, eventually underwent resuscitation. Again turning to Newton for an example, his idea that all material bodies are composed of only a very few elementary particles, and that what appear to be chemical elements are really compounds of those building blocks, and, therefore, could be transformed into each other, was seen as an unsightly blemish on his great career for two centuries. However, as Pyle notes:

> "This criticism seems unfair and unwarranted. In the first place, the Newtonian matter-theory is remarkably close to what we no believe to be the truth. Chemical species do only arise at a 'molecular' level, i.e. as a result of the aggregation of simpler (and chemical neutral) constituents. The chemical atom of Dalton is a highly complex structure, made up of neutrons, protons, electrons, etc., held together by powerful interparticulate forces of various kinds. The transmutation of the so-called 'chemical elements' is physically possible although, as Newton foresaw, highly difficult owing to the strength of those forces" (1995: 433).

3. 21st Century Scientism and Neoclassical Economics

Although the mechanical philosophy is long dead and buried, our age is not without its own dogma regarding properly scientific explanations, and economics is no

exception.[5] For Adam Smith and his contemporaries, the study of economics was fundamentally rooted in analyzing the behavior of *acting* individuals. The actions of man in their fallibility, creativity and uncertainty occupied the center of economic analysis. Partly this was a result of the fact that Smith and the other Scottish moral philosophers considered questions of morality inextricably linked to questions about exchange activities. But it was just as much the result of the fact that in their vision, the purpose of economics was to understand real-world, purposive, and thinking individuals.

Many features of "real-world man" simply cannot be captured by formal, deterministic modeling. For instance, the notion of uncovering previously unrecognized profit opportunities by imagining new ways to employ existing factors of production, which Israel Kirzner termed "entrepreneurial discovery" roughly two centuries after Smith, defies mathematical modeling. "Pure discovery" is just that; probabilities cannot be assigned to it, nor is the essence of "entrepreneurial alertness"—the product of a special kind of human creativity—something that can be meaningfully expressed in mathematical formulae. The constantly changing, unknowable future, or what Ludwig Lachmann called "radical uncertainty," falls into the same category mathematically intractable components of the economic world. These omnipresent features of economic reality do not yield testable implications and represent causal "forces" not susceptible to measurement. But this does not diminish their importance for economics. Indeed, for Smith and many of his contemporaries,

[5] Mises (1949, 1957) and Hayek (1948, 1952) were among the first to forcefully make this argument with respect to mainstream, neoclassical economics. Mises, for instance, long insisted on the importance of methodological dualism, which prescribed different methods of investigation for the natural sciences and the social sciences owing to the different subject matters involved and the differing position of the scientist in each with respect to the object of study (see Leeson and Boettke 2005). Hayek similarly argued against what he called "scientism" in economics—the uncritical importation of the natural sciences into the social sciences where they do not belong (see Caldwell 2003).

these elements of reality were the indispensable concepts required to understand the economic world.

The classical economists did not have at their disposal the tools of modeling and measurement that today constitute the hallmarks of mainstream economics. But it is unlikely they would have chosen these methods even if they had been familiar with them. Their verbal style of reasoning was better suited to capture the nuances and complexity of the "political economy of every day life" than the tools of modern economics. Smith and the economists who followed him were interested in exchange *processes*, in how *institutions* shape these processes, and in the consequences, intended and unintended, of the human interactions that generate exchange and its institutions.

Though they differed strongly with respect to the issue of the universality of economic insights, the historicists of the 19th century, such as Werner Sombart and Gustav Schmoller, and later the Old Institutionalists, were fundamentally interested in the economy of acting man as well. They, too, reasoned verbally, because that approach was the best way to address the issues of cultural complexity and the evolution of historically unique institutions.[6]

It was not until well into the 20th century, with the growth of modern neoclassical theory, that acting man was effectively purged from economic analysis and the "machine economy," amenable to formal modeling and statistical testing, took his place.[7] The following quotation from Donald Patinkin captures this tendency nicely. As he put it: "we can consider the individual—with his given indifference map and initial endowment—to be a utility computer into which we 'feed' a sequence of

