

American Economics: The Character of the Transformation

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One possible interpretation of our volume title, *From Interwar Pluralism to Postwar Neoclassicism*, is to understand interwar pluralism as a code name for the “old institutionalists,” postwar neoclassicism as full-fledged general-equilibrium mathematics, and the path between them as the natural and inexorable victory of mathematics and science over bumbling historicism. It is not difficult to recognize this account for what it is: a set of straw men ready to be blown over. Pluralism consisted of more than just institutionalism, and there was nothing predetermined about the waning of pluralism and the waxing of neoclassicism. The challenge is to provide a more convincing account. What exactly was the nature of that pluralism and of the neoclassicism that apparently replaced it? And by what set of processes did the one turn into the other? These are complex questions, and the essays in this volume provide critical components of the answers. They also place constraints on any overall account, particularly with respect to the

This introductory essay draws directly on the stimulating discussions and papers presented at the special *HOPE* conference on the subject of our volume held at Duke University in April 1997 and on the subsequent comments of conference participants in their capacity as referees for the essays submitted for the volume. Unfortunately, we were unable to include all the conference papers in this volume, but we thank all the participants for their help and apologize if we have unwittingly quoted them without acknowledgment. We thank the *HOPE* editors for their invitation to edit this special issue and the *HOPE* office for their help and advice during the conference and in the preparation of this volume. We thank the Lynde and Harry Bradley Foundation for its generous support of the conference.

timing of the transformation. We use this essay to lay out the borders of such an account and to sketch a general picture of how these components fit together in a particular way.¹ We are all too aware that we still lack knowledge of many of the elements we need to understand the full process of transformation: our account remains speculative and incomplete.

Pluralism

The first two elements for late-twentieth-century historians to understand are the extent and dimensions of the pluralism within American economics in the interwar period. One strong indication is provided by the practical difficulty of characterizing interwar economics or economists in any convincing way. It is common to think of interwar American economics in terms of institutionalists versus neoclassicals, but when one probes more closely, the picture becomes much less clear.

Although its roots extend back to the 1880s, institutionalism became a self-identified movement only in 1918 (Rutherford 1997). In the interwar period, institutionalism made strong claims for itself as a school and succeeded in becoming the most visible, if not the dominant, group in American economics. The movement cohered not around a tight theoretical agenda but around a particular view of science and a conviction of the inadequacy of the unregulated market. It cannot be said that institutionalists such as Thorstein Veblen, Wesley C. Mitchell, Walton H. Hamilton, John R. Commons, J. M. Clark, Rexford Tugwell, and M. A. Copeland all pursued exactly the same research program or utilized the same techniques of investigation. Institutionalism included Mitchell's quantitative methods, Commons's documentary histories and interviewing, Hamilton's case studies of firms and industries, and Clark's applied theorizing. Institutionalism consisted of a number of loosely related research programs, one cluster centering on business cycles and unemployment, with a reform agenda involving some notion of overall planning, and another cluster centering on the legal dimensions of markets, with a reform agenda focusing on labor law and business regulation. Institutionalism also shaded off into more "orthodox"

1. Landreth and Colander (1997) offer another account of the transformation in their broad survey paper. Samuels (forthcoming) offers an account, as well as a survey of all the conference papers and a report of the discussion that followed the presentation of each paper.

theory. For example, J. M. Clark never rejected J. B. Clark's theoretical contribution but saw himself as attempting to continue his father's efforts to develop a dynamic theory. J. M. Clark's accelerator, Mitchell's and Simon Kuznets's work on national income accounting, and Copeland's flow of funds all became standard tools.

Institutionalism, then, was a broad movement and quite nonexclusive. Institutionalists as a group had no one method to defend and no one economic theory to peddle. What they did have was a commitment to serious scientific investigation, detailed empirical work (though with no one method), serious theory building (which eschewed simple assumptions), and a commitment to understand the importance of economic institutions in determining economic outcomes. This last point relates to the institutionalists' view that new institutions or methods of "social control" were required to overcome the economic and social problems created by the existing market system.

