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20 After the "New Economics," pragmatist turn?

William Milberg¹

Introduction

In the past five years there has been an empiricist turn in economic research, a reaction against the "New Economics" that developed in the late 1970s. The New Economics was itself a reaction to the era of general equilibrium, in which economic knowledge was understood to progress through mathematical proofs of the existence, stability and uniqueness of a general equilibrium set of prices and quantities which appealed to successively weaker sets of assumptions. The New Economics was a response to the widely perceived irrelevance of the general equilibrium approach. By reversing the hypothesis generation process from a strict hypothetico-deductive formula to a creeping inductivism, and adding imperfect competition, increasing returns to scale technology and strategic behavior by firms and states to the analysis of markets, the New Economics became more relevant but less robust than the general equilibrium models of the previous era had been. The lack of robustness was a problem for those interested in drawing policy conclusions from the models. Equally important was the growing sentiment that the models were *ad hoc* and could be used to model any pre-determined outcome.

The response to the weaknesses of the New Economics in the late 1990s was an empirical turn. In this era, hypotheses are often rooted in simple economic logic, intuition, or even as a response to current events, and emphasis is placed instead on the sophistication of the measurement of variables and the correlations among them. The move is reminiscent of the work by Burns and Mitchell in the 1940s that was attacked by Koopmans (1947) as "Measurement without Theory." One reading of the current methodological turn is its de-emphasis of theory, and in the conclusion of the chapter I take up the issue of whether this turn constitutes an embrace of pragmatism or simply naïve falsificationism. Inductivism is not necessarily pragmatism, but pragmatist considerations have resurfaced in research methodology across an array of otherwise differing theoretical tendencies. The gradual erosion of the narrow deductivist criterion for the generation of hypotheses has created a broadening of the acceptable criteria for hypothesis generation. There exist today an array of tendencies in economics – both within and outside the

mainstream – each of which claims as a strength its pragmatist methodology. I argue that it is not obvious which of these tendencies, if any, will become the mainstream of a future economics but that, in any case, economics may be headed for a more pluralist era, in which debates over the most appropriate form of pragmatism figure prominently.

Knowledge from mathematical generalization: the case of general equilibrium analysis

General equilibrium analysis has a long tradition in economics, dating back in some accounts to the writings of the Physocrats, exemplified by Francois Quesnay's "Tableau Economique" of 1758.² While the marginalist revolution of 1871 ushered in the utilitarian problem of the simultaneous satisfaction of given and heterogeneous, subjective individual preferences, the general equilibrium approach to the theory of prices and resource allocation did not become dominant among economists until after the Second World War.³ Its ascendancy in academic circles was spearheaded by Americans, most significantly, Kenneth Arrow, Paul Samuelson and Gerard Debreu.⁴ Interestingly, this rise of a strict hypothetico-deductive method followed an era of theoretical pluralism in which institutionalism had a prominent place. The rise of mathematical formalism and general equilibrium analysis in economics at this time has been attributed to the rise of scientism generally, the success of linear programming and operations research methods for the purposes of wartime planning, and the increasingly free-market ideological climate during the Cold War.⁵

Axiom-based hypothesis generation

General equilibrium analysis begins with a set of axioms describing the behavior of rational individual agents operating in perfectly competitive markets and with complete information, and derives the properties of a set of commodity prices and quantities such that all agents are optimizing their objective functions and all markets clear. Proof of the existence of such an equilibrium was of course a prerequisite to any attempt to prove the equilibrium's uniqueness and stability. But equally important was the welfare implication showing the optimality of the decentralized market system.

In the era of general equilibrium analysis, economic knowledge was understood to progress through repeated proofs of existence, uniqueness and stability of general equilibrium with appeal to successively weaker sets of assumptions. That is, an economic model was understood to generate new knowledge if it provided a proof of a known result, but required weaker, that is more general, assumptions than did existing proofs of that same result. For example, if the abiding proof of the existence of general equilibrium relied on concave utility functions, then a proof that assumed preferences to be quasi-concave constituted progress in knowledge. Similarly, the assumption of weakly transitive,

instead of transitive, individual preferences in a proof of existence or optimality is another example of how the robustness of the knowledge increased.

The great strength of this methodology was the clarity of its criterion for establishing the progress of knowledge – increased mathematical generality, or robustness, of its proofs.⁶ Given the general equilibrium school's equation of mathematical generality with theoretical progress, it is not surprising that the era of the dominance of general equilibrium analysis is often characterized by its emphasis on mathematical formalization. However, the era was not simply one of the greater use of mathematics in economic modeling, but one in which a particular mathematics – based on the axiomatic method – came to dominate the development of theory. According to E. Roy Weintraub: "mathematization" of economics is not quite the right description. ... It was the dominance of axiomatics, not rigor *per se*, that characterizes modern neoclassical economics. Applied economics is also mathematical and rigorous" (Weintraub 1998: xx)

The axiomatic method followed a narrowly construed, hypothetico-deductive approach to knowledge. Our description of knowledge and its progress in this era has, so far, not referred at all to empirical testing. This is because empirical evidence did not figure in the knowledge-generation process. Economics textbooks during this era continued to insist that knowledge in economics hinged on the testing (verification or falsification was usually not specified) of its deductively generated hypotheses. But academic economic research and journals did not consider such testing to be important. Mark Blaug (1980) critically referred to this era as one of "innocuous falsification," in which the generation of testable hypotheses was the prime role of economic research.

