Pluralism, Formalism and American Economics

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

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

Recently there have been a number of considerations of formalism, pluralism, and their relationship to the evolution of economic thought over the last 100 years.[1] In Morgan and Rutherford there seems to be a sense that formalism is bad and that nonformalism is inherently pluralistic and good, and that, in an ideal pluralistically committed world, being out of step with the mainstream should be a strength. In a profession devoted to a pluralist methodology, researchers would turn to those who are out of step for applicable solutions, because the insights one might gain from them would likely be higher than from other sources. Leland Yeager certainly would be considered pluralistic and open; in his work he has demonstrated a willingness to give every view consideration, and he has always dealt seriously with those that he felt met his standard of insightfulness, regardless of whether they advanced an "in" theory or not.

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[*] An earlier version of this paper was presented at a History of Political Economy conference at Duke, April, 1997.

[1] For other views of the reorientation of economics, mostly complementary and compatible with ours, see Blaug, (1998, 2002, 2003); Niehans (1990); and Samuels (1998). Quite different conclusions are reached by Morgan and Rutherford (1998), and Yonay (1998). While this paper focuses on the changes that took place in American economics during the 20th century, the importance of American graduate education in economics strongly suggests important ramifications for the development of non-American economic thought.

He follows a self-described libertine approach to methodology.[2] His argumentation demands rigor but is almost impervious to ideological positions—he criticizes mainstream, Austrian, and radical economists with equal vigor.

Yeager is in a small minority in following this pluralistic approach on either side of the formalist/nonformalist divide. Commitment to a pluralistic approach it is not a characteristic of the profession--and, in our view, his commitment to pluralism has played an important role in reducing his work's influence.[3] Our argument is that a pluralist methodology, such as that practiced by Leland, and that supported by Morgan and Rutherford, is not a systemically stable methodology. This presents a problem for researchers committed to a pluralist methodology: How does one exist in a world that is not committed to pluralism? We see this question as a Yeageresque question; for Leland there is no ideal world, there is only the world we live in. And in this world the periods of pluralism that we observe generally have not come about because researchers have made a commitment to pluralism, but instead because various opposing methodological groups have found themselves of roughly equal strength. The reality is that if you fall outside the methodological mainstream of your time, your work will get less consideration than it otherwise would. It follows that, other things being equal, methodological libertines such as Yeager will have less success than methodologically committed individuals. Our argument is not that this situation is good -- in this paper we take Yeager's commitment to what is, rather than to what should be, seriously, and simply say that this is the way it is: a commitment to pluralism is not an evolutionarily stable strategy.

We raise these issues because they relate to how one might understand the history of the profession over the past century. Specifically, Morgan and Rutherford, having considered that history, have described how the formalist revolution wiped out the pluralism that existed in the early 1900s. In their story what they term neoclassical economics overcame a pluralistic institutionalist approach here in the U.S., with the result that modern economics is far less pluralistic than it was earlier.[4] They seem to lament both the formalization of economics and the loss of pluralism that occurred in the interwar period. We find that story unsatisfying. We see the pluralism that existed then as a byproduct of other forces. It was simply a temporary part of a dynamic process in which the formalist and nonformalist methodological positions were of somewhat equal strength. None of the players in the interwar period was so dominant that others were excluded from academic appointments at important graduate programs, from space in the major journals, from representation in the power structure of the American Economics Association, or from research support. As we will show, our approach provides a different view of the formalist revolution over the last 100 years from that found in previous studies. Ours is a process-oriented view of the profession in which ideas compete given the institutional realities of the profession. Those that succeed are those that meet the institutional requirements of survival.

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[2] As he points out, by this he does not meant that "anything goes, or that whatever one comes up with is automatically valid." He is simply saying that one should "let people work with whatever method works for them, and fits with their talents and inclinations." Yeager (1988)

[3] Our argument is not that he was wrong in holding his views; only that holding those views reduced his influence.

[4] There are many dimensions of pluralism. There can be pluralism in policy proposals, where the profession comes to multiple mainstream positions on policy. We have not seen a significant Post World War II decline in policy pluralism. Where we believe there has been a decline in pluralism is in methodological pluralism. There is less diversity of approach today than there was in the 1930s. It is that aspect of pluralism that we focus on in this paper.