[6] The trouble of formally capturing institutions is well-understood by most neoclassical economists. This, it seems, is partly the reason why they ignored them for so long. As Arrow put it, "I am not going to attempt a formal definition of an organization, which would probably by impossible" (1984: 176).
[7] For more on this development in the history of economic thought see Boettke, Coyne, and Leeson (2003).

market prices and from whom we obtain a corresponding sequence of 'solutions' in the form of specified optimum positions" (1965: 7). The introduction of new methods of modeling and measuring made it easier for more economists to use the kinds of tools that had been employed with such success in physics and chemistry. The relaxation of this technical constraint, in tandem with the prevalent, positivist idea that "real" science is based on empirically verifiable, functional relations between measurable quantities, increasingly narrowed the field of respectable economics.

In the years immediately following WWII, the "neoclassical consensus . . . coalesced around the doctrine that one must start with Walras and Slutzky, and no where else, to become an orthodox 'mathematical economist' in good standing . . ." (Mirowski 2002: 199). Indeed, mathematical analysis increasingly became necessary to be viewed as an economist in "good standing" at all—i.e., for one's work to be considered "scientific." This trend only increased with the passage of time. Writing in 1971, for example, Wassily Leontieff, himself a prominent mathematical economist, bemoaned this unfortunate state of affairs when he referred to the "nearly mandatory use by modern economic theorists of mathematics" (1971: 1).

The crowning achievements of the first phase of the neoclassical hegemony were the proofs of Arrow, Hahn and Debreu, which solved complex systems of simultaneous equations so as to demonstrate a unique, general equilibrium for the entire economy. Their results led to the "discovery" of the first and second welfare theorems, enabling economists like Paul Samuelson and Abram Bergson to develop the notion of a social welfare function, which in turn made "scientific" welfare economics possible.[8]

[8] An interesting irony of this "scientific welfare economics" was that it utterly ignored the impossibility of aggregating individual preferences into a coherent social preference (i.e., one that satisfies the same criteria of rationality as individual preferences), as mathematically proven by Arrow himself (1951).

The main body of neoclassical theory consisted of comparisons of static equilibria. But the approach did not, and indeed could not, analyze the real-world processes that move markets towards equilibrium in the first place. In the Walrasian world, for example, plans are pre-reconciled, and perfect coordination is ensured before exchange is allowed to take place. Timeless, deterministic neoclassical models must assume away human creativity, genuine uncertainty and unexpected change, which are the most important aspects of time for human actors; in other words, they have no room for acting man.

The result was a flurry of mathematics with virtually no attendant connection to economic reality. As a former president of the Econometric Society put it: "it cannot be denied that there is something scandalous in the spectacle of so many people refining the analysis of economic states which they give no reason to suppose will ever, or have ever, come about . . . It is an unsatisfactory and slightly dishonest state of affairs" (quoted in Leontief 1971: 3).

But what the neoclassical approach lacked in "honesty" and realism, it made up for with technical sophistication, which hid its dearth of economic substance with impressive notation. As Leontief noted, neoclassicism's "uncritical enthusiasm for mathematical formulation tends often to conceal the ephemeral substantive content of the argument behind the formidable font of algebraic signs" (1971: 1-2). Mainstream economics may have purged the elements that Smith deemed most essential to economic study, but it did generate mathematical models, yield specific predictions and offer empirically testable hypotheses, thus creating the appearance that economics was a "genuine" science like physics or chemistry. Questions concerning whether or not it was in fact desirable to proceed in economics as in the natural sciences were of

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On this issue and its implications for socialist planning, discussed below, see Boettke and Leeson (2002).

course never seriously entertained. Neoclassical orthodoxy had already established that it unquestionably was and never looked back. As Robert Solow indicated, “If the project of turning economics into a hard science could succeed, then it would surely be worth doing” (1985: 331).