The demise of competitive general equilibrium analysis as the dominant modeling methodology resulted not directly from its failure to empirically test its implications, but from its aridity, that is, its insulation from institutional and historical detail. The insulation was conscious and was considered essential to the rigor of the project. In the preface to his 1959 monograph, *The Theory of Value: An Axiomatic Analysis of Economic Equilibrium*, which would later earn him a Nobel Prize, Gerard Debreu states that while the problem of general equilibrium had been approached by others in the past, they were insufficiently rigorous. He states:

The theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics. ... Allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations. (Debreu 1959: x)⁷

"Interpretations" would typically refer to how the model relates to the workings of actual market economies. The separation of "logic" from "interpretations" could be found in the early work of the marginalists, in

particular in Walras' distinction between "pure" and "applied" analysis, the former referring to the "natural science" of the determination of "value in exchange" and the latter relating to "the organization of industry under a system of the division of labor" (Walras 1954 [1871]: 76). For Walras, "given the pure theory of economics, it must precede applied economics" (Walras 1954 [1871]: 71). The distinction would not have been emphasized by the marginalists until the arrival of the American general equilibrium school. Arguably, the initial phase of acceptance of neoclassical economics was due to Alfred Marshall's insistence on merging theory and application into an economics for the common businessman. As Robert Heilbroner writes:

Marshall gives voice simultaneously to two approaches to economics. One of them is unquestionably that of marginalism, to which he made contributions of great importance. The second is a very un-, even anti-marginalist conception of economics as intrinsically sociological, or, to say the dreaded word, moral. Throughout the text the two approaches intertwine, not always for the best from the viewpoint of logical clarity, but unfailingly to the interest of those who see in the economy a subject that may have law-like "natural" attributes, but which also possess a core for which there is no counterpart in the world to which science directs its gaze.

(Heilbroner 1996: 228)

The intellectual crystallization and professional dominance of the Walrasian approach would not come until after the Second World War, and from American academics. Criticism of the general equilibrium project – Marshall's contributions were in the area of partial equilibrium analysis – would come soon after Arrow and Hahn's (1972) broadening of the concise presentation in Debreu's 1959 monograph. In a review essay of the Arrow and Hahn book, Alan Coddington (1975) compared the contribution of general equilibrium theory to the understanding of actual economies to "the contribution of fliminess to mountaineering." This sarcastic comment was a response to the standard defense of general equilibrium theory that it represented a benchmark of an ideal, fully decentralized, private enterprise system, useful as a standard against which actual and "imperfect" economies could be assessed. Coddington went on to criticize general equilibrium theory for being hermetically sealed off:

[W]e can see clearly the Procrustean temptations that are held out to the structure of general equilibrium: to consider the arguments not on their own merits, but only to the extent that they can be reformulated within the general equilibrium framework.

(Coddington 1975: 555)

Recently, historians of economic thought have attempted to explain the demise of this technically powerful paradigm. Blaug identifies misplaced rigor as part of the cause of the decline:

[T]he most rigorous solution of the existence problem by Arrow and Debreu turns general equilibrium theory into a mathematical puzzle, applied to a virtual economy that can be imagined but could not possibly exist, while the extremely relevant "stability problem" has never been solved either rigorously or sloppily. General equilibrium theory is simply a research program that has run into the sands.

(Blaug 2001: 160)

Colander (2000) notes that the demise of general equilibrium theory was its inability to lead to applied analysis. He writes:

In the 1950s and 1960s, it was hoped that practical models would be guided by general equilibrium theory. Thus, when Arrow/Debreu proved the existence of a general equilibrium in 1957, there was hope that the pure science of economics would progress in tandem with the practical application of that science. By the 1970s economists recognized that the Arrow/Debreu general equilibrium work was not going to get to the promised land.

(Colander 2000)

Colander notes that this disgruntlement with general equilibrium theory led to a new generation of economic models for which general equilibrium was not the core, guiding principle. He writes that the new generation of theory "freed economists to deal with practical policy models that were inconsistent with general equilibrium theory" (Colander 2000). The criticisms from Coddington, Blaug and Colander, an ideologically diverse but largely mainstream group of economists, are representative of the views of the profession as they evolved over the last quarter of the twentieth century.

Creeping inductivism and the "New Economics"

Poor predictive power has never led to the demise of a dominant paradigm in economics, and the reason for the professional decline in status of general equilibrium analysis must be found elsewhere.⁸ The answer seems to be at a stage prior to prediction, that of hypothesis generation itself – that is, in the "context of discovery" as opposed to the "context of justification." A new approach to economics began to develop in the late 1970s in response to a growing perception of irrelevance of the hypotheses generated by general equilibrium analysis. This development – what I will refer to in this chapter as the "New Economics" – arose in a series of sub-fields in the profession, including international economics, labor economics, industrial organization and macroeconomics.⁹

These new approaches had some common features across sub-fields, including an emphasis on imperfect market competition (rather than perfect competition), asymmetric information (rather than symmetric information),

on increasing returns to scale technology (rather than constant returns to scale), on strategic behavior by firms and governments (as opposed to optimization independent of rival behavior). While these new sets of assumptions are typically identified as the chief characteristics of the New Economics, methodologically speaking, the important shift was the move away from the strict hypothetico-deductivism of general equilibrium analysis and toward a vaguely construed inductivism. It became increasingly recognized that the questions being raised by general equilibrium theory were out of touch with "real world" concerns.¹⁰ I will show in the next section, using the example of the New International Economics, that the New Economics did not cause an abandonment of rational choice mathematical modeling (and in the case of New Keynesianism actually increased it – see footnote 9), but it constituted the beginning of a reversal of the direction of the relation between observation and hypothesis, that is in the accepted conventions for producing economic knowledge.