Similarly, it is especially difficult to define "orthodox" or "neoclassical" economics in the interwar context and to provide a grouping of individuals under these labels. The marginalism of J. B. Clark was highly influential in America, but most economists of the time, including Clark himself, felt that his static theory of competition was only a starting point for a more complete and dynamic analysis. Austrian and subjectivist ideas were also important. The more "orthodox" group in the period from the 1880s up to the First World War was highly diverse and included Arthur Hadley, Frank Taussig, J. B. Clark, Frank Fetter, H. J. Davenport, and Edwin Seligman. Most of these individuals continued to contribute during the interwar period and were joined by others, such as Frank Knight, Irving Fisher, Jacob Viner, and Allyn Young. These individuals had a greater respect for the existing body of economic theory and for the market system than the more outspoken of the institutionalists, but they did not all adopt the same theoretical or methodological positions; nor did they ignore the shortcomings of the existing theory or remain unconcerned with advancing its scientific status. Hadley studied institutions and the problems of the railroads, Davenport combined Austrian and Veblenian influences, and Young brought to his theorizing, and to that of his students, an institutionalist sensibility concerning the need for greater realism. Adopting a more "orthodox" theoretical position also did not necessarily imply a lack of commitment to reform or a rejection of advocacy. Fisher is only one of many examples of this, and it is worth noting that Fisher and Commons

could profess mutual respect for each other and join forces in the Stable Money League. However, the nature of scientific economics, the nature of the reforms indicated, and the place of advocacy were all actively contested. The profession as a whole was very much in the process of defining itself and its social roles.

As we have said, pluralism is not to be understood as a code word for “institutionalism.” It was a genuine pluralism, to be taken in a positive sense. Pluralism meant variety, and that variety was evident in beliefs, in ideology, in methods, and in policy advice. We are used to thinking about the institutionalists as difficult to pin down because of their varied interests and practical approaches. But variety appears to be true in general, for there are no clean lines separating schools; indeed, it is not even clear that one can specify schools. And it is no easier to provide simple, accurate labels for many other economists active in the interwar period. Economists felt at liberty to pursue their own individual combinations of ideas. Pluralism, as Warren Samuels remarked at our conference, describes not only the difference between individuals; pluralism was in each economist. Coats (1992), in a wide-ranging survey of the period, suggests that the most “influential” economist of the period was Mitchell, an institutionalist renowned for quantitative analysis, whereas the most “representative” economist was J. M. Clark, who bridged the divide between institutionalist and neoclassical thinking. Clearly, then, in the interwar period it was possible to hold a number of different economic beliefs and to do economics in many different ways without being out of place or necessarily forfeiting the respect of one’s peers. The major institutionalists and noninstitutionalists alike published in the major journals, held professorships at leading universities, and became presidents of the American Economic Association (AEA).

This variety, along with a certain tolerance, was a feature not only of the interwar period, but also, as Bradley W. Bateman shows in this volume, of the period before the First World War. Admittedly, the original AEA statement of principles had excluded a number of “old school” economists, but the association dropped its statement to effect a conciliation and become a more catholic organization. It is worth noting that this was done out of a feeling of strength and that the *laissez-faire* economists were a small and fading minority. This was a time when a very wide range of economists, from marginalist to historical, shared a commitment to economic justice. This commitment was supported by

the population at large, as evidenced by the manifesto known as the “Social Creed” that the Protestant churches adopted before the war, as we learn from Bateman’s essay. An extraordinary document, the manifesto called for economic regulation and intervention, along with high wages, to ensure the economic well-being of the American population. Rather than a social creed, it was an economic creed and a call for action fully in keeping with the Progressives’ program. The creed provided neither a set of theoretical economic beliefs nor methods of economics but a concrete statement of faith in economic intervention and a set of specific economic aims.

But although the collapse of the Social Gospel movement and the consequent loss of impetus behind the Social Creed undermined that faith and left a vacuum in terms of ideology and policy action, the underlying plurality of economic approaches was unaffected. Economics carried its pluralistic beliefs and methods into the interwar period. When the Great Depression brought an urgent call for economic action, that plurality blossomed into a variety of analyses of, and possible solutions to, the problems. Proposals for intervention or “planning” of many different types became the fashion of the 1930s. As economic historians have long recognized, no one set of consistent economic policies made up the New Deal; even within each agency, economic aims were often at odds with each other. This reflected the variety of “planning” approaches held by the individuals concerned, as Márcia L. Balisciano documents in this volume. Although seen as a considerable failure both at the time and in most modern accounts, the New Deal made “the economy” an important responsibility for all subsequent American governments. The associated creation of demand for economic advice from the political sphere and its continuation through the period are important parts of our story of transformation, and we shall return to them later in this essay.

