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Economics and Its Modes

Abstract: Often different schools or styles of doing economics are seen as inevitably at odds with each other, so that one must be crowned 'correct' and the others vanquished as defective. However, if they actually represent alternative but potentially enlightening views of economic phenomena, then it will be foolish exclusively to pursue one approach at the expense of all others. This paper argues that the latter is a more accurate view of economics than is the former.²

Keywords: economic methodology, methodological pluralism, modality, Oakeshott.

I. Introduction

'The social sciences, said Schumpeter, suffer from two deep-seated and pernicious ills: 'first, from that almost childish narrow-mindedness which regards its own method of work as the only possible one, which is to make it the universal one, and considers it ones foremost task is to annihilate all others in holy anger; second, from that complete lack of even elementary knowledge of all branches of learning outside one's own...When at last will the day come when all will realize...that the ocean of facts has innumerable different aspects

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which call for innumerable different modes of approach?' (Machlup, 1978: 464)

Shifts of methodology are often accompanied by manifestations of exclusiveness; new methods, not unlike new religions, often claim to be uniquely qualified to discover 'the truth' (Gottlieb 1968: 22).

Today, under the general heading of 'economics', we find a number of disparate activities cohabiting, intellectual pursuits whose practitioners, while occasionally accepting peaceful co-existence with colleagues following different approaches, more often interact with them in what might be called, with little exaggeration, 'methodological warfare.' Quite typically, the presently ascendant neoclassical economists dismiss any deviant views of how to address the subject as 'unscientific.' Meanwhile, their heterodox critics, including Neokeynesians, Marxists, Austrians, critical realists, and new institutionalists, often accuse the neoclassical mainstream of being trapped in a dogmatic devotion to abstract formalisms bearing little relation to economic reality. And prominent macro-economists have even expressed doubts as to whether their discipline has any solid core at all.³

This paper argues for a ceasefire in the 'war of the schools' even while warning of the pitfalls of rummaging among their ideas in a naively eclectic fashion. The case for a truce is built on the belief that a particular school or method of economics is not obliged to drive all alternatives from the playing field in order to justify its own right to exist. In terms of the most general divisions of economics into different sub-fields and schools, what are offered are not mutually exclusive explanations of economic events underpinned by a common explanatory framework. If that were true, then indeed the acceptance of one of the rival approaches would entail the rejection of all of the others. Instead, the most fundamental, seemingly

[3] See Garrison (2001: 3).

irreconcilable disagreements in economics are the result of practitioners directing their inquiries towards inherently distinct modes of explanation.⁴ If the battling approaches are correctly recognized as alternative perspectives, each potentially achieving a characteristic understanding of economic reality based on its own, unique vantage point, then the notion that all other options must be vanquished is revealed as an illusion. On the other hand, understanding the modal nature of the various sub-disciplines within economics should alert theorists to the perils of indiscriminately conjoining insights plucked from two or more research programs that are built on distinct, perhaps incompatible, presuppositions.

For those unfamiliar with the philosophical concept of modality, a brief introduction to it may be in order. The ideas presented here are not novel, except, in so far as I can determine, in their application *within* economics. But the idea that human knowledge exhibits modalities characterized by their *a priori* assumptions emerges from a philosophical tradition recognizable at least as early as Descartes and Spinoza. That pair viewed the modality of knowledge as stemming from the 'essences' of the *objects* of knowledge. However, Kant's 'Copernican Revolution' turned that model on its head, tracing the original source of modality to the perceiving subject herself, an unavoidable consequence of her limited viewpoint and her disposition to seek novel ways of seeing the world. As Kant expressed this idea: 'The categories of modality... express the relation of the concept to the faculty of knowledge' (1929: 239).

Over a century after Kant, Michael Oakeshott conceived the nature of modality, 'a mode of experience is experi-

[4] Van Fraassen (1980) categorizes such distinct types of explanation as being framed in terms of different *contrast classes*.

ence with reservation, it is experience shackled⁵ by partiality and presupposition; and its character lies, not merely in the particular reservations and postulates which distinguish it, but in the entire world of ideas which these postulates... imply, call forth, and maintain' ([1933] 1985: 74). The foundation of the present paper is largely built upon Oakeshott's exposition of modality.

II. The Modal Character of Theorizing

The adventure of theorizing is initiated when something already partially recognized and understood incites a quest to comprehend it more fully and coherently. Objects and classifications that were taken on their face value in the world of everyday practice begin to be examined in the attempt to discover what deeper principles might lie behind the world of appearances. This effort entails abstracting *ideal characters* from the concrete flow of experience. But such idealizations do not provide a terminus to the process, for it becomes apparent that they themselves contain pre-suppositions that are in turn open to critical scrutiny.