The New International Economics

The demise of the neoclassical general equilibrium model of international trade began over twenty years ago, partly in response to the model's failure to predict some commonly observed phenomena such as the large volume of intra-industry trade and the cases of successful government intervention along the lines of export-oriented industrial policy, especially in Japan and South Korea. In fact, the issue goes beyond predictive failure. These developments in the international economy in the 1970s were not imaginable given the Procrustean nature of the established, general equilibrium-based theory of international trade. Thus the established theory was of little use even in developing the hypotheses to test. By assuming deviations from the perfect competition, Pareto-optimal general equilibrium model – for example, by assuming the prevalence of imperfect competition, increasing returns to scale technology, strategic interaction among firms or even among governments – the "New International Economics" was able to logically derive the predictions mentioned above about intra-industry trade and the welfare-enhancing effects of "strategic trade policy."

An important feature of the new models was their flexibility: they could be used to "explain" almost anything. This was initially seen as a strength. Phenomena casually observed but inexplicable with the traditional model – Germany's simultaneous import and export of automobiles, Korea's export-generating system of subsidies and trade protections, subsidy competition between the US and EU governments in support of their respective aerospace industries – could now be rooted in the "rigorous" rationality of utility- and profit-maximizing microeconomic agents. In this sense it represented an inward turn of research: providing rational, microeconomic models of casually observed phenomenon was important mainly to the economists themselves.

"Outward" embrace of the theory proved a disaster. The models of increasing returns and imperfect competition showed that state intervention (e.g. subsidy or quota) in international trade or technology development could raise national welfare. Much to the horror of the new international economists, the theory was used as intellectual support by policy makers and political scientists making the case for interventionist trade policies. Such a policy stance, however, was at odds with the longstanding free-trade position of most economists, and some economists responded that because of its lack of robustness, the theory could not effectively be applied in reality.¹¹

This generation of economic models had another important feature. Because of the particular functional forms assumed on preferences and technology, proofs of Pareto optimality became mathematically intractable or structurally impossible. Welfare analysis focused increasingly on the capture of rents by national firms or governments, sometimes at the expense of other nations' firms or governments, representing a distinct break from the Paretian tradition. The rents resulted from the market power created by clever intervention that depended on the particular demand or technological conditions.¹²

The flip side of the flexibility was the growing sentiment that the models of the New Economics were *ad hoc* and could be used to model any outcome.¹³ This raised some important questions for the scope of research. In particular, what outcomes would be deemed acceptable? Or, put differently, from where should hypotheses come? Unlike the era of general equilibrium in which the name itself characterized the outcome under scrutiny, in the era of the New Economics, "rigorous" or "casual" observation could serve as the initial basis for a prediction. Sometimes the observation related to policy, and thus the model served to explain the rationale for the policy.¹⁴ Sometimes the outcome was based on an observation of a phenomenon that appeared to contradict standard theory.

With a wide range of outcomes to "predict," economic modelers were forced to adopt particular, as opposed to general, assumptions, and highly stylized model structures. This led to a sense of *ad hocery* in economic modeling. According to Colander (2000), "Modern applied microeconomics consists of a grab bag of models with a model for every purpose." For Colander, what distinguishes this era of economics is not its tendency toward formal modeling. He describes this era as one dominated by "applied policy models" as distinct from the "pure theory models" of the general equilibrium period. The shift is a change "in the nature of the modeling." The economists of the New Economics would be best described, he asserts, as "*Ad hoc* modelers, or eclectic modelers" (Colander 2000: xx).

Closely related to the ad hocery of the models was their distinct lack of robustness: because of the stylized mathematical structure of the models, a change in a single assumption often led to a completely different