The Changing Standards of Scientific Economics

Understanding the pluralism of the interwar period in terms of the concept of variety does not necessarily give us any grip on the process of transformation. In order not to prejudge exactly what the outcome might be, let us start with a very broad characterization of the changes in American economics during the period by considering changes in

what it meant to be “scientific.” It seems that in the 1920s many different kinds of economists considered themselves “scientists.” From our current perspective, the mantle was broad: An economist was an investigative scientist whether he or she used the methods of history, statistics, theoretical deduction, empiricism, mathematics, or whatever. There was no hegemony of method: Whatever method might be appropriate for a particular investigation, or favored by a particular economist, might be adopted. This does not mean that all these methods were equally popular, as Roger E. Backhouse reminds us in his essay in this volume, for neither statistics nor mathematics was a popular method during the period. Nor does it mean that the label “scientific” was uncontested with respect to methods. On the contrary, it was hotly contested.

Among institutionalists, the concept of science seems to have been based on a view of natural science methods as empirical and experimental. Mitchell’s quantitative approach was quite explicitly modeled on what he thought of as the nearest approach to the methods of the natural scientist that it was possible to achieve in economics. But other institutionalists did not place the same emphasis on quantitative methods as did Mitchell; Tugwell talked of experimental economics, and Copeland talked of the natural science point of view. Although the specific techniques of investigation used by institutionalists varied, in all cases the goal was to investigate actual conditions and to create a theory that was based on realistic assumptions and that could address real-world issues and problems. Institutionalists contrasted these methods with what they saw as the overly abstract nature of much of the standard theory as it then existed. This is not to say that most, or even very many, of the more orthodox economists were pure theorists but to suggest that their theories of rational behavior and competitive markets were seen by institutionalists as relying on highly simplified and unrealistic assumptions that limited their applicability and usefulness in solving real-world problems.

Institutionalist claims concerning the nature of scientific investigation appropriate in economics echoed similar arguments being made in other social sciences, and such ideas impacted on the economics profession even beyond the institutionalists’ own ranks. One result of this was the highly concrete and problem-oriented nature of the vast bulk of work in economics, whether conducted by institutionalists or not. It was in this period that the specialist areas of labor economics and industrial organization developed, and much of the work in these areas

was of a highly empirical and concrete nature. Differentiating the institutionalist from the noninstitutionalist on the basis of the work produced in these areas is often extremely difficult. Even in the more theoretical areas of the discipline, the effect can be seen in the development of more “realistic” theories of imperfect competition, market failures, and business cycles. Of course, the institutionalist conception of a scientific economics was not simply conceded by all economists, and counterarguments were made, appearing more frequently in the late 1930s and 1940s. Individuals with more orthodox theoretical predilections, such as Knight, launched something of a counterattack, arguing that natural science methods could not simply be brought into economics without modification and that natural science was more theoretical and abstract than was being contended (Rutherford 1997). Their success was slow in coming, but particularly after the Second World War, their notions of science were reinforced by other factors impacting on the discipline, as discussed later. Ross B. Emmett’s essay in this volume outlines the changes in the nature of the University of Chicago’s economics curriculum from the 1930s to the 1950s, which reflected a growing emphasis on neoclassical theory and a move from problem-oriented to methods-oriented field courses that stressed the application of “core” theoretical and statistical methods.

Although interwar pluralism was characterized by arguments over what constituted the correct set of scientific methods for economics, this does not mean that there were no shared scientific standards. Indeed, economists must have held some shared standards that allowed them to argue over matters of method and yet share the same platforms and contribute to the same journals. Looking back from today, we gain a sense that several kinds of standards were operating across interwar pluralism. First, it is revealing that today’s natural categories of economic science, “theoretical” and “applied,” simply do not fit well in this earlier period, as Backhouse points out. The fact that economists from the late nineteenth century through the interwar period wanted to find out about the world with a scientific spirit did not mean that they used one single method of approach. But it did mean that most economics was expressed in concrete rather than abstract terms, whether the topic was one of specific or of general import.