This obsession—that economics should achieve a status on par with physics—led to a situation in which what was valued and assessed highly within the profession was modeling and measuring, *per se*, rather than insight, applicability to reality, and increased understanding of the actual economic world. As Leontief complained, “preoccupation with imaginary, hypothetical, rather than with reality has gradually led to a distortion of the informal valuation scale used in our academic community to assess and to rank the scientific performance of its members” (1971: 3). Specifically, those who use formal models and sophisticated econometric testing are assessed as intellectually high-powered and scientific, and those who do not are seen as less capable, unscientific minds. As Solow put it: “My impression is that the best and brightest of the profession proceed as if economics is the physics of society” (1985: 330). The dimmer lights, it is supposed, try their best and make due with mere words.

Ironically, it was partly empirical testing—the handmaiden of technique-driven economics—that made it increasingly difficult to ignore the shortcomings of the neoclassical approach. Over time, empirical anomalies mounted up to a rather substantial body of evidence undermining its models. The failure of neoclassical convergence was one case in point. It was followed by the unexpected results of experimental and behavioral economics, and then the failures of developmental aid programs in formerly socialist countries predicated on “getting the prices right,” as suggested by the general equilibrium model. Even in its quite early stages—on issues as fundamental as demand functions—the neoclassical framework failed empirically

(Mirowski 2002: 193-195). But this did not stop mainstream economists like Paul Samuelson from "spen[ding] countless hours pursuing testable regularities that aggregate demand data must theoretically obey" (Samuelson 1986: 223). In short, neoclassical models, which generally predicted very specific outcomes, were often empirically off the mark.[9]

However, instead of questioning their use of deterministic models, neoclassical economists made *ad hoc* adjustments to their theories to bring them in line with the deviant empirical results. The explanation for this move is straightforward. For most neoclassical economists, abandoning the dominant methodology would have entailed admitting that economics is not a "hard" science like physics, with a consequent loss of prestige. It would have been tantamount in their eyes to accepting economics as a "mere social study," as it was in the era of the "primitive," descriptive political economy of Smith, Say, and Cantillon. In fact, for some, it would have meant abandoning the very objective of economic study, which was not so much illuminating the world, as economics achieving the scientific status of physics. As Irving Fisher, for instance, declared: "the goal on which my heart has been most set" was "the goal of economics becoming a true science comparable with physics" (quoted in Mirowski 1989: 232).

[9] A neoclassical economist might retort that empirical failure is superior to no empirical examinations at all, a frequent charge against heterodox economists, such as the Austrians. The point, however, is that heterodox economists eschew certain types of empirical work because (a) they pretend to be able to do more than they can (e.g., the point predictions of econometric analyses) and (b) they ignore the most important components of the economic world. Heterodox economics does not reject empirical work, so much as it seeks to circumscribe the sphere of empirical claims that economists can realistically make. The Austrians, for example, do not deny prediction. They deny point prediction in human action owing to the absence of constant relations in human affairs and the complexity of social phenomena (which prohibits the ability to perform controlled experiments, as in a lab). But they embrace pattern predictions of general tendency (see, for instance, Mises 1949; Hayek 1948). Similarly, they do not reject empirical work, but instead qualify the kind of empirical work that can yield insight into the essence of human behavior. Data crunching may not be able to answer certain questions about, for instance, institutional embeddedness, but case studies can. On the issue of Austrian empirical work see Leeson and Boettke (2005).

Rather than accept a reduction of their discipline's status, neoclassicism embraced a new form of modeling: game theory. Though mathematically founded, at its inception, there was some hope that game theory would overcome many of the difficulties of coping with acting man that the older neoclassical modeling confronted. John von Neumann and John Nash, two of game theory's earliest developers, were both mathematicians. But one of its founding fathers, Oskar Morgenstern, came from an economic tradition outside the mainstream.[10] Morgenstern attempted to introduce the elements of real time, imperfect foresight and equilibration processes to the nascent field.[11]

Unfortunately, his influence was short lived. Game theory emerged precisely because of the failures of the general equilibrium framework (Rizvi 1994), and yet it was fated to follow in its path. Instead of incorporating realistic, acting individuals into economic analysis, it postulated agents more akin to robots.[12] Game theoretic modeling was an advance in that it at least recognized that individuals can meaningfully interact. But to make the modeling exercise tractable, it adopted simplifying assumptions about stable preferences, fixed and exogenously given payoffs, and complete and perfect information, which, in addition to removing precisely those features that comprise the substance of the economic world, are also entirely unrealistic.