The "truth" or "appropriateness" of the idea is only one of many deciding factors of the success of an idea.[5]

The alternative story we tell is one in which pluralism has occurred by default, as the profession has swung from a nonformalist to formalist methodology, as one side or the other gained prominence while holding an unpluralistic methodology. Formalism and nonformalism are both disequilibrium situations which, over the broad course of the history of economics, have swung like a pendulum from one side to another and will likely continue to swing indefinitely in the future. Given this pendulistic swing, our argument is that, when viewed in its historical context, the last 100 years is best seen not as a movement away from pluralism, but simply as part of the swing of the pendulum.

In our "process" view, a pluralist methodology in which individuals are actively committed to pluralism has seldom been the nature of the equilibrium; it is simply a state in the evolutionary process in which competing sides are of relatively equal strength. Thus, in our view the unpluralistic formalism that emerged in the latter half of the 20th century was a temporary state, one that, in our view, is already changing. Today the formalism of that period is combining with the informal work of earlier times, creating a new type of economics that is inductive, highly mathematical, and institutional.[6] This paper, however, is concerned with the ascendancy of pure formalism, not its current demise, although we will briefly discuss that demise in our concluding comments.

The Swinging Pendulum

The ongoing debate between formalists and nonformalists can be seen in the approaches of the major economists of classical economics. Smith was a non-formalist, Ricardo a formalist. Mill moderated Ricardo's formalism, while post-Millian economists diverged as to which track to take. In the late 1800s the battle between the two approaches peaked in the famous Methodenstreit that pitted the German historical school against the newly emerging marginalists. This Methodenstreit set the backdrop for the rise of the American economics profession, and, with that rise, the shift of the center of world economics from Europe to the United States.

At the beginning of the 20th century, the debate considered by Morgan and Rutherford was between the institutionalist non-formalists and the neoclassical formalists who incorporated the newly emerging marginalist ideas as the centerpiece of their approach to economics. The initial debate, however, was nowhere near as stark as it might have been, because at the time the primary standard-bearers of the formalist views were, in large part, Marshallians. From a formalist perspective, this period was hardly pluralistic. In fact, as Blaug notes in 1930, "it is doubtful that there were more than a half-dozen economists in the world who had ever read Walras, much less understood him." (Blaug 2003, page 150)

Marshall's approach to economics was itself a compromise approach, using formalist techniques but then moderating them with history and institutions at every point. Marshall's approach was essentially a straddle between the German historical school and the marginalist formalists. Thus, contrary to what is implied in the Morgan and Rutherford volume, from the

[5] For a further development of this idea, see Colander, (1991).
[6] This view of economics is developed in Colander (forthcoming) and Colander, Holt, and Rosser (forthcoming).

perspective of a formalist, the 1930s were hardly pluralistic. What would at that time be called the super-formalists, such as Edgeworth and Walras, were in a small minority in the U.S. during the interwar period.[7]

Why this History is Important

The long history of battles between the two sides is important because of the perspective it adds to the transformation of economics that has occurred since the 1930s. It strongly suggests that whatever pluralism existed in the interwar period was a tenuous pluralism existing because neither side had eliminated the other, not a pluralism grounded in pluralistic methodological foundations. The history of the development of the economics profession in the U.S. is one that abounds with intrigue, hostility, and warfare between advocates of the different views. (Barber 1988) Given this lack of a pluralistic methodological foundation, the transformation away from pluralism that occurred in the Post World War II era is about as surprising as the tipping over of a coin standing on its edge. The relevant question is not: Why did the coin tip? It is: Why did the coin land on the side that it did? Specifically, why did the superformalism become the center of the American economics profession?