Second, the scientific status of the work was associated more with the personal qualities and attitudes of the economist qua scientist than with any particular method used. This is consistent with several recent

history-of-science accounts suggesting that, at various times and places, personal factors have been particularly important in establishing claims to scientific objectivity. Daston (1995) has described this kind of objectivity as dependent upon a “moral economy”: a set of personal virtues or values of scientific inquiry (honesty, integrity, etc.). Respect for these qualities and values seems to have been shared by all economists, be they working in academia, in government (such as the Bureau of Agricultural Economics), or in the new privately funded institutes of economic research (such as the National Bureau of Economic Research [NBER]).

Third, economists’ integrity and commitment to a spirit of scientific investigation did not necessarily mean they pursued a value-free or policy-free scientific agenda. Both in the period before the First World War and in the interwar period, economists could characterize themselves as being scientific in their approach to their material while holding strong values and views concerning the aims of economics via economic policy. Furner (1975), in her wonderful account of American economics during the late nineteenth and early twentieth centuries, associates objectivity with evenhandedness. It became the professional ethos of economists of the period to teach both sides of a case: both free trade and protectionism; gold standard and bimetallism; labor unions and capitalism. Professionalization demanded evenhandedness. But this very demand recognizes the existence of different analyses, with different results, resting on different beliefs and values. Evenhandedness meant acknowledging differences of opinion, but it also meant impartially rejecting sectionalism in favor of the promotion of the social interest. The social interest, of course, could be variously defined, and different economists could hold different policy positions. So evenhandedness did not necessarily imply silence or neutrality on available policy options, and many economists argued strongly for particular reform packages. The economist could be an advocate in the policy domain, but only if his or her views were buttressed by a properly objective scientific inquiry.

Economists of the early twentieth century shared a kind of scientific economics (more often concrete than abstract), a moral commitment to ensure standards of scientific inquiry, and an evenhanded objectivity combined with advocacy. Pluralism was supported, not compromised, by these standards. How do these characteristics contrast with the kinds of scientific standards and objectivity we associate with postwar

neoclassicism? Modern neoclassical economics takes for granted objectivity at the level of investigation, but what is striking is that now objectivity is thought to extend to the level of beliefs and to policy advice. Two transforming processes brought about this shift.

In the first process, the notion of objectivity associated with a set of personal attributes that guaranteed the standards of scientific economics gave way to a notion of objectivity vested in a particular set of methods, namely, mathematics and statistics. These were methods that could be pronounced unambiguously scientific on the grounds that they had to be used in a technical, i.e., nonsubjective, way. The late-nineteenth-century development of statistical methods has been portrayed as ensuring the “objective” treatment of economic data (see Gigerenzer et al. 1989), while the parallel development of mathematical methods in economics carried an equivalent “objective” label for theoretical analysis. These technical treatments of both inductive and deductive arguments, and of ways of dealing with evidence, provided economists with an apparent neutrality. Economists who could rely on such technical methods no longer had to be so scrupulously evenhanded or to depend so entirely on their virtues. These technical approaches created a new kind of professional expertise that enabled economists to offer “objective” policy advice, for they could argue that the objectivity of their methods warranted the objectivity of the results of the analysis and of the associated policy advice. Porter (1995) has described, in convincing detail, the American development of cost-benefit analysis during this period of transformation to show how the turn to technical expertise (rules of calculation, mathematical formulas, and statistical data) provided economists with a defense of their analysis against attacks by those promoting political agendas or those with strong opposing values.

These “objective methods” were slow to catch on in American economics, with agricultural economics, the statistical analysis of Mitchell and the NBER, and the emerging econometrics (including the Cowles Commission) probably being the strongest areas between the wars. But the development of technical expertise, wherein scientific credibility depends on methods, was a necessary prerequisite for the application by economists of simple mathematically and statistically based problem-solving techniques in various government departments during the Second World War, as discussed in Craufurd D. Goodwin’s essay in this volume.