However, that does not imply that the theorist must perpetually strip away successive layers of postulates without rest. As Oakeshott says:

This engagement to be perpetually *en voyage* may be arrested without being denied. The theorist who drops anchor here or there and puts out his equipment of theoretic hooks and nets in order to catch the fish of the locality, interrupts but does not betray his calling. And indeed, the unconditional engagement of understanding must be arrested and inquiry must be focused upon a *this* if any identity is to become intelligible in terms of its postulates. An investigation which denies or questions its own conditions surrenders its opportunity of achieving its own conditional perfection; the theo-

[5] Over several decades of subsequent theorizing, Oakeshott continued to regard modes as conditional and partial 'platforms of understanding', but this early contention that they were therefore 'shackled' or 'defective' disappeared from his later works.

rist who interrogates instead of using his theoretic equipment catches no fish. (1975: 11)

The idea that progress in understanding experience from a particular vantage depends on adopting, rather than questioning, the assumptions establishing that viewpoint, also has been forwarded by Longino, albeit with emphasis on the practical use of a theoretical discipline's results:

[I]f scientific inquiry is to have any effect on a society's ability to take advantage of natural processes for the improvement of the quality of its life, criticism of assumptions cannot go on indefinitely.... The utility of scientific knowledge depends on the possibility of finding frameworks of inquiry that remain stable enough to permit systematic interactions with the natural world (1990: 79).

What is the nature of the fishing grounds at which a theorist may dally? Oakeshott terms any such location that has been methodically explored and charted a 'platform of understanding', a place from which the theorist may survey the conditional intelligibility of the surrounding waters. And it follows from the above considerations that the foundation of each such platform is the particular set of postulates that characterize, direct, and limit the type of understanding it makes accessible.

III. The Two Fundamental Categories of Theorizing

Although the number of possible platforms of understanding appears to be limited only by human ingenuity, they all fall into one of two fundamental categories, representing two distinct viewpoints the theorist may adopt towards the phenomena being investigated: he can regard goings-on in the world either as an active displays of intelligence or as a mechanical, unintelligent processes. There is nothing objectionable in alternately examining the same phenomenon from both vantages: a family vacation usefully may be understood as an exhibition of the laws of mechanics, thermodynamics, and chemistry, or compre-

hended in terms of the beliefs, intentions, and actions of the members of the family in question. However, the co-presence of assumptions from both categories in a single explanation signals a categorical confusion. Typically, in such cases, effects conceived in terms of one category are traced to causes conceived in terms of another: The engine overheating in the family car, seen from the standpoint of auto mechanics, is blamed on the driver's angry reaction to a remark one of his children made, or the mechanical laws governing the internal combustion engine are invoked to answer the question of why the family chose to drive to Orlando rather than Pittsburgh for its vacation. (Certainly, the family might have taken various physical aspects of cars, such as their poor performance underwater, in deciding, say, not to attempt an automobile trip from New York to London. But such facts are relevant to understanding the choice of destination in that they were *considered* by the actors involved, and not as mechanical determinants.) And it is worth noting that to embrace the view that theorizing has these two, fundamental branches does not necessitate adopting Cartesian-style ontological dualism. It is compatible with most ontological stances, except hard-core, physicalist reductionism; in particular, it is compatible with supervenient reductionism. It is a theory about modes of *explanation*, not modes of *being*.

Like the related idea of modality, this basic epistemological duality appears, famously, in Descartes. Spinoza again follows, but with a twist, and a premonition of Kant, in his conclusion, 'the mind and the body, are one and the same individual, which is conceived now under the attribute of thought, now under the attribute of extension' (1994: 132). Once again, we pass through Kant, who observes that Spinoza's idea applies to the theorist himself: 'The man who believes himself endowed with an autonomous will... places himself in another order of

things and relates himself to determining grounds of an entirely different sort from when he perceives himself as a phenomenon in the sense-world and subordinates his causality to external determination under natural laws' (quoted in Cassirer, 1981: 257-258).

In the late nineteenth and early twentieth centuries, the ideas of Spinoza and Kant re-surfaced in the work of the 'neutral monists' such as Mach, James, and Russell.⁶ Some positivists took this monism to imply that only the physical viewpoint is necessary, as they believed that mental events could uniformly be translated into the physical mode of description, while the reverse is not true. But the assertion of the universal translatability of mental descriptions into physical ones is ungrounded, as demonstrated by Donald Davidson (2001: 207-227).

Some theorists have posted 'methodological dualism' as, perhaps, a stopgap measure only required pending future advances in our understanding of the physical sciences.⁷ To the contrary, Spinoza and Kant understood it as the consequence of the primitive dichotomy intrinsic to theorizing, as, more recently, did Oakeshott:

This distinction between 'goings-on' identified as themselves exhibitions of intelligence and 'goings-on' which may be made intelligible but are not themselves intelligent, is not a distinction between mental and physical or between minds and bodies regarded as entities. It is a distinction within the engagement of understanding, a distinction between 'sciences' (that is ideal characters) and the identities with which they are concerned. And in calling it a categorical distinction what is being asserted is that the understanding of identities recognized as themselves exhibitions of intelligence cannot be 'reduced' to the understanding of identities not so recognized. (1975: 14-15)

Davidson offers a contemporary defense of a fundamental bifurcation in theorizing:

[6] See Blanshard, 1962: 111.

[7] See, for instance, Mises ([1949] 1998).