Of course, as Leontief as pointed out, "it is precisely the empirical validity of these assumptions on which the usefulness of the entire [mathematical] exercise depends" (1971: 2).[13] But assumptional invalidity did not prevent most neoclassicists from charging ahead. Even later, when the latter assumptions were relaxed, a new

[10] Morgenstern was in fact an Austrian.
[11] For more on this development see Mirowski (2002).
[12] Mittermaier (1986) called this tendency in neoclassical economics "mechanomorphism."
[13] On this point and the (missing) issue of magnitude in mainstream economic analysis see McCloskey (2002).

form of cognitive perfection was introduced in the form of Bayesian updating. These modeling devices created same sort of determinism that doomed the general equilibrium framework. Real actors were again banished from the models, but this did not seem to bother many economists since the threat to economics' status as a genuine science was averted.

Game theory proved to be a far more adaptive form of modeling within the neoclassical structure than the GE framework had been, which is at least partly responsible for its staying power in mainstream economics. The "folk theorem" showed that an infinite number of equilibria were in fact possible in repeated games, as long as agents' discount factors are sufficiently low, which meant that game theoretical models could be made to neatly fit any observed behavior. The "anything goes" consequence of the existence of multiple equilibria brought about a kind of "formalistic historicism" in modern economics, in which the method of reasoning—mathematical modeling—was universal, but the results of the models were particular.

The parallels between modern neoclassical economics and the mechanical philosophy in the 17th century are quite striking. Both claimed a monopoly on scientific explanations and sought to exclude from consideration certain critical elements of the phenomena they studied because they did not fit into their schemata. In the mechanical philosophy this meant rejecting explanations that posited the operation of forces not resulting from the collision of bits of matter. In neoclassical economics it meant jettisoning elements that were irreconcilable with formal modeling and econometric measurement, removing genuine human action from the scope of economic analysis. Both the mechanical philosophy and neoclassical economics bolstered their claim to the title of science with formal models and then

used this method of analysis as a criterion to judge the extent to which competing explanations were scientific.

4. Damaging Detours of Neoclassical Orthodoxy

As RAND economist, David Novick, warned in the 1950s amidst a discussion about growing scientism in economics: "The current use of mathematics in social science is largely a form of intellectual shorthand and in no way demonstrates that the methods heretofore so successful in the physical sciences have suddenly become adaptable to the social sciences . . . the theory may be a most interesting one, susceptible to 'toy' proofs, but not at all adaptable to the facts of the real world" (1954: 357). Novick was pointing out that neoclassicism's aping of the natural sciences came at the expense of understanding the economic world.

But his warning fell on deaf ears. Neoclassical scientism forged ahead, and, just as the mechanical philosophy's dogmatism hindered scientific progress in the 17th century, so did the strictures of neoclassical economics stunt advancement in economic understanding in recent times—sometimes successfully derailing the discipline for decades. There are several notable examples of this.[14] One is the neglect, until quite recently, of institutions in economic analysis. General equilibrium is a timeless world of perfect information, instantaneous market clearing, and zero mistakes. Therefore, it is not surprising that institutions, which underpin real market interactions, are entirely absent from its framework. In the world of general equilibrium, institutions by assumption have no role to play.

Institutions are coping mechanisms that emerge to facilitate social cooperation where there is change and thus uncertainty, time and thus lagged responses between

[14] For additional examples besides those considered here see Boettke, Coyne and Leeson (2005).

interacting variables, and errors and thus a need for some mechanisms of dealing with those errors. They operate only where there is a society that gives rise to them. The Walrasian auctioneer and infinite number of atomized utility maximizers in GE hardly constitute a society, so institutions are absent.