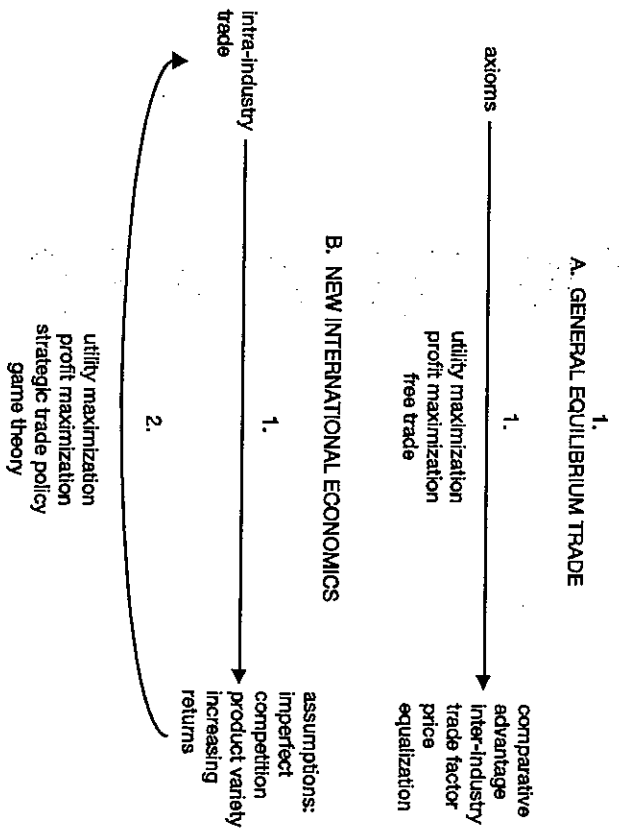


Figure 20.1

prediction,¹⁵ As I have argued, robustness, in the sense of mathematical generality, had been the hallmark of the general equilibrium, marginalist tradition. Progress of knowledge was synonymous with the increasing robustness of the hypothesis. In the New International Economics, by contrast, the hypothesis was often identified in an *ad hoc* manner and the model constructed precisely in order to generate the hypothesis. The distinction is depicted in Figure 20.1 which compares the relation between assumptions and predictions in the traditional general equilibrium approach to international trade (Heckscher-Ohlin-Samuelson) and the New International Economics. It shows how this relation was reversed: in the general equilibrium era, assumptions led to hypotheses. In the New Economics, the inductive basis of hypotheses created the need for particular assumptions in order to deductively reconstruct the original hypothesis.

The reversal of the relation between prediction and assumptions created the need for very particular assumptions on behavior, conjectural variations, or even functional forms. Consider the completely general functional form of the utility function assumed in the general equilibrium tradition:

$$U = U(x_1, x_2, x_3, \dots, x_n)$$

where $U =$ utility
 $x_i =$ good i and $i = 1, \dots, n$

Now consider the utility function that dominated theory in the New International Economics, the so-called Dixit-Stiglitz utility function:

$$U = (\sum x_i^\theta)^{1/\theta}$$

where U and x_i are defined as above and θ is a parameter reflecting the elasticity of substitution, that is the consumer's willingness to substitute one good for another in response to a change in their relative price.

The Dixit-Stiglitz utility function dominated the literature because it implies that utility rises with more product variety, even though it also implies that all goods are equally substitutable in consumption with all other goods. Paul Krugman (1979), in one of the early and pioneering works in the New International Economics, noted that "This is a restrictive functional form which appears to be necessary if the model is to have a steady-state equilibrium in Sec. III below." He goes on:

Something should also be said about the assumption that all goods enter demand symmetrically; this is clearly unrealistic: There is no reason why mopeds and toothbrushes should have identical demand functions. It also assumes away all differences in substitutability among goods, making all goods equally good substitutes for one another. The only justification for the assumption is its simplifying power which allows us to analyze economies producing many goods.

(Krugman 1979: 256)

It appears that in the New International Economics, mathematical tractability had replaced robustness as the criterion for the choice of functional form.

The *ad hocery* and unrobustness of the New Economics models was reinforced by the difficulty economists had in conducting empirical tests of their hypotheses. The peculiarities of the model specifications made the models difficult to operationalize in a standard multiple regression framework. Researchers turned instead to simple correlation analysis and simulations based on calibration to benchmark data. Neither of these techniques was sufficiently compelling to give a sense of broad empirical support for the models.¹⁶

One interpretation of the New Economics is that it represents a further generalization of the existing general equilibrium tradition, adding in considerations of market power, non-constant returns to scale and strategic behavior by firms and states.¹⁷ This is both wrong and an understatement of the methodological significance of the New Economics. Imperfect competition is not a generalization of the assumption of perfect competition and increasing returns to scale is not a general representation of technology that comprises constant returns to scale as a special case. To argue that the general equilibrium tradition and the New Economics tradition are methodologically

identical is to accept the symmetry thesis concerning prediction and explanation. Beyond the philosophical criticisms of the symmetry thesis (see Blaug 1980: 5–7), economists themselves have viewed the New Economics as a distinct break from the general equilibrium trajectory. Krugman (1983) and Greenwald and Stiglitz (1987) both argue that their work constitutes a distinct break from competitive equilibrium analysis of trade and macroeconomics, respectively – both in terms of model construction and hypothesized results. Krugman (1983: 346) offers that the new theory is “of some use for thinking about issues – including important policy issues – which cannot be handled by traditional theory.” Colander (2000) argues that the New Economics (what he calls the era of “applied policy models”) breaks so completely from the concern with general equilibrium that it should not even be characterized as “neoclassical economics.”

The move to empiricism: pragmatist turn or naïve falsificationism?