When we attribute a belief, a desire, a goal, an intention or a meaning to an agent, we necessarily operate within a system of concepts in part determined by the structure of beliefs and desires of the agent himself. Short of changing the subject, we cannot escape this feature of the psychological; but this feature has no counterpart in the world of physics. (2001: 230)

IV. The Idioms of Theorizing

Oakeshott describes each of the two main categories theorizing as composed of various 'idioms':

[A] theorist... has at his disposal... what I have called 'idioms' of inquiry. Each such idiom is an unambiguous system of theorems which has acquired (or which aspires to) the condition of a distinguishable 'science'... (1975: 17)

For example, an event considered as a mechanical going-on may be theorized within the idiom of, for instance, classical mechanics, relativistic mechanics, quantum mechanics, chemistry, molecular biology, or genetics; and an event regarded as an exhibition of intelligence may be further viewed from the standpoint of history, jurisprudence, ethics, politics, aesthetics, semiotics, or sociology.

Each such idiom is based on one a set of postulates in addition to the fundamental assumption determining in which of the two basic categories of theorizing it operates. For example, Cassirer notes that modern physical science depends on 'the presupposition that nature is determinately quantified, the presupposition that specific elements in it are unchanging and conserved, and the presupposition that events follow one another according to a rule' (1981: 166). Furthermore, it must assume a relationship exists between the aforementioned quantities and scientific measurements; as Kyberg puts it: 'The theory of measurement is not only a proper theory, it is a theory that cannot be refuted in any simple way by a contrary observation, and this deductive awkwardness immediately spreads to the testing of any quantitative law' (1988:

70). Classical mechanics must assume at least one of Newton's three laws of motion, as it is possible to empirically test any two of them only by positing the third *a priori*.⁸ Chemistry assumes the concept of chemically distinct substances. Biology assumes the idea of living things⁹, jurisprudence the notions of law, legal authority, and legal obligation, and ethics a meaningful distinction between virtue and vice. Collingwood's 1940 *An Essay on Metaphysics* pre-figured Thomas Kuhn's more famous work in demonstrating the unavoidable dependence of sciences on pre-supposed metaphysical principles. And more recently, Kitcher ([1984] 1998) traced the irreducibility of classical genetics to molecular genetics to the genetic hypothesis, foundational to classical genetics but absent in its molecular sibling.

This model of idioms can be profitably compared to some other, recent models of the scientific enterprise. Its similarity to 'the methodology of scientific research programmes' developed primarily by Lakatos (1978) is obvious, where the Lakatosian 'hard core' of a program maps neatly to our concept of the founding postulates of a discipline (or of a school within a discipline in a state of fundamental flux).¹⁰ The modal view also can be likened to Kuhn's model from *The Structure of Scientific Revolutions*. In Kuhn's theory, *paradigms* play a role analogous to platforms of understanding in our own. What Kuhn calls *normal science* is a period during which a science uses its postulates rather than questioning them. A Kuhnian *paradigm shift* involves abandoning at least some of a discipline's fundamental assumptions and replacing them with new ones.

[8] This was convincingly demonstrated by Kuhn (2000: 65-73).

[9] See Polanyi (1962: 347-380) on this point.

[10] Rizzo (1982) suggests the correct understanding of the *a priori* foundations of logical economics (discussed in Section V) is as a set of postulates forming the hard core of a Lakatosian research program.

However, the view adopted here clarifies the nature of how scientists come to adopt one or another paradigm during revolutions, an issue that is obscure in Kuhn. One puzzle he leaves mostly unanswered¹¹ is how paradigm choice, given that competing paradigms disagree about what postulates should form the 'hardcore' of the discipline, and that those postulates are the primary guides as to what makes one theory superior to a rival, can be made on rational grounds. According to Kuhn it appears that, in the mid-sixteenth century, a scientist's preference for either the Ptolemaic or Copernican system could only be a matter of taste, style, or extra-scientific social factors.¹² Kuhn is correct in contending that such a shift cannot be based primarily on *scientific* grounds, because precisely what is under dispute is what the proper grounds of the science in question *are*, so that two schools may espouse incompatible criteria for evaluating theories. But in our view that does not make such choices necessarily irrational, based solely on political power, social structures, fads in thought, or any of the other, non-rational factors by which it has been said such issues are resolved. A choice between Kuhn's rival paradigms or among the postulates competing for the status of foundational for a discipline can be made, at least in part, on *philosophical* considerations and, as such, it can be the outcome of critical, reasoned debate. As Longino says: '[T]hese discussions involve issues that are metaphysical or conceptual in nature and that, far from being resolvable by empirical means, must be resolved (explicitly or implicitly) in order to generate questions answerable by such means' (1990: 73).