Of course, the claim to “objective” methods works only when different economists using similar methods produce the same answers. When they don’t, the economists’ whole claim to scientific objectivity is doubly undercut. So disagreement over how to measure and what to count among economists working on cost-benefit analyses for different arms of the government meant, in Porter’s case, that technical expertise could not force closure in the policy debate. But the very fact that technical methods are debated so vehemently speaks to their importance to modern notions of scientific economics. Disagreement over what constituted the correct set of statistical methods was one element in the famous “measurement without theory” debate between the NBER and the Cowles Commission that epitomized the divide between institutionalists’ and neoclassicals’ use of alternative types of statistical methods in the late 1940s.

The second and equally important transforming process was the growing faith in the “market solution” and the virtues of free competition. As we see in Bateman’s and Anne Mayhew’s essays in this volume, these beliefs cannot be taken for granted as part of the American tradition. Rather, such beliefs were not generally held by American economists of the late nineteenth century. They might have been precipitated by the perceived failures of economic intervention in the New Deal. But it was only in the postwar period that economists began to see and portray the free market, perfect competition, and individual economic rights as, in themselves, embodying objective, value-free truths for reasons we shall discuss later. Thus the primacy of economic efficiency as the guiding value and the possibility of separating economic values from other considerations were both postwar developments among American economists, according to Steven G. Medema’s discussion in this volume in the context of the relationship of law and economics. By contrast, the old idea, dominant in the pre–First World War period and holding into the interwar period, was that there is a necessary interrelation, indeed interdetermination, of legal and economic institutions. Institutions embody economic values, and values, by definition, cannot be value-free.

It may help to stress what we are *not* arguing here. The fact that the ethical commitments of the pre–First World War period went out of fashion (because of the failure of their supporting ideology) does not mean that a “value-free” and technical neoclassical economics would necessarily fill the vacuum. Modern neoclassical economics was not

the only possible response. The loosening of the ethical ideology allowed the commitment to self-interest freer rein. This could have meant a retreat to classical *laissez-faire* economics, also presented as value-free by its proponents but no more technical than nineteenth-century historical economics. Yet the original AEA statement of principles had been formulated exactly to exclude the old-style classical economists such as William G. Sumner. That option was ruled out. Another possible outcome of these changing standards might have been a move to statistical empirical economics, one way for our two processes implying a more technical and value-free economics to link up. Indeed, such a picture fits Mitchell and NBER economics at least up to the 1950s. Neoclassical economics was not the only viable option, and it took some time to catch on.

What does emerge from the two processes we have described is that, in the postwar era, economists increasingly adopted methods that carried the warrant of objectivity to the results of their analysis and to their policy advice. At the same time, they learned to present certain economic beliefs as value-free and therefore objective. For reasons that will become clearer, the professional ethos of economics changed. In this new ethos of economics, it became the fashion to offer consensus advice, in strong contrast to the contrary advice economists offered in the New Deal. The economist became the neutral, professional scientist, offering expert, value-free advice in a language the public could understand. At the same time, internal professional disputes began to be expressed in a separate technical language.

Economists and the Economy

Since it is widely thought that the mathematical method and neoclassical beliefs are inseparable, it might be taken as natural that changes toward quantitative tools and neoclassical beliefs occurred together. These two changes may have been concurrent, and possibly they were driven by the same causal factors, but the essays in this volume suggest that the causal factors operated in different ways.

One factor we have already discussed is the economic historical context. The Great Depression, the key event of the period from 1920 to 1960, demanded that economists take up the challenge of diagnosing and treating the illness in the economy. It is difficult to imagine an economy in which output fell by 25–30 percent and in which unem-

ployment reached a level of 25 percent and did not return to its 1929 level until 1942 (despite the demand stimulus from wartime allies). Even the most laissez-faire economists would have doubts about the efficacy of the market solution. But this was not the only depression of the interwar period. A very sudden and severe depression occurred in 1921–22. And during the war years, most economists were convinced that after the war ended, the economy would return to the 1930s depression or else would fall into a sudden slump, as after the First World War. In wartime itself, of course, the economy required considerable planning. In these circumstances, it comes as less of a surprise to find that, on the whole, economists remained pro-interventionist into the 1940s.