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

The Players

Let us begin by briefly considering who the players were in the early 1900s and in the interwar period. Those players can be divided into three loose groups that represented divisions similar to those that existed in Europe at the time. The largest group was the institutionalists. This group represented the German and English historical-institutional approach to economics as

[7] Since this debate between the formalists and the non-formalists plays such a central role in the transformation of American economics, it needs to be clarified. It is not a debate between those who favor mathematics and those who don't. It is a debate about the worldview that individuals have concerning the complexity of the economy, and the usefulness of formalizing discussions of the economy with the mathematical tools that exist at the time. Non-formalists believe that the mathematical tools available at the time are insufficient to capture the complexity of the economy, whereas formalists believe that those tools are sufficient.

What this means is that as mathematical tools change, people's view of the usefulness of a formal approach may change. For example, with the recent developments in math such of chaos and catastrophe theory, and with the increase in the ability of computers to handle difficult problems, views of whether formalism is useful can be quite different today than they were in the 1930s when the tools involved relatively simple differential calculus, and almost no developed statistical analysis.

a discipline and contained a number of German trained PhDs. However, the principal intellectual force in this group came from American-trained Veblen, Commons, and Mitchell.

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

The third group was a swing group between the two. It probably best goes under the name Marshallian, because its methodology and approach closely followed Alfred Marshall. Marshall had masterfully built an economic engine of analysis that tried to straddle the institutionalist and pure formalist schools. It argued for a type of pluralism in which no rigid lines were drawn on almost any issue of scope, method, or content, and all were welcome under the big tent.[8]

In this development a distinct Austrian school did not exist; it was simply part of the Marshallianism that characterized the period. By 1900, the beginning of the time frame we are mostly concerned with, the existing main contributions of those who later became called "Austrians," in the minds of most economists of the time, had already been incorporated into the Marshallian views of the time, views that came to be called neoclassical economics.[9]

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

In the early part of the 20th century, institutionalists were the most powerful group. Thus, the first part of the story is their loss of power. That loss was in many ways due to the institutionalists' failure to meet the institutional requirements of an ongoing research program within the economics profession's institutional structure. To see this we need to look more closely at the three groups of institutionalists who, though never united in a coherent research program, came to be linked to one another primarily by their opposition to theory, whether it be formalist, or Marshallian. Thus the glue that held institutionalists together was not a positive glue, but a negative glue.

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

> The psychological and anthropological preconceptions of the economists have been those that were accepted by the psychological and social sciences some generations ago. The hedonistic conception of man is that of a lightning calculator of pleasure and pains, who oscillates like a homogeneous globule of desire of happiness under the impulse of stimuli that shift him about the area, but leave him intact. He has neither antecedent nor consequent. (Veblen, 1919, pp. 73-74)

[8] We see Leland's methodology as similar to Marshall's. In many ways Leland was the consummate Marshallian straddler.

[9] Austrian economics, developed as a separate school only later in the 1970s as a group of economists worked hard to organize themselves into a separate school.

Wesley Claire Mitchell, in a letter to J. M. Clarke, made even more biting comments about the formalists. In explaining why he could not take neoclassical theory seriously, he compares the grand theorist to a great-aunt with whom he argued when he was young. In arguing with that great-aunt, who "was the best of the Baptists, and knew exactly how the Lord had planned the world," he found when he presented her with logical difficulties that her simple scheme could not handle, she always "slipped back into the logical scheme, and blinked the facts," just as the grand theorists do. For Mitchell, developing grand theories was child's play. He states, "Give me premises and I would spin speculations by the yard." (Mitchell as cited in Clarke (1936), pp. 410-411)

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

The Veblenese part of institutionalism was, in large part, unique to Veblen. Mitchell rejected it, and while almost all will agree that Veblen's approach was highly insightful, it offered little that ordinary students could build upon. Veblen's approach was carried on largely in the work of Clarence Ayres and his students. In retrospect it appears to have been a non-viable research program, with PhDs receiving training in what was wrong with Marshall and more formal economics but with few tools to bring to a positive research agenda. The Ayresians never were able to gain editorial control of a major economics journal, and they often squabbled with editors of journals publishing in the historical-institutional tradition. The criticism that the Ayresians had no analytical framework or research program led Ayres to write "The Coordinates of Institutionalism," which had little impact on the profession. Veblenian-Ayresian institutionalism was fading in post Word War II America.[10]