If the coherence and distinctiveness of a discipline grow upward from the roots of its founding postulates, then

- [11] In Kuhn's (2000), he repeatedly rejects the complaint that his model of scientific revolutions renders paradigm change an irrational affair.
- [12] A number of others have noted this difficulty in Kuhn, e.g., Laudan (1998), Popper (1970), and Hempel (1998).

what are we to make of activities such as cross-disciplinary research efforts, the importation of the results achieved in one discipline into a foreign field, or of the appearance of concepts that emerged from a theoretical science in the practical world of technology and engineering? If the integrity of the interacting fields is to be maintained and the sort of knowledge each achieves is to be properly understood, then we should recognize that ideas and results originating in one idiom must be translated into the language of another for them to be used coherently in the second field. For example, for a new theory in physics to be used successfully in engineering, it must be translated into an idiom that includes terms completely foreign to physics, such as 'safety margin', 'cost-benefit analysis', 'worst-case scenario', 'human factors', and 'aesthetic appeal.' Whitehead recognized this requirement in asserting, 'It is a great mistake to think that the bare scientific idea is the required invention, so that it only has to be picked up to be used. An intense period of imaginative design lies in between' ([1925] 1967: 96-97). Kitcher ([1984] 1998: 988-992) also indicates the need for translation in moving concepts between classical and molecular genetics, and he details some of the difficulties encountered along the way. The topic of translation between idioms will be explored further below, as we proceed to apply the view of theorizing we have outlined so far to modern economics.

V. Modality in Economics

What insights into the current state of economics might be gained by considering it in light of the ideas discussed above? In an attempt to answer that question, I will examine a handful of different approaches now followed by various sub-disciplines or schools within the field, without making any pretence at completeness. I will argue that the common question of which of them is 'the correct' way

to address economic phenomena mistakenly presupposes that one of them eventually must eliminate all of its 'competitors' and emerge as the single, true method of conducting economic inquiry. Instead, I suggest that most of the factions in modern economics represent attempts to comprehend the subject based on different core assumptions, and that there is no sound reason not to 'let a thousand flowers bloom' and enjoy whatever insights a particular approach can yield.

At the same time, it is important not to forget the modal nature of these undertakings. While sound economics can tolerate pluralism, indiscriminate shifting among different idioms of theorizing exhibits a serious confusion. The coherence of an economic sub-discipline is rooted in the consistent adherence to its fundamental postulates. If concepts are to be transplanted from the soil of assumptions in which they grew to a foreign field, they must be altered in order to flourish in their new environment. Unfortunately, as we will see, this process of transformation is often neglected, producing a muddle where findings from one branch of economics are thought to be in conflict with those of another, or where distinct modalities are intermingled so as to produce nonsense.

Furthermore, nothing in the argument here suggests that every modally coherent approach needs to be followed or that each will prove equally fruitful. This paper only argues that different modes of approaching economics are not *of necessity* at odds with each other. It still may be the case that one approach yields results so much more interesting than another that, in a world of scarce resources, it makes no sense to pursue the second. But such determinations can only be made based on letting the various 'flowers' take seed, and seeing which exhibit vigorous growth. But that does not justify ruling out a coherent approach as unscientific and denying it even the chance to take root.

VI. Logical and Mathematical Microeconomics

The oldest economic idiom is the logical, or, as it has sometimes been called, the praxeological tradition. It was often the implicit method of the British classical economists, and hints of it appear as early as the writings of Aristotle, whose ideas were developed further by the Scholastic philosophers such as Jean Buridan, Luís de Molina, and Juan de Mariana.¹³ In the late-eighteenth century, Kant's *Critique of Practical Reason* undertook an extended exploration of the logic underlying instrumental activities as it is revealed by transcendental analysis. Roughly a hundred years later, the economists who founded the 'Austrian School' of marginalism, Menger, Weiser, and Böhm-Bawerk, focused their work on exposing the necessary implications of the fact that people must choose to employ their available resources for the attainment of some ends at the cost of foregoing others. The first explicit formulation of economics as the science of human action may have been Sidney Sherwood's, in 1897: 'Economics deals with wants consciously felt, resources consciously perceived, and consciously directed to the end of gaining conscious satisfaction...' (quoted in Kirzner 1976 [1960]: 153). Wicksteed, Anderson, Fetter, Collingwood,¹⁴ Croce, Mises, Robbins, Oakeshott,¹⁵ Kirzner, and Rothbard are among the later proponents of this approach,¹⁶ with Kirzner (1976 [1960]) even declaring it to uniquely characterize 'the economic point of view'. The logical economists recognize the free exercise of an agent's intelligence, in

[13] See Rothbard (1995).

[14] See, for instance, Collingwood (1925).

[15] For a discussion of Oakeshott as a praxeologist, see Callahan (2005).

[16] In addition to these economists who are unambiguously members of the logical school, the Nobel Prize winners Hayek, Coase, Buchanan, North, and Smith all have acknowledged the school's precepts as composing an important part of their understanding of economics, even while denying that it forms the whole of the subject — a view compatible with the central thesis of this paper.

responding to his circumstances as he understands them to be, as 'the driving force of the market'.¹⁷

However, mainstream economists currently converse in a more recently developed economic idiom, a formalized language in which the economy is postulated as a complex system of mathematical equations. (However, as we shall see, they often find it difficult to avoid lapsing into the logical idiom in their efforts to explain what their models have to do with economic reality.) This current orthodoxy, which is commonly called 'neoclassical economics', is not particularly methodologically self-conscious, perhaps because its present dominance in academia makes critical examination of its precepts seem an idle pastime. For example, much of the mainstream still regards Milton Friedman's *Essays in Positive Economics*, published in 1953, as capturing the essentials of how economics should proceed if it is to be a proper science, despite the fact that many philosophers of science had already concluded, even by the time of the book's publication, that the positivist model, which Friedman embraces, falls seriously short of providing an adequate description of or a useful guideline for the methods even of the physical sciences.