These historical problems in the economy not only turned economists toward intervention but also created the demand for their services to make concrete plans and suggestions for which the new technical tools of simple mathematical models and statistical techniques were well adapted. It was not that the old tools could not provide answers or that they were not technical tools. Economists were accustomed to providing specific answers to concrete questions. Railroad regulation had long depended on economic analysis of rates, and agricultural economists were used to measuring and manipulating the agricultural sector. It was not even that the Great Depression was immediately regarded as something different, requiring new solutions. The problem was seen as massive but not new. It was only in the New Deal that the demand for economic solutions widened: Every aspect of the economy became open to economic attack. American economists of every stripe responded. Charles Roos, one of the active econometricians in the early years of the Cowles Commission, became chief research economist at the National Recovery Administration (NRA), building mathematical and statistical models of industrial competition. Mordecai Ezekiel, agricultural econometrician, and Tugwell, institutionalist, both became involved in general planning. But by and large, economists did not find the New Deal a successful experience; it did not improve their reputations. And to the extent that institutional economists were involved in these schemes, they, along with the other economists, shared in the failure.

Only in the war years, and only after the United States entered the war, did the economy regain its old strength. The war, as Goodwin's essay discusses, was a watershed in several ways. Economists not only

found their technical expertise useful in making decisions about how to deal with economic shortages (rather than oversupply as in the Great Depression) but also turned their techniques to any number of wartime questions, using simple mathematical optimizing models, linear programming techniques, and statistical measurement devices. Economists were brought in to fight the war directly, planning the optimum bombing-raid design and statistically analyzing firing patterns. Economists found that by using tool-kit economics and the developing neoclassical technical expertise they could answer questions in very different fields. Economics emerged from the war covered in glory, perhaps launching the “economic imperialism” in social sciences over the last half century.

In the postwar world, as Goodwin shows, the nascent neoclassical technologies continued to prove useful, but note that this was tool-kit neoclassical economics, formulated to answer clearly specified, well-defined questions, not grand general-equilibrium theorizing. Advice remained at the level of basic microeconomics, both during and after the war, and may not have been very different from the earlier advice offered by economists at the Bureau of Agricultural Economics or in the New Deal administrations. This was the economics of Hadley (the American Marshall) rather than the sophisticated neoclassicism of Paul Samuelson. Thus, although for this new generation the concrete kinds of questions investigated and answered might have been no different from those investigated by earlier economists, the methods had become more technically oriented and the role of the economist had changed. He (mostly) offered answers but without the accompanying advocacy of the earlier period. These answers were naturally “correct” because they were the result of “objective” methods and because by that stage economists were beginning to spurn intervention and to turn to their new love: the belief of neoclassical economics in the market, in competition, and in the primacy of the self-interested individual.

The timing of this change is something of a puzzle. Why did economists fall fully in love with the market and out of love with control and intervention just as they became successful at practicing the latter in response to the events of economic and political history? It might be a question of selection: The new sort of economics expertise provided by neoclassical tool-kit methods of analysis was best adapted to the demands created by this set of events. According to such an argument, it is because of the success of their tools that economists came to

believe in the ideas behind them. This is certainly an interesting reversal of the normal internalist history of economics that portrays ideas (“thought”) as the leading light in any account. This reversed argument is also consistent with the claim usually made for Britain that the success of Keynesian tools and concepts used in running the war economy encouraged economists’, and politicians’, belief in that system of ideas and led to its popularity in the postwar period. The wartime experience of active tool-based intervention is an important causal factor in the transformation, but it is only part of the story.

Economists, War, and Society

It might seem perfectly reasonable that a society tired of war and depression and delighted with the postwar boom should react by embracing the goals of free markets and healthy competition. These views might well take time to emerge, given the still vivid memories of the Great Depression, and might take equally long to become fixed in the minds of economists. A more cogent claim, and one which surfaces in a number of the papers, relates to the cold war, a war, Goodwin reminds us, of economic ideologies. This explanatory factor has the virtue of fitting our timing puzzle, for at just the right point, American society moved solidly in favor of the virtues of free markets and open competition. In so doing, it reinforced, at a critical point just after the war, the neoclassical belief system. We find this argument more cogent because in this case the “reasonable” argument of reaction implies that European societies, more borne down by wartime economic controls, would embrace the free market more enthusiastically than the United States, but they did not. In addition, the cold war was not nearly as frozen in Europe, where reconstruction planning and welfare statism evolved into the mixed economy and the effect on social scientists’ ideas was less dramatic.