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

[10] As an example, consider the path of one of the authors. He, together with three other Texas economics PhD candidates from the University of Texas at Austin, transferred to the PhD program at Harvard during the middle 1950s.

they developed into a separate group: their basic framework was so different from that of the formalists that they could not communicate with them.[11]

What happened to the Commons-Wisconsin part of the historical-institutional camp is complex and subtle.[12] Here was a progressive research program with possibly an element more important than the tools: a view that government and intellectuals should work together to help solve some of the social problems created by the industrial society. The union between the state government at Madison and the academicians produced a long list of social legislation. The depression of the 1930s found a cadre of academicians ready to go to Washington D. C. to apply the Wisconsin model of government-academy cooperation. It is not by chance that one of the foremost advocates of Keynesianism in the United States, Alvin Hansen, was a Wisconsin PhD who brought the Wisconsin model to Harvard in its fiscal policy seminar and began a Harvard-Washington D. C. nexus which remains today.

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

The Institutional Cause of Institutionalists' Demise

The link between the demise of the three brands of institutionalism was the failure of each to meet the institutional requirements for survival. Institutionalist economists were seen as anti-theoretical and anti-mathematical. Neoclassical economists were seen as theoretical. Mathematical neoclassical economists portrayed economics as a predictive science that involved specifying a theory and empirically testing that theory. Such a method created large numbers of small jobs, enough to keep an academic neoclassical army of students busy. Institutionalism, however, presented economics as a policy-driven combination of the study of institutions and of empirical facts about the economy, neither of which required a formal theory or definitive -- and labor intensive -- empirical testing. Given those choices, it is quite clear which view would succeed institutionally -- and it was not the institutionalist view.

Whether one believes that a grand theory is true in some fundamental sense is irrelevant. Even if you do not *believe* a theory, it can still be useful in the metaphysical sense of organizing

[11] This doesn't mean that their ideas weren't correct or better than the emerging ideas; it simply means their ideas no longer were compatible with the institutional structure of the emerging shape of the economics profession's institutions.

[12] Lampman's *Economists at Wisconsin 1892-1992* (1993) may trigger research that will produce clearer insights into what happened to the Wisconsin school.

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

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

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

What we are arguing is that having a branch of economics working on a formal grand theory was a requirement of survival in the U.S. educational environment. Lacking a grand theory reducible to simple textbook models, the institutionalists' complex economic worldview was incompatible with the pedagogical institutions through which economic ideas were propagated. Their decision simply to not discuss formal theorizing rendered them incapable of competing in the metaphysical grand theory realm; whereas the neoclassical worldview *succeeded* in providing a system whereby students could organize their thinking about the economy. Once the simplicity of that worldview was built in, moreover, it was not questioned, and it soon became the norm by which economists approached their work. Little consideration was afforded the implications of the institutionalists' complexity leap of faith, while more and more elaborate theorizing was developed on the simplicity leap of faith.

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

Thus, institutionalism failed institutionally, and its demise was sped up by the enormous growth of universities, requiring large numbers of new PhDs during the Post World War II era.

The Victory of Formalists over Marshallians

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

To understand why this second transformation occurred, we need to look more carefully at Marshallian economics. One can view Marshall's economics as an attempt to prevent either side of the long-continuing battle between formalists and non-formalists from winning. Marshall argues that what is needed is the broadest of scopes, methods, and content with some problems and issues more satisfactorily pursued by less rigid, more historical-institutional approaches, and other problems and issues by more formal abstract analysis. It all depends said Marshall. This "it depends" answer irritated both of the other groups. Marshall irritated the would-be formalists in his Appendix B of his *Principles* (Marshall 1961) praising Adam Smith as a model of method; in Appendix C, "The Scope and Method of Economics," and Appendix D, "The Uses of Abstract Reasoning in Economics," where he commended the methodology of the German historical school; in his widely circulated letter to Bowley deprecating the role of mathematics and abstract reasoning in economics; in his refusal to give precise definitions of economics, factors of production, or the representative firm; and in his *Principles* in which he preaches that "a man is likely to be a better economist if he trusts to his common sense, and practical instincts…" (Marshall, 368). The institutionalists were similarly irritated with Marshall's attempt to take what he regarded as something from all sides. They saw him as essentially accepting neoclassical theory and then slightly modifying its application.