The result of the perceived ad hocery and unrobustness of the New Economics has been to discredit any claims its practitioners may have made for its hypothetico-deductive foundations and to gradually legitimate the inductivist pursuit of economic knowledge. The legitimacy crisis of the New Economics has opened a new era in mainstream economics. International economists, for example, may now ask simple, open-ended, experientially relevant questions, and apply sophisticated statistical techniques to answer them. For example: Does trade liberalization raise employment (Levinsohn 1999)? Does foreign direct investment raise wages in host developing countries (Aitken *et al.* 1996)? Does international outsourcing by US firms raise US wage inequality (Feenstra and Hanson 1997; Slaughter 2000)? Is democracy associated with higher wages (Rodrik 1999)? Utility and cost functions do not even appear in these works, and the specific empirical results provided as an answer to the research question posed do not necessarily “test” a particular theory. Sophisticated and careful variable construction and statistical analysis do appear, however, and these are research methods acceptable for publication in leading mainstream economics journals.¹⁸

The new, empiricist, wave of economic research carries the creeping inductivism identified in the New Economics to a new level. There is less insistence on rational choice microfoundations. Hypotheses are often rooted in simple economic logic, intuition, or current events, and emphasis is placed instead on the sophistication of the measurement of variables and the techniques used to show correlations between and among variables. The New Economics and general equilibrium theory shared a common “context of justification,” in particular an insistence on rational choice microfoundations. The recent empirical move constitutes a break from this methodological concern. However, the New Economics had already broken from the general equilibrium tradition in its “context of discovery” and the empirical turn

described here grew as an extension of this tendency that began with the New Economics. Both the New Economics and the recent empiricism are defined by broader criteria for the generation of relevant hypotheses, including completely *a posteriori* observation.¹⁹ Some economists appeal to very simple supply and demand theory but casual empiricism, common sense and introspection – rather than axiomatics – have also motivated the recent research.²⁰ In sum, there appears to be a further loosening of the already loose set of criteria for hypothesis generation. A new set of rules, a new context of discovery, is forming, with a clear, if unconscious, pragmatist bent. Before we assess whether the new trend is pragmatist or simply naïve falsificationism, we consider briefly some examples of recent research that characterizes the new trend.

Gordon Hanson on subsidies and foreign investment

An important research paper by Gordon Hanson, Professor of Economics at the University of Michigan, on the question of whether or not government subsidies, in the form of tax breaks or subsidized credit, are effective in inducing transnational corporations to invest, relies heavily on case studies for its evidence and conclusions (Hanson 2000). The question is motivated by the observation that subsidies are commonly used to attract foreign capital – their prominence is central to the debate over a possible “race to the bottom” that they may spur. Hanson does rely on a formal microeconomic model to develop a list of conditions under which subsidies to foreign direct investment are warranted. But it is on the basis of three case studies of actual subsidies – for Ford Motors and General Motors in Brazil and Intel in Costa Rica – that Hanson concludes that “there is little basis for subsidizing foreign direct investment.” The source of information for the case studies is mainly industry trade journals, newspaper and magazine accounts, and published on-site accounts. While data on subsidies are presented for descriptive purposes, the analysis relies on no statistics to come to its unambiguous and bold conclusion.

Dani Rodrik on democracy and wages

Dani Rodrik of Harvard University is one of the leading development economists today. His 1999 article, “Democracies pay higher wages” in the *Quarterly Journal of Economics* – one of the most prestigious economics journals in the world – takes up the question of how the level of political freedom in a country relates to its wage levels. This grand question is motivated by the observation that labor productivity – the standard predictor of wage levels – deviates from wages to a much greater extent in Mexico than in the United States. If productivity is not a good predictor of wages, then, Rodrik proposes, “it is possible that the political context in which labor markets operate shapes behavior in these markets and influences wage outcomes” (1999: 708). The

second section of the paper, following the introduction, is "Data Sources." Not only does the paper not contain a mathematical model – microeconomic or macroeconomic – developing the idea, the hypothesis in the paper is not linked in any but the loosest way to traditional economic theory. Only after the statistical correlation between democracy and wages is established does Rodrik ask "Why does democracy matter to wages?" He sets up a simple (one equation) game theoretic framework, but provides only "the intuitive solution" (Rodrik 1999: 727), and turns quickly to regression analysis of wages with political competition and participation as independent variables – an exercise largely delinked from the game theory intuition. Rodrik concludes that "it would be desirable to sort out some of the causality issues in greater detail. Is there perhaps a two-way relationship between wages and democracy?"

Alan Krueger on class size and student performance

Alan Krueger of Princeton University has produced a series of highly regarded research papers in which formal economic models play almost no role. In 1995 he co-authored a book entitled *Myth and Measurement: The New Economics of the Minimum Wage* (Card and Krueger 1995), which provided a highly detailed test of the relation between the minimum wage and employment based on evidence from the fast food industry in New Jersey. The book contains enormous detail on the statistical relation between these two variables, but the theoretical model tested is the same supply and demand diagram shown in all first-year economics textbooks. And the authors' intent is to show the irrelevance of that diagram, not to provide support for it. Only in the last chapter do the authors seek a theoretical explanation for the 354 pages of evidence preceding this chapter that refute the received view. As with Rodrik's work discussed earlier, the hypotheses offered at the end are heavily influenced by the authors' understanding of the evidence from New Jersey's fast food industry.

More recently, Krueger has produced two papers – one in the *Quarterly Journal of Economics* and one a working paper of the National Bureau of Economic Research – on the question of whether class size matters to student performance (Krueger 1999; Krueger and Whitmore 2000). While the question implies an attempt to determine the conditions for maximum student performance, the hypothesis is not generated with appeal to utility-maximizing agents, or by any formal economic model. Instead, Krueger is interested in testing the commonly held view that smaller class size allows teachers to give more attention to each student, thus raising student performance. Beyond this intuition, there is no prior economic theory about the relation between class size and student performance. The work is also motivated by the availability of a large database on an experiment with various class sizes for kindergarten through third grade performed in Tennessee between 1985 and 1989. In fact, the main claim for the originality of the

research is the "well-designed experiment" (Krueger 1999: 528) that produced the data set and the statistical techniques used to capture the benefits of smaller classes. There is almost no attempt, beyond the intuition described above, to explain the relation between class size and student performance. Krueger (1999: 529) concludes that "more research is needed to develop an appropriate model of student learning."