Nevertheless, a number of serious thinkers have contemplated what sort of foundations might support neoclassical theorizing. The understanding of its character that I propose here, namely, that neoclassical economics is a specialized branch of mathematics, also has been forwarded by the philosopher Alex Rosenberg and the political theorist Terry Nardin (2001b). Rosenberg, after carefully examining the character of the discipline, concludes: '[E]conomics is best viewed as a branch of mathematics somewhere on the intersection between pure and applied axiomatic systems... one devoted to examining the formal properties of a set of assumptions about the transitivity of abstract relations: axioms that implicitly

[17] See Kirzner (2000).

define a technical notion of “rationality”, just as geometry examines the formal properties of abstract points and lines’ ([1983] 1994: 672). In a similar vein, Nancy Cartwright sees neoclassical economics as creating artificial ‘socio-economic machines’ constructed to achieve precision by abstracting away the messiness of the real economic world (1999: 137-151).

However, my view differs from the ones above in that they seem to take it for granted that neoclassical economics, as it is conducted today, simply is *the* way to engage in economic theorizing. They do not appear to ask whether economics ever is, ever was, or ever could be fruitfully approached with any different set of presuppositions. Here, I concur with the economist Tyler Cowen, who argues (in his 2003) that logical and mathematical economics should be recognized as distinct subjects, each offering a different *sort* of explanation for economic happenings in response to different modes of interrogation. For instance, the logical school attempts to illuminate how an actor’s efforts to improve her perceived circumstances as much as she is able to do, when occurring within a social context including several property, will result in prices for economic (scarce) goods.¹⁸ On the other hand, a neoclassical explanation, which will have at its core a mathematical model, will try to demonstrate why, under certain conditions that are taken as true by initial assumptions, a *particular* price will arise when some particular quantity of a good is brought to market.

As I see it, conceiving of neoclassical economics as a branch of mathematics neutralizes the common criticisms accusing the approach of precise but irrelevant formalism,¹⁹ of the frequent resort to wildly unrealistic assumptions, and of populating its models with ‘people’ that are

[18] See Menger (2004: 77-113).

[19] E.g., Boettke (1996).

merely automaton.²⁰ However, the bark of these complaints is not without its bite, for in the neoclassical literature, its native method of mathematical theorizing is often mingled uncritically with reasoning based on quite alien postulates; most often the older and more intuitive idiom of logical economics is unconsciously adopted in attempting to relate the behaviour of a model to real events, so that genuine human action is quietly summoned in defense of some parameter in an equation or the shape of some curve. But causally efficacious, conscious choice is excluded from any causally closed, deterministic model by hypothesis.²¹ A system of equations can neither exhibit intelligence nor make choices, and so any proposed explanation of its operation based on such concepts exhibits a categorical confusion. As Lawson puts the matter, 'The desire of [neoclassical] "theorists" to "explain" events in some theoretical manner, combined (if implicitly) with a belief in the universal relevance of the deductivist mode of explanation, sets them deriving theoretical constructs that have conditions to guarantee the 'whenever this event then that event' formulation built into them' (1997: 100). Lawson notes that the very nature of these formal models excludes human action from their purview, so that 'real choice is treated, in the end, as illusory' (1997: 38).

By way of illustration, let us examine an instance of an exposition that inappropriately slides from the mathematical to the logical idiom. While the standard microeconomic model of supply and demand under perfect competition is a perfectly sound piece of mathematical reasoning, many presentations of the model explain its

[20] Representative examples of such criticism include Jacobs (2000), Keen (2001), and Simpson (2000).

[21] And note that I am not wading into the quagmire around the problem of free will and determinism: perhaps all human behaviour is mechanically determined and our sense of making real choices is an illusion. Here, at least, I am only arguing that one cannot have it both ways, offering deterministic models but invoking human action whenever it is convenient.

behaviour using causal factors that are excluded by the very assumptions on which it relies. A typical case can be found in Steven Landsburg's *Price Theory*, a popular textbook for the second level of undergraduate microeconomics. He first warns the student to avoid a common error: 'It is important to distinguish causes from effects. For an individual demander or supplier, the price is taken as a given and determines the quantity demanded or supplied. For the market as a whole, the demand and supply curves determine both price and quantity simultaneously' (1999: 18). In other words, the price of a good traded in a perfectly competitive market is *not* influenced by the decisions of individual market participants as to what to bid or ask for the item, or how much of it to buy or sell. Instead, they all find themselves confronted with a market price that for them is a *fait accompli* and that determines the quantity of the good they will exchange, figures that arise from the mathematical abstraction of intersecting the supply and demand curves. And this assumption is crucial to achieving the mathematical determinism that motivates the model.