Institutions therefore did not really appear in modern mainstream economic analysis until the pioneering work of Doug North, and even then they did so slowly and in a rather restrictive fashion.[15] The emergence and dynamic evolution of institutions has only recently begun to infiltrate mainstream economic discussions and issues relating to how individuals mental structures interact with and map into formal and informal institutions have just now started to receive attention. Despite some positive signs, these critical areas of inquiry remain stifled by the mainstream's refusal to accept unquantifiable and formally intractable elements in economic analysis. Thus, with few exceptions, the preponderance of research that takes the complexity of institutions seriously is being undertaken outside of the mainstream among more heterodox fields of economics, such as evolutionary economics, and in sister disciplines like sociology.[16] To the extent that neoclassical economics has dealt with institutions, they are included as exogenously given parameters and treated like budget constraints in standard optimization models.

In practice, the failure to analyze or even recognize a role for institutions meant disaster for development policy. The mainstream economics world was stunned when its solution in the post-socialist transition countries of "getting the prices right" failed miserably. In the neoclassical framework, the reason for this part of the world's

[15] North's earlier work (1981, 1990) in particular treated institutions as simple constraints, amenable to traditional neoclassical analysis. His more recent work (2005), however, rejects this approach and instead takes one that seeks to appreciate their complexity. It is also worth noting that for his entire career, though especially the latter part of it, North was working on the outer edge of mainstream, neoclassical economics.

[16] For more on the exclusion of institutions from economic analysis, its effects, and recent attempts to take institutional analysis beyond that engaged by neoclassical economics see Boettke et al (2005).

economic troubles was that prices were not free to respond to market conditions. Free its prices and you will free its prosperity.

To be sure, there is great truth in this insight. Free pricing is indeed a necessary condition for economic progress. But it is not sufficient. The type of markets that generate widespread prosperity do not appear *ex nihilo* without the proper institutions that enable them to emerge in the first place.[17] Standard neoclassical remedies take a host of these institutions for granted. Well-defined private property rights, for instance, are needed before price liberalization can work any of its beneficent effects. Without it, price liberalization is meaningless. But this institution simply did not exist in much of the developing world—a fact, which, only after a series of failed solutions had been tried, was actively acknowledged by neoclassical theory.

Furthermore, as the development community is slowly coming to realize, a variety of informal institutions are in turn required for more formal ones, such as property rights, to work. Informal institutions, such as "bridging" civic capital, certain belief structures, and appropriate indigenous customs and social norms are critical to making property rights "stick" in underdeveloped countries. But these cultural aspects that comprise the political economy of everyday life, much like entrepreneurial discovery and genuine uncertainty discussed above, defy measurement and formalization. They are therefore rarely mentioned, let alone genuinely treated in modern mainstream analyses, which continue to direct professional attention largely along lines that though formally tractable and measurable, have already been explored and found wanting in ability to generate useful knowledge for the economic world.

[17] On how different institutional regimes generate different types of markets and entrepreneurial behavior see Coyne and Leeson (2004).

Traditional arguments about market failure and the need for state intervention proffered by Pigouvian welfare economics suffered from similar errors of omission, and unleashed a separate set of plagues on economic study. The problems of this approach were pointed out more than 40 years ago by Ronald Coase (1960) and later James Buchanan (1969), but their arguments have been largely ignored owing to mainstream economics' preoccupation with modeling and measurement. Not only do their criticism's damn the model and measure mentality that is part and parcel of the Pigouvian framework, their analyses themselves are mathematics and econometrics-free.

In the Pigouvian world, deviations between private marginal costs/benefits and social marginal costs/benefits generate market failures in which the private sector oversupplies goods for which the PMB > SMB (or PMC < SMC), and undersupplies goods for which the PMB < SMB (or PMC > SMC). Government is called upon to correct this failure by taxing producers in former case and subsidizing them in the latter to bring private marginal benefits/costs and social marginal benefits/costs into alignment.