Krueger's work on class size and student performance also gives a new twist to the fears of economists as "imperialism" in the social sciences.²¹ This imperialist tendency is often attributed to the adoption of a methodological individualist rational choice model for the study of phenomena outside the standard scope of economic inquiry – Gary Becker's study of marriage being the classic example. It now appears that economists' imperialism may result instead from its pragmatism, that is its willingness to ask intuitively appealing questions and apply sophisticated statistical analysis to answer those questions.

"Measurement without theory" or revival of pragmatism?

The recent empiricist turn is reminiscent of the work by Burns and Mitchell that was criticized by Koopmans (1947) in his paper "Measurement without Theory." Burns and Mitchell's work on business cycles has been understood as heavily influenced, as was much of American institutional economics, by pragmatist considerations of description and explanation rather than prediction and falsification. The recent empirical turn, while not consciously pragmatist, shares some features of the Burns and Mitchell research project, including broad attempts to find regularities, and careful empirical filling out of well-developed accounting or taxonomic schemes.²² Empirical research skills and computing techniques have improved considerably since the 1940s, but there is also heightened awareness of the limitations of regression analysis and thus greater acceptance of other types of empirical evidence, including case studies. Despite advances in times series analysis (with the advent of cointegration analysis) and a retreat from unfounded claims of causality (with the common adoption of vector autoregression techniques), economists have still not overcome the sense, among both economists and non-economists, that regression results are simply not compelling enough to settle an argument.²³ In particular, economists have not resolved the issue of "data mining," in which only selective empirical tests are performed or presented. A debate over the merits and demerits of data mining has clarified some issues, but there has been no broad agreement on how to remove the sense of "con" in econometrics, as Leamer (1983) put it in his widely-read article. Still, multivariate regression analysis remains the main statistical technique used in published research.

Does the new trend in mainstream research constitute a pragmatist turn or simply a naïve empiricism? Pragmatism would indicate a deeper inductivism than currently practiced in today's mainstream empirical research. But the recent research has, unconsciously perhaps, embraced the pragmatist notion

that observation is central to hypothesis formation. A few researchers have begun to conceive the observation process itself more broadly.²⁴ Peircean abduction more aptly describes the hypothesis generation process in some contemporary work than simple deduction or induction. Dewey's logic, according to Hirsch (1998: 101):

encourages extensive observation, using whatever theoretical frameworks are available or seem to be called for by the observations. Such extensive observation is considered desirable in this way of thinking because Dewey encouraged reasoning from observed facts to formulated theoretical concepts.

The new research constitutes a clear break from neoclassicism in its general equilibrium form and, I have argued, even from its New Economics variant. In the neoclassical tradition, observation plays little role in formulating economic questions because the scope of such questions has been narrowly molded in the confines of a mathematical model of rational individual choice. Silberberg (1982), for example, defines "economic questions" as those that can be addressed with the techniques of marginalism. While the New Economics was concerned with the *ex post* construction of rational individual choice theoretic foundations,²⁵ much of the recent mainstream work I have cited makes no appeal to a formal mathematical model but moves quickly into sophisticated measurement and statistical analysis. It is not obvious that this new trend will continue, much less grow to dominate the mainstream. Still, its emergence at this time is neither an accident, nor an isolated move, as we will see in the next, and concluding, section.

The coming fight for the mantle of pragmatism

In fact, the tendency to pragmatism in mainstream economics comes at a time when a number of other schools of thought claim pragmatism as their philosophical foundation.²⁶ Some groups, including Friedmanian monetarists and American institutionalists, have long identified themselves as the representatives of pragmatist thought within the field of economics. Others, including feminist economists and complexity theorists, are relative newcomers on the scene. In this concluding section, I briefly review each of these views of a pragmatist economics and then speculate on the outcome of this sudden coincidence of multiple, self-proclaimed pragmatist approaches to economics.²⁷

Institutional economics was born out of a commitment to pragmatism by its founders, Veblen, Ayres and Commons. Institutionalism dominated the American economics profession in the early twentieth century (including the formation of the American Economics Association) and fell out of favor with the rise of neoclassicism after the Second World War. But institutionalism has seen a resurgence of late, inspired in part by a growing mainstream search to

understand economic evolution and institutional change and by an embrace by European economists of the ideas of institutionalism.

American institutionalists, perhaps justifiably, have long claimed to be the true practitioners of pragmatism in economics. Veblen, for example, wrote passionately on the incompatibility of pragmatism with the individual rational choice methodology. Others argue that the mainstream also includes research that is heavily influenced by pragmatism, especially the work of Milton Friedman and other old Chicago school economists.²⁸ Hammond (1995: 34), for example, identifies Friedman with the well-grounded theoretical school of Alfred Marshall rather than the hypothetico-deductive tradition of general equilibrium marginalism. And Hirsch and DeMarchi (1989) explain at length the pragmatist foundations of Friedman's work. They argue that Friedman's greatest contributions (to economics and to pragmatism) come from his adherence to the Deweyan principle of working back "from observed regularities taken to be implications to hypothetical premises." They continue:

What we learn from Friedman's working experience is that many of the hypotheses that suggest themselves in the process of inquiry involve unobservables – like permanent income and utility functions – and that it is difficult to make the connection between the hypothesized premises and observed implications and thereby derive theory whose implications can be meaningfully tested with further data.