However, when Landsburg wants to describe the process by which, under such conditions, gluts and shortages are eliminated,²² he ignores what he has just cautioned his reader never to forget, namely, that causality in the model always resides in the equations, and never with individual buyers or sellers. He imports an explanation appropriate in logical economics, but excluded by his own presuppositions, writing: 'Suppliers do care about the cost of feed corn, and are *willing* to produce fewer pork chops at a given price when that cost goes up' (1999: 18, emphasis mine). The model he is explicating requires that market participants, willy-nilly, supply (and demand) precisely and instantaneously whatever quantity of pork the equa-

[22] In fact, even positing the existence of gluts or shortages, however fleeting, is inconsistent with the assumptions of the model.

tions spit out when fed the inputs representing a change in conditions, a mechanical process in which the number of chops any individual is 'willing' to produce is wholly irrelevant.

A similar case is that of the efficient markets hypothesis. The typical presentation presents a mathematical model of stock price movements in which they are a random walk. The conclusion drawn from this is that it is pointless for individual investors to try to beat the market through trying to outguess other investors, because stock prices 'already contain all available information.' The difficulty is the same as we found in Landsburg's passage. The mathematical model of an efficient market contains no place for human action, and so can offer no advice to guide such action. Nevertheless, investors are told that it is irrational to spend their time studying the market, since all they could discover is already accounted for in current prices. However, in the world of human action, stock prices only contain the information they do because investors have spent time studying market fundamentals, in the rational expectation of profiting. If they heeded the advice of efficient market theorists and never researched stocks, *stock prices would contain no information whatsoever*, presenting huge profit opportunities to whoever performed even a little research. This muddle arises from confusing an abstract model with the world of real human action.

If this argument for the modally distinct character of logical and mathematical economics is sound, then must we dismiss the vast body of mathematical theorizing that has been produced by the neoclassical enterprise as worthless for comprehending events happening in actual markets made up of real people? I do not believe that our considerations call for such a harsh verdict. Rather, I suggest that the rigorous models formulated by mathematical economists can beneficially be used as foils to clarify and check the sort of reasoning that characterizes logical eco-

nomics. However, to avoid the kind of incoherent muddle of inconsistent postulates we saw above, practitioners should be aware when they are using a concept in an idiom foreign to it, carefully considering how to translate it so that it is not gibberish in the new tongue. A work of logical economics might, for example, use the model of perfect competition as an aid in conceiving the state of affairs that, although inherently unrealizable, represents the limiting conditions towards which the actual market process has a tendency to approach asymptotically as the number of participants in a particular market keeps increasing, the information relevant to their decisions to buy or sell is communicated ever more rapidly, and their transaction costs steadily fall.²³ As Mises noted: 'What distinguishes the [logical school]... is precisely that it created a theory of economic action and not of economic equilibrium or non-action. [It], too, uses the idea of rest and equilibrium, which economic thought cannot do without. But it is always aware of the purely instrumental nature of such an idea...' (1978: 36).

VII. Macroeconomics

Macroeconomics is usually regarded as a sub-discipline distinct from neoclassical microeconomics, second only to 'micro' in importance at most universities. Its mission is to search for 'causal connections between hypothetically measurable entities or statistical aggregates' (Hayek, 1988: 98). (The task of creating and refining those aggregates is also part of the theoretical work of macroeconomics, especially since there is no unique procedure for assigning a quantity to phenomena like 'an increase in the general price level' that is indisputably superior to its many plausible alternatives.) In the effort to find these large-scale

[23] For a more extensive discussion as to why real markets can never meet the conditions required for the state of perfect competition, see Callahan and Murphy (2005).

economic regularities—most often searched for at the level of the nation-state in the hopes of improving the economic policy of its government and the well-being of its residents—macroeconomists usually pursue econometric, i.e., statistical, research. The fundamental assumption they employ is that a national economy is a (perhaps extremely complex) mechanical system, the future states of which result from the combined effects of various ‘forces’ in a deterministic or deterministically probable fashion. Any particular macroeconomist can posit that the regularities she hopes to discover or believes she has found as either universal economic laws akin to those of physics and chemistry, or as historically contingent patterns generated by the specific institutions, customs, practices, and beliefs prevalent in some area during some period. I will argue that the existence of universal macroeconomic laws is rendered highly improbable by the nature of economic activity itself, and that the exploration of more or less transient regularities is the proper object of macroeconomic enquiry.

That exploration (at least in the latter form) is both a worthwhile and coherent focus around which to organize a discipline. National and international economic events may indeed be made more intelligible by the discovery of persistent relationships between theoretical entities such as ‘the gross national product’, ‘the level of unemployment’, or ‘the rate of inflation’. However, ignoring the fact that macroeconomics is founded on assumptions quite different from those of either logical or mathematical microeconomics courts modal confusion. Various attempts have been made, most notably by Nobel Prize-winner Robert Lucas,²⁴ to develop ‘microfoundations’ for macroeconomics, tracing the source of macro-level phenomena back to the actions of the individual, rational agents pos-

[24] See Van Parijs (1983) for an effort to give Marxist macroeconomics microfoundations.

ited by neoclassical microeconomics.²⁵ Still, the bulk of macroeconomic theorizing remains modally distinct from any economic understanding of the individual.