Conceptually, the Pigouvian solution is sound—but only if one remains inside its neoclassical assumptions, which neglect the role of both institutions and information. Coase and Buchanan's argument was precisely this. If these factors, necessarily absent from the Pigouvian model, are acknowledged, government intervention is either redundant or non-operational. On the one hand, if we allow for the fact that private parties develop private solutions to obstacles that confront them (the function of informal institutions), private actors can negotiate away the conflict despite the presence of positive transaction and information costs. If, on the other hand, positive transaction and information costs mean that private parties are unable

to realize the relevant costs and benefits and bring them into alignment, how are government officials to do so? In short, recognizing a role for institutions that enable individuals to overcome transaction and information costs makes the Pigouvian scheme either unnecessary or unworkable.

Despite being well known, since these issues collide with the neoclassical approach, many mainstream economists still resist the arguments of Coase and Buchanan. William Baumol, for instance, maintains that the Pigouvian tradition is "impeccable" even while admitting that "All in all, we are left with little reason for confidence in the applicability of the Pigouvian approach, literally interpreted. We do not know how to calculate the required taxes and subsidies and we do not know how to approximate them by trial and error." As a result of this willful resistance, not only have alternative avenues of investigation along the more realistic lines suggested by Coase and Buchanan gone largely unexplored, but governments, making use of the neoclassical model, have gone about attempting to correct market failures through tax and subsidization policy. In the best case the result was squandered resources, and in the worst case, the market was left more distorted than before the "improvement."

It seems that neoclassical economics would rather cling to a theory for its elegance, than reject it for the fact that once elements of reality are incorporated, it no longer makes sense. Where markets are said to fail, neoclassical economics thus continues to generate mathematical models to identify this failure and show how government can be used to correct it. As Coase summarized the problem: "My point was simply that such tax proposals are the stuff that dreams are made of. In my youth it was said that what was too silly to be said may be sung. In modern economics it may be put into mathematics" (1988: 185).

Perhaps the largest and most devastating detour generated by neoclassical economics was the notion that socialism could somehow not only match capitalism's capacity to create wealth, but in fact surpass capitalism by also eliminating monopolies and business cycles thought to be endemic to the market system. It may seem a strange thing to say now, since we currently associate neoclassical economics with pro-market reforms, but we must not forget that it was this same neoclassical economics that at one time made central planning seem possible. Indeed, it led prominent economists like Paul Samuelson to declare as late as 1989 that socialism could economically out perform, and in fact was economically out performing, capitalism (Samuelson 1989).

In the 1930s the Polish economist Oskar Lange and Abba Lerner launched their defense of market socialism against Ludwig von Mises and F.A. Hayek on the basis of the neoclassical model. They proposed the following: First, allow a market for consumer goods and labor allocation. Second, put the productive sector into state hands but provide strict guidelines for production to firms. Managers would be directed to price their output equal to marginal costs, and produce that level of output that minimizes average costs. Adjustments can be made on a trial and error basis, using inventory as the signal.

In the neoclassical framework these production guidelines ensure that the full opportunity costs of production are taken into account and that all least-cost technologies are employed. The market socialists saw the neoclassical model of competitive equilibrium as the formal blueprint for socialist planning to follow. Socialism simply needed to reproduce the micro-efficiency conditions of capitalism in the neoclassical model of competitive equilibrium.

Only in the wake of this argument did Mises and Hayek understand that they were not neoclassical economists. Up to this point, both believed that there was no longer such a thing as "Austrian economics." Its insights had been fully incorporated into the neoclassical framework; to twist a phrase made famous by Richard Nixon, in the minds of Mises and Hayek, "We're all neoclassicists now." They were thus astonished when the very argument leveled against them by the socialists came from neoclassical economics. In the midst of this debate they realized that their understanding of the market fundamentally differed from the neoclassical one. Their whole point was how, with inappropriate institutions and thus no means of generating the necessary information, could a firm manager under socialism know what marginal cost was or know where the lowest point on the average cost curve was located?[18]

The institutions that underlie markets enable market participants to capitalize on the division of knowledge and learn to satisfy consumers through the market process. Private property and market prices under the profit and loss system guide producers' decisions such that prices tend towards marginal cost and the least-cost technologies are employed. But without the market process and its attendant institutions, there is no means of discovering these things. Competitive equilibrium is a hypothetical construct—a highly stylized and thus entirely unrealistic set up