(Hirsch and DeMarchi 1989: 147)

Institutionalism and old Chicago neoclassicism have both resurfaced and claimed the pragmatist mantle. But two theoretical newcomers have also made their claims. Nelson (2003) argues that feminist economics requires a pragmatist approach to knowledge, a "feminist-process view" of understanding, which involves an alternative ontology that would remove the science/value split and call on new empirical methods involving experience. In this view, "reality" is not objectively given or even fixed, but a function of subjective experience. Knowledge, in this case, embodies values as well as facts. Nelson writes:

In a universe conceived as open, the question of knowledge must be reframed. Our knowledge is not just about reality in process thought. Rather, it creatively adds to reality. ... Values and morals are of the same fabric as science and economics; not merely incidental. ... The feminist-process view ... sees the world, including the economic world, as unfinished and evolving, and sees knowledge adding to that world, for better or for worse. Science is, thus, intrinsically a matter of value.

(Nelson 2003: 11, 24)

Feminist economics, then, draws on a pragmatist conception of knowledge. At the same time, a feminist perspective also provides an explanation for why pragmatism, process-oriented knowledge and institutionalism have been so widely dismissed: they are "in conflict with ideals that have been historically, socially, and psycho-sexually associated with masculinity" (Nelson 2003: 2).

Finally, the other emerging school of thought that seems influenced by pragmatism is so-called complexity theory, which gets its name precisely because of its resistance to techniques that give simple closure to economic models and embrace instead nonlinearities and path dependencies that potentially create explosive or chaotic outcomes. Complexity thinking, Nelson argues, shares feminism's resistance to reductionism. Thus, she points out, their coincidental rise in economics is of note. Colander (1999, 2000) sees complexity-based economics as the next dominant paradigm. In an amusing and insightful article written from the perspective of the year 2050 and describing the evolution of economic thought to that year, Colander (1999: 6) writes that around the turn of the millennium economists saw "the fall of loose-fitting positivism and the rise of pragmatism." Colander writes:

In 2050, the belief of economists in derived analytical models has given way to a belief that underlying reality is too complex to be understood with these sorts of models. [I]n New Millennium economics, "proofs" in economics rely much more heavily on empirically determined economic patterns that have developed through simulation work, experimental work and economic modelling built on generally accepted observed patterns... Economic models will be "grown" from observations, rather than from assumptions.

(Colander 1999: 9, 11)

Hirsch (1998) proposes that "Dewey can help us ... to formulate the rationale of aposteriorism in a way which makes sense even to those who are not disciples of aposteriorism." Hirsch's faith in methodological discussion in economics is impressive since I can think of no pure methodological argument that has ever brought a significant change in economic thinking. Moreover, the writings of classic pragmatism provide no single set of instructions on what constitutes pragmatist economic research. Bernstein (2001) notes that "pragmatism provides no blueprint for how to do social science."

It is unclear which of these pragmatist tendencies will come to dominate economic research in the future, or if a new era of pluralism will emerge. The new mainstream economic research is quite compatible with most of the other pragmatist tendencies and it is not hard to imagine that pragmatism will serve as a loose methodological core for varieties of economic research.²⁹ In any case, it is clear that an opening has been created and, consciously or not, a pragmatist sensibility is likely to be influential in molding the future of economic thought.

Notes

- 1 I am grateful to Elias Khalil, Michael Lawlor, Ahmet Tonak, and James Webb for comments on the first draft and to Luca Fiorito, Duncan Foley, Stephen Gelb, Wade Hands, Geoff Hodgson, Ann Mayhew, and Michael Piore for general discussion of the issues addressed here. All remaining errors are mine.
- 2 See Walsh and Graham 1980.
- 3 My colleague Robert Heilbroner recalls that as an undergraduate economics major at Harvard in the mid-1930s, he was assigned the textbook by Alfred Marshall—the 1898 *Principles of Economics*, best known for its detailed partial equilibrium analysis of markets. Utilitarian at its core, Marshall's book makes no more than one or two passing references to the work of the earlier, general equilibrium, marginalists.
- 4 Debreu 1959; Arrow and Debreu 1954; Samuelson 1965.
- 5 Morgan and Rutherford's 1998 volume, *From Intervention Pluralism to Postwar Neoliberalism*, contains a superb collection of essays exploring this subject. Mirowski (2002) focuses on the role of computerization, both as a method for research and increasingly as a model of rationality. Spiegel (1994) attributes the mathematization to the multi-ethnicity of American culture at the time. In Milberg (2001a), I argue that the post-war appeal of neoclassical general equilibrium theory resulted from its allegorical role as an ideal of ethnic assimilation.
- 6 This criterion of the generality of a proof was slightly different from that evoked by Keynes in his *General Theory* of 1936. Keynes claimed theoretical superiority not because his more general set of assumptions gave the same result as did previous theories. He argued that his distinct theory gave a more general result, that is, in which the previously held result was a special case.
- 7 Weintraub and Mirowski (1994) trace this tendency to extreme abstraction to the influence of Bourbakiism.
- 8 Here I am referring not to prediction of the future, but of any "if, then" hypothesized relation.
- 9 On the New International Economics, see the next section. Industrial organization theory was transformed more narrowly along the lines of game theory although it has also been influenced by the "complexity" school. And labor economics already had a strong inductivist tradition, although now even its mainstream has moved in this direction. The New Keynesian macroeconomics is typically understood as a response not to the failure of general equilibrium theory but to the lack of rational choice microfoundations in Keynesianism. I would argue that while this interpretation is valid, the New Keynesian economics fits the methodological pattern of the other sub-fields in terms of the shift within the context of discovery.
- 10 Colander (2000) writes that "shedding some light on a problem is all that the practical track of modern economics requires." Robert Solow writes that "model-building economists tend to be natural-born, loose-fitting positivists." See Solow (1997: 50).
- 11 On the "inward turn," see Heilbroner and Milberg (1995). For the debate over "international competitiveness" in *Foreign Affairs*, see Krugman (1994), and subsequent responses in that journal as well as my discussion in Milberg (1996).
- 12 See Milberg (2001b).
- 13 Greenwald and Stiglitz (1987: 131), for example, write that the choice of model assumptions "must be dictated by the phenomenon to be studied."
- 14 Thus Colander (2000) refers to this method as rooted in "data mining."
- 15 In fact this feature was one reason given by the New International Economics theorists themselves to oppose the actual adoption of the policy conclusions of the models. The other reason given was that governments were not sufficiently