As I mentioned above, I believe that the hunt for universal macroeconomic laws offers slim hope of success. For one thing, if we accept the common-sense idea, the denial of which requires extraordinary contortions,²⁶ that people can learn from experience, then it follows that a person's interpretation of physically identical conditions will alter over time, as he adopts new ideas. In consequence, two situations that are the 'same' in every detectable, physical respect except that one occurs earlier than the other, can elicit quite different responses even from the 'same' person. Therefore there cannot be a stable scale for measuring what is *economically* significant about even the most homogeneous, material good, which is always a matter of what that good *means* for some agent. And the lack of such a scale makes truly objective, quantitative comparisons of the economic aspects of unique actions impossible, an impossibility that 'trickles up' into economic aggregates.²⁷ The physical sciences do not suffer from that difficulty, since we assume that the entities they study do not have ideas that affect their behavior, and do not learn from the events that befall them. We do not suspect, for example, that protons may one day decide that they aren't really as attracted to electrons as they had previously believed, so

[25] Lawson notes that Lucas's efforts suffer from the fact that, in his models, 'The problem of aggregation... is implicitly assumed [away] as the behaviour of any sector of the economy, or the economy as a whole, is equated with that of an (optimising) individual' (1997: 82).

[26] One obvious hurdle facing anyone wishing to deny that people really learn new things is the question of how he discovered that they don't without simultaneously learning it.

[27] Of course, if the actions under study were all horizontal leaps, we could objectively measure the distance each covered. But that would not quantify their economic importance — a leap that at one time could make a man rich, because it established a new world record, at a later day might gain him nothing, if a longer jump had been made during the interval in question.

that physicists would have to discover new equations to describe the relationship between the two types of particles.

Because every action is a response to a unique situation, because every action attempts to address a unique unacceptability perceived in that situation, and because every action seeks to achieve a unique satisfaction, the outcome of different actions cannot be weighed on a single scale or be meaningfully assigned commensurable, cardinal values, such as certain amounts of 'utility' gained or lost through their undertaking.²⁸ Although we often can discern commonalities in some of the situations to which agents respond, the commonalities themselves are relevant to an individual performance only to the extent that the agent perceives the commonality as present. There is no objective yardstick by which a social scientist could measure that in some situation, a particular agent was acting 57% as a middle-class male, 30% as a member of the Democratic Party, and 13% as a Lutheran. And even if we counterfactually imagined that there were one, there still would be no justification for assuming that such a weighting would produce identical responses at different times even in one person's life, let alone across different individuals. The sort of constant relationships constitutive of the physical sciences are, in the social sciences, at most a temporary condition. As Mises writes:

In the realm of physical and chemical events there exist (or, at least, it is generally assumed that there exist) constant relations between magnitudes, and man is capable of discovering these constants with a reasonable degree of precision by means of laboratory experiments. No such constant relations exist in the field of human action outside of physical and chemical technology and therapeutics. ([1949] 1998: 55)

And Lawson makes much the same point:

[28] See Oakeshott (1975: 53).

In the social realm, indeed, there will usually be a potentially very large number of countervailing factors [to any particular cause] acting at any one time and/or sporadically over time, and possibly each with varying strength.... [And] the mechanisms or processes which are being identified are themselves likely to be unstable to a degree over time and space.... Indeed, given the fact of the dependence of social mechanisms upon inherently transformative human agency, where human beings *choose* their courses of action (and so could always have acted otherwise), strict constancy seems a quite unlikely eventuality. (1997: 218–19)

Nardin summarizes this view of statistical social science as follows: ‘Generalizations about how people usually behave are not scientific generalizations about a truly time-independent class of phenomena; they are more or less well-disguised descriptions of customs specific to a particular historical situation’ (2001a). Thus, our considerations above yield two hypotheses concerning macroeconomics. Firstly, given the assumption of mainstream macro that the object being studied is a mechanical system comprised of forces such as ‘inflationary pressures’ and ‘capital flows’, the search for microfoundations is neither necessary nor promising. And, secondly, macroeconomists should be aware that the correlations they discover in the operation of such forces are almost certainly historical contingencies rather than timeless, universal laws.

VIII. Neuroeconomics and Logical Economics

Neuroeconomics is an economic sub-specialty that has attracted much attention of late. Once again, I will argue that even though an idiom of theorizing may comprise a genuine ‘platform of understanding’, it is likely to generate serious confusion, as well as the unjustified and deleterious abandonment of alternative approaches, whenever the intrinsically conditional nature of the knowledge it yields is ignored and theorists speaking the idiom assert some unique precedence for its voice over all rivals.

Vernon Smith, who recently was awarded the Nobel Prize in economics, is guilty of such 'imperialism' in his claims for neuroeconomics. In critiquing Ludwig von Mises's philosophy of economics, he fails to grasp the categorical distinction between Mises's preferred idiom of theorizing economics and his own, and so he mistakenly takes the coherence of his own approach to entail the incoherence of Mises's.