[18] The Austrian argument against socialism tended to assume away problems of worker incentives and planner motivations to prevent the socialists from challenging their arguments on grounds of value-ladeness and to demonstrate the fragility of socialism even under the best case assumptions about incentives and motivations. Their argument was that even if socialism confronted no incentive problems and planners were perfectly benevolent, the absence of market prices would prevent them planners realizing their ends. This argumentative strategy focused attention on the informational issues socialism faced and ensured the value freedom of the Austrian criticism, which purely evaluated the appropriateness of the means chosen by the socialists (state ownership) to attain their stated ends (advanced material production). The approach of the public choice economists was precisely the opposite. They relaxed the incentive and motivational assumptions of central planners and tended to ignore informational issues to focus analytical attention on the problems these things presented for socialism. On this see Boettke and Leeson (2003a, 2003b, 2004, 2005).

Interestingly, in the course of the calculation debate, Lange (1936, 1937) actually argued that considerations of incentives should be excluded from the discussion on the grounds that they lie in the realm of sociology and not economics. This is somewhat ironic since, as we discuss later, the neoclassical argument against socialism became one about incentives and Lange's argument was leveled on neoclassical grounds.

describing the static state of affairs that real markets tend towards when no additional change is introduced. But it has little to do with the operation of the real world. Real markets are characterized by constantly changing tastes, technology and resources.

How can central planning adapt to these changing conditions? The central contention of Mises and Hayek was that it can't. The "formal similarity argument" of Lange and Lerner is no help because it only makes sense in the context of perfect information and zero change. The productive efficiency conditions that define competitive equilibrium are only realized *ex post* through the market process. They constitute a backward looking description of where the market tends only after the market process has been allowed to work itself out. Murray Rothbard (1962) likens equilibrium conditions to the mechanical rabbit in a dog race; the dogs represent entrepreneurs who chase, but never catch the rabbit. However, without the entrepreneurial market process, we have no clue as to where the rabbit is. Therefore the conditions defining competitive equilibrium cannot be used as *ex ante* guides for production decisions by a socialist planning board.

In deploying the formal similarity argument the socialists were assuming precisely what they needed to prove. But this fact did not prevent the Soviet Union from attempting to centrally plan society. Nor did it prevent prominent neoclassical economists from conceding the socialists' position that economic calculation was in fact unproblematic for central planning, and in doing so stifling economic inquiry that examines the full nature and extent of the institutional/informational problem identified by Mises and Hayek. Joseph Schumpeter, Leon Walras, Vilfredo Pareto, Enrico Barone, Fredrick Taylor and Frank Knight all argued that for socialism to match capitalism it would merely have to satisfy the same formal efficiency

requirements that markets did under conditions of equilibrium.[19] Incapable of reasoning outside the Walrasian box, Schumpeter and Knight, for instance, were persuaded by the logic of Lange's argument, which did in fact describe, on neoclassical grounds, how in strictly economic terms at least, socialism could achieve capitalism's productive efficiency.[20]

Thus, instead of analyses that would consider the connection between institutional forms and informational constraints (which was only much later taken up in part by the New Institutional Economics), effort was channeled into material balances approaches, mechanism design theory, and input-output analysis towards the end of optimal planning, which, it is now widely recognized, did little to increase economic understanding or improve the state of the economic world. For decades following the socialist calculation debate, until the collapse of socialism in the early 1990s, the desirability of socialism was defended by many, and the neoclassical model backed their claims. Even for those who believed socialism was a bad idea to embark on, with the exception of the Austrians and a few other heterodox economists not wedded to the GE framework, socialism was thought to be problematic because it suffered from political problems involving issues of bureaucracy and incentives of workers, not because of a fundamental confusion of reality with the GE model.