- precise to be able to implement the (theoretically) welfare improving interventions. See Milberg (1996) for an overview.
- 16 The diversity of approaches in the essays in Feenstra 1988 reflects some of these difficulties.
- 17 I am grateful to Michael Piore for raising this as a possible interpretation of the New Economics.
- 18 Stephen Gelb has pointed out in discussion that at the same time that there has been a decline in the status of hypothetico-deductivism, there have occurred heightened debates over the measurement of basic economic variables (e.g. GDP growth, consumer price inflation) and of the notion of causality underlying multiple regression techniques (i.e. with the rise in use of vector autoregression).
- 19 With the introduction of non-convexities in functional forms in the New Economics, there was less ability to generate traditional welfare results based on Pareto optimality. This quiet disappearance of the traditional criteria for social welfare has led to the adoption of intuitively appealingly proxies for welfare as outcome variables (as opposed to axiomatically given behavior in general equilibrium and some New Economics analysis), including wages, productivity and skills attainment, with only a tacit understanding of their role as proxy for welfare. This tendency has created a parallel looseness in the criteria for choice of outcome variable. According to Wade Hands, this is reminiscent of the work on business cycles by Geoffrey Moore and others in the 1940s, in which it was unclear which variables should be the focus of study.
- 20 See, for example, Card and Krueger (1995), regarding employment effects of changes in the minimum wage; Feenstra (1998); Levinsohn (1999), regarding the effects of trade on relative labor demand.
- 21 For a discussion, see Harcourt (1978).
- 22 For example, see Rodrik (1999) on investment and economic development, on the relative importance of forces driving trade growth or Slaughter (2000) on trade and wage shares.
- 23 Mitrowski and Sliwias (1991) show the enormous variation across estimates of economic "constants." The point about the effectiveness of econometrics in setting a debate is made eloquently by Summers (1991) who, interestingly, cites the simple time series data presentation in Friedman and Schwartz's *Monetary History of the United States* – now viewed by some as a classic pragmatist work in the field – as the most compelling form of empirical argument.
- 24 NBER Working papers now include a number of case studies and of course experimental economics has attracted attention from mainstream theorists.
- 25 Krugman (1995) goes further, arguing that knowledge does not exist in economics until it is established within a mathematical model.
- 26 Renewed interest in pragmatism goes beyond economics. The Dewey conference is testament to the renewed interest in pragmatism among philosophers. This extends to law and cultural studies as well. In these fields it is seen as a potential middle ground between modernist foundationalism and postmodernist relativism. Hands (2001) makes this case for pragmatism in the field of economic methodology. I have argued here that in economics the motivation is different, although there may be an analogy if the New Economics is read as a postmodern moment in economic thought.
- 27 In fact, economic methodologists seem in disagreement over the relation between pragmatism and the hypothetico-deductive methodology that defines general equilibrium analysis. Blaug (1980: 2), on the one hand, identifies American pragmatism (along with the Vienna Circle) as important to the creation of this model of scientific explanation. Hands (1998: 377), on the other hand, identifies pragmatism in American institutionalism as one of the few alternative "philosophical visions" to positivism in the history of economics. Both are no doubt correct

- since pragmatism has had multiple and even contradictory influences. See, for example, the discussion of Peirce versus Dewey in Mounce 1997.
- 28 Those seeking to rehabilitate Milton Friedman based on his pragmatist methodology include Hirsch and DeMarchi (1989) and Hammond (1995). Even McCloskey's rhetorical interpretation of economics has been associated (by McCloskey and others) with the pragmatism of Friedman, Stigler, Knight and others in the old Chicago school.
- 29 Similarly in the field of economic methodology, Hands (2001) sees the opening of a new eclectic era in which the "Received View" of hypothetico-deductivism no longer serves as a criterion for theory appraisal, but is replaced instead by a looser and more diverse set of methodological tools.

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