According to Smith, several of Mises's central tenets have 'been overtaken by recent trends in neuroscience', which indicate that unconscious processes are much more important than Mises had believed. In fact, Smith claims, 'the brain fools the mind into believing it is in command of mental activity' (1999: 200-201). (It is unclear to me just what the mind 'being in command of mental activity' is supposed to mean, and the view is without attribution. As I understand it, the mind simply *is* mental activity, and therefore cannot be 'in command of it'.) In support of his contention, Smith cites research claiming to show that the brain 'finishes' its 'work' half a second before it lets the mind know about what has transpired. Our minds simply 'observe the output'. Why we should find ourselves saddled with such an entirely pointless quality as, in this view, consciousness turns out to be, is left unexplained, and, seemingly, inexplicable. In any case, Benjamin Libet, who performed some of the studies purportedly backing Smith's case, explicitly rejects the idea that his results leave no room for consciously directed activity: 'My conclusion about free will, one genuinely free in the nondetermined sense, is... that its existence is at least as good, if not a better, scientific option than its denial by determinist theory' (2002: 563).

Neuroscience explores the functioning of the brain seen as a system of not-intelligent, mechanical phenomena, an unproblematic and coherent way of theorizing about humans. But the mechanistic nature of their subject matter

is not a discovery neuroscientists have made along the way, but rather a foundational presupposition of their discipline. That their research only finds such mechanical relationships between brain states is a natural consequence of their assumptions, and therefore offers no support to the thesis that all explanations of events framed in terms of intelligent activity can be 'reduced' to mechanistic explanations without any loss in understanding.

Indeed, if the fundamental distinction, presented in section III of this paper, between the two basic modes of theorizing is correct, then it follows that some electro-chemical activity in the brain and a conscious choice *cannot* coherently be posited as the relata of a causal relationship, for they never stand to each other as elements of a single theory that potentially could exhibit a law-like temporal pattern; instead, they are each elements of categorically discrepant ways of conceiving *the same events*. As Langer put it:

...nerves undergoing electrochemical changes, and the elements of consciousness—emotions, sensations, ideas—as elements of different systems, are not of one metaphysical category.... Their incommensurability... springs from two different concepts of reality: reality as 'primary substance', or 'matter', and reality as the 'datum', or 'immediate experience' But two interpretations, or 'loci', of one form cannot be causally linked in a larger pattern. (1970: 8)

Attempts to causally relate an element from one of these 'loci', such as the firing of a neuron, with an element from the other, like a consciously held idea, represent a categorical confusion. They are not two distinct phenomena characterized and differentiated using the concepts of a unified mode of theorizing, within which one might sensibly be understood as causing the other; they are alternative descriptions of a single phenomenon, each only intelligible from the particular 'platform of understanding' upon which they are constructed. In more concrete terms, 'An inspection of the chromosomes of a performer

is categorially irrelevant to the understanding of his actions' (Oakeshott, 1975: 93). If we devise causal explanations that attempt to straddle this divide, it is as if, after both photographing and X-raying a body, we began to debate whether the legs we see in the photograph cause the legs appearing in the X-ray, or vice-versa.

IX. Conclusion

I hope that the considerations offered above will have persuaded the reader to at least entertain the possibility that there is more than one acceptable method for skinning the economic cat. I believe that the availability of different vantages from which to study economic phenomena, so long as the investigations they inspire are pursued coherently and systematically, is likely to yield a richer and more complete understanding than is the exclusive adoption of any single approach, however much allure the simplicity of a universally sanctioned methodology might have for practitioners. If that belief is even partially justified, then the methodological battles that often ensue when adherents of different schools of economics encounter each other mostly serve to squander intellectual energy, which otherwise could be devoted to pursuing new insights. Instead of fearfully attempting to wipe out all its rivals in a struggle to justify its own legitimacy, a particular school or approach might recognize that an alternative methodology could produce genuine knowledge without rejecting its own, unique tradition and its characteristic idiom of explanation. It is even possible, as I have argued above, for concepts, models, and empirical results to be incorporated within an approach different than the one in which they arose, if sufficient attention is paid to translating them so that can be spoken sensibly in the idiom of their new country.

It could be suggested, contrary to the view just expressed, that the apparent co-existence of several dis-

tinct idioms under a single subject heading is a problem, and that all economists really are, or that they all really *should* be, speaking a single language. If that idea is embraced by someone with an eclectic and ecumenical disposition, it is likely to produce a confused effort to 'blend' favorite phrases from two or more idioms in a single explanation, ignoring the incompatibility of the various assumptions being incorporated into the concoction. On the other hand, a theorist of a more single-minded nature, if troubled by this 'problem', may conclude that all idioms other than his own are only abortive attempts to reach the true, scientific style in which he speaks. Then he will try to 'reduce' any widely appreciated explanation, formulated in some other idiom than his own, to a 'true' explanation using his vocabulary. This paper suggests that both of these ways of denying pluralism in economics are mistaken.

In closing, I suggest that this topic is of far more than 'mere' theoretical significance, due to the influence of economic theories on both government policies and our view of social life. As Keynes noted, 'the ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else' (1935, Chapter 24). If economists exploring different avenues for understanding their subject can learn to communicate with each other more fruitfully and better appreciate the unique insights that each offers, then I believe we all stand to gain.

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