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

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

> Marshall operates not as a theorist who sets up his assumptions and then 'reasons out' (to some general conclusions for hypothetical categories of cases), *but as one who actually has to give advice, or to make the decision*

> *in favor of one tax over another, or for no tax at all* [emphasis supplied]. He cautions frequently against making direct application of the results of simple first-round impact analysis. A prefatory note in his Memorandum, for example, points out that 'the incidence of import duties is extremely complex' and he adds: 'the indirect are often much more important than the direct effects'. . . . Marshall also warns that although the exposition to follow is concerned chiefly with 'proximate causes and their effects' a student should actually be 'endeavoring to probe to the causes of causes'. . . . (p. 162).

Despite the fact that Marshall worked assiduously not to fall into any particular methodological or policy position, his partial formalization gave a suggestion of scientific aura to the results of models. Marshall's concept of consumer surplus seemed to make it possible to draw policy results from analytic models. We can see this in Pigou's proposal to subsidize industries, and in the development of cost-benefit analysis, and the enormous focus of the economics profession on efficiency and waste to the exclusion of other issues such as the inability of government to implement proposals, or the information transfer role of prices. Thus, when there was a debate about market socialism, it concerned technical issues, and the subtlety of Hayek's arguments against socialism was lost until rediscovered in the 1980s.

The Instability of Marshall's Straddle

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

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

Marshall's vision of economics was far more complex than this, but that complexity did not come through the supply and demand diagrams. As those diagrams became institutionalized, Marshall's broader pluralism was lost. Thus, it was Friedman who picked up the mantle of

Marshallian economics in the U.S., and he used it to push a laissez-faire policy agenda.[13] In Friedman's hands, Marshallian economics led to laissez-faire policy conclusions, just as in Lerner's and Pigou's hands it led to activist policy conclusions.

In the 1930s the supply-demand diagram was expanded upon and expanded upon. It was in the 1930s that the standard monopoly concepts were created, and many of the geometric tools that are now standard in introductory and intermediate microeconomics were introduced. This geometricization of economics started a shift within Marshallian economics--towards less focus on historical and institutional detail and more on formalization.

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

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

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

The limitations of partial equilibrium analysis were recognized early on, and in the 1930s the work of Abraham Wald and John von Neuman on equilibrium conditions of static and dynamic models turned the heads of mathematically trained economists towards general equilibrium theory. As Samuelson cogently noted, "To a person of analytical ability, perceptive enough to realize that mathematical equipment was a powerful sword in economics, the world of economics was his or her oyster in 1935." (Samuelson, 1946, 315) Thus, beginning in the 1940s, economics began considering issues in a formal mathematical manner nicely described by Blaug.

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

[13] See Colander (1995) for further discussion.

its *strengths.* It showed the enormously strong assumptions that were necessary to draw out any actual information from the theory much more than it showed the power of the theory to explain real world events.

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

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

Some Final Comments and Some Thoughts about the Future

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

Formalism started winning out in the 1930s because of the failure of nonformalist schools to meet the institutional requirements for survival. It tried to become too policy-oriented, and seemed to be arguing that one could draw out policy conclusions from positive economics. Formalist writing delineated the problems with that position, but in the process created a set of institutions that kept the pendulum swinging toward formalism. Analytic and computing power also changed during this time period, causing applied work to become more technical--and more useful. It is important not to confuse the formalism of Hilbertian general equilibrium theory that Blaug is describing as formalism with the highly technical applied mathematics and econometrics that characterize much of the modern applied work in economics. That work is technical but non-formal. This increase in the technical nature of economic analysis is not an increase in formalism; it is simply a reflection of a change in technology. Whereas Marshall had

to rely on observations, today we can rely much more on technical data analysis. Vector auto regression is highly technical, but it is not formal theorizing. Similarly, much of modern applied mathematics is nonformal: researchers are not concerned with proofs but rather with pulling information out of data.

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

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