Still today, some of the world's most prominent economists contend that socialism's failure had nothing to do with the informational concerns that Mises and Hayek pointed out were necessarily missing from the neoclassical framework (see, for instance, Shleifer and Vishny 1994). In their eyes, Lange and Lerner were right; as the neoclassical model shows, there is no information problem for socialism. Central

[19] For more on the socialist calculation debate and its connection to the neoclassical framework see Boettke (2000).

[20] For Schumpeter and Knight, for instance, socialism was not an economic flawed but instead suffered from political problems, which made it undesirable.

planning collapsed purely under the pressure of weak worker incentives and bad bureaucrats responding rationally to perverted costs and benefits generated by state ownership. Admitting a role for the informational problems identified by Mises and Hayek would reveal serious shortcomings of the neoclassical approach and threaten its very foundation. Thus neoclassical economics took the profession, and indirectly the world, on a wily ride through the decades of socialism when Paul Samuelson lamented the problems of capitalism and lauded socialism's ability to out perform the market.

5. Concluding Remarks

Today, the prevailing belief is that genuine economic science must be mathematical and quantitative. This currently popular methodology has been imposed on diverse disciplines with little regard to whether it is suitable to their subject matter, but simply because it is thought to be the only respectable way to do science.[21] The philosopher John Dupré calls this "scientific imperialism," meaning "the tendency for a successful scientific idea to be applied far beyond its original home, and generally with decreasing success the more its application is expanded" (2001: 16). As in the 17th century, we witness a frantic effort to generate models fitting the accepted paradigm with little regard for the realism of the assumptions and mechanisms from which they are constructed.

We hope that we have made the relevance of the history of the mechanical philosophy to the circumstances with which non-mainstream economists currently struggle apparent. Natural science eventually abandoned the restraints imposed on it by the mechanical philosophy in the interest of progress. This episode can serve to

[21] For more on this see Leeson and Boettke (2005).

illustrate several lessons that should give hope to those who hold unfashionable views in economics today:

First, it is not the case that science always makes steady progress; it sometimes enters cul-de-sacs that it must eventually back out of in order to move forward again. This is especially the case when a methodology from one science is imposed on another without concern for its aptness in the new domain. Although the mechanical philosophers' monopoly on science stunted advances in knowledge, it was ultimately dislodged and, free from its fetters, science again was able to progress. There is no reason to that this could not also happen in economics, no matter how dim the chances appear at present.

Second, it is not the case that scientific views can be decided by a "market test." Science is not toothpaste, and markets cater to the preferences of participants, without regard to whether those preferences arise from scrupulous examination or ill-considered prejudice. Where individuals incur no real cost for holding incorrect beliefs, they will be more inclined to do so. Economic ideas seem to satisfy this requirement quite easily. It is true, if the economics community expends its intellectual efforts on useless ideas, the entire community (and those outside of it) suffer. But each individual member of this community bears only a tiny fraction of the total cost. If a researcher vested a great deal of effort in defending "bad theory," her benefit from defending the status quo is significant and surely enough to outweigh the small cost she bears from maintaining prevailing beliefs, even though they are mistaken. All of this simply means that the most popular view need not be the correct one. Thus, the heterodoxy of a position in economics is not necessarily a statement about its validity, as many would suggest.

Third, it is not the case that a real science must forget its forefathers. Often, the key needed to unlock some gate barring the way forward can be found in the ideas of a long-dead thinker. This was partly the case with Newton, whose ideas finally triumphed over the mechanical philosophy, and could very well be the case in economics as well. Unfortunately, under the weight of popular scientific preconceptions in the economics profession, the field of history of economic thought is quickly dying. Only a rapidly shrinking number of departments offer courses in the subject, there are only a handful of jobs available in history of thought each year, and the field is generally looked down upon by the rest of the economics profession. This is serious mistake on the part of modern economics, but those who have retained their interest must forge ahead, as they may be in the best position to revolutionize economics if and when the neoclassical hegemony comes to an end.

Finally, it is not the case that economists should placidly drift with the prevalent scientific tide like so many jellyfish bobbing in the waves. If Newton had taken this attitude, we might still be stuck with the mechanical philosophy. The greatest scientists have often been the ones who had the courage to swim against the current and economics is no different.

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