The moment at which society’s values line up with those of economists is a point to watch. Just before the First World War, the Social Creed was accepted by the mainstream Protestant churches, society lined up behind the economics of the AEA founding generation, and its program seemed ready to be put into practice. As Bateman tells us, the Social Gospel movement provided economists with a language and an opportunity to talk to the wider community and supported a pluralism of economics under the ethical umbrella. The moment was temporar-

ily lost because of the nationalist turn taken by the churches in response to the First World War. Not until the New Deal did economists again have an opportunity to talk with conviction, and a similar plurality occurred then because the ideas and approaches were those of the pre-1920 period, brought forward partly by a new generation. It is one of the ironies of our tale that just as something close to the Social Creed of the churches became politically well supported and official economic policy at the end of the Second World War, a new form of nationalism appeared. (A comparison of the episodes revealed in Bateman's and Balisciano's essays is instructive.) At this second moment, the nationalism of the cold war pushed society's commitment to economic freedom well ahead of that of the main body of American economists. In the climate of those times economists found it safer to conform, and the developing neoclassical economics, which incorporated similar values, was given a big boost.

The effects of McCarthyism surely cannot be ignored in any account of the transformation of American economics. Although we have records of émigré economists who arrived in America to escape fascism in the 1930s (see Hagemann and Krohn 1991, Craver 1986), we have little more than anecdotes of economists who left America to avoid anticommunist persecution. Some of these, of course, held views that might be communist, or prosocialist, but even Keynesianism was a suspect heterodoxy, and those who had espoused planning for the postwar economy a few years before, let alone staunch New Dealers, might well have found themselves outcasts. Some of these economists returned, others did not, but their absence was certainly one of the factors that made for a narrowing from the earlier pluralism of American economics in the postwar period.

The result of these pressures on those who remained was both to narrow the range of beliefs and to restrict the acceptable ways of expressing them. Goodwin's essay suggests that this narrowing was achieved partly through a turn toward greater technicality and toward the apparently "neutral" languages of mathematics. As we have noted, this post-Second World War technical defense by itself does not necessarily imply neoclassical economics. Although Keynesianism might have been thought dangerously close to Marxism, an IS/LM diagram probably looked innocuous to an outsider, and statistical numbers such as those of Mitchell had long held their own neutral status as "data." Economics expressed in geometry, algebra, or numbers could be a good

self-defense in the cold war days and pass muster in the classroom as well as in the government. Indeed, in our more general framework, this move to technical methods was precisely the move that Porter (1995) suggests made economics defensible against democratic power, whoever wielded it on whichever side. The cold war enforced, if it did not create, the trend toward economists offering professionally neutral, objective expertise, which contrasted strongly with the ethical, and strongly held, advocacy of the late-nineteenth-century professional economist. Even in their “evenhanded” mode, public statements of the late nineteenth century offered considerable political ammunition compared to the expert jargon and tool-kit style of postwar economics, which could be used to disguise theoretical content and ideology to the outside world.

Although this move to mathematics was partly a self-imposed defense undertaken by individuals (see Johnson 1977), it was also encouraged by academic institutions seeking “safe” teachers and research institutes seeking “acceptable” researchers. As Goodwin’s essay suggests, patrons of economic research exacted an obligation of political correctness in line with the cold war that had the effect of narrowing the views that could be expressed and count within the mainstreams of economics, be they academic or governmental. There is some suggestion that patrons and economists colluded in hiding radical ideas from the public, for institutions, as much as individuals, sought safety in the climate of political repression. Elsewhere there was open warfare. One of the few case studies of the academic effects of the cold war, by Solberg and Tomilson (1997), describes events in the Economics Department at the University of Illinois. There, “academic McCarthyism” drove out both nascent Keynesians and those advocating modern tool-based economics, a combination that included Margaret Reid, Leonid Hurwicz, Dorothy Brady, Robert Eisner, Don Patinkin, and finally Franco Modigliani. Regardless of the process, the effect was the same: Both open persecution and closet correctness led to the narrowing of permissible economic opinion.

The retreat was not total, for although being a follower of Keynes was a dubious label for American economists in the 1950s, Keynesianism could be made compatible with market economics in the American environment. This was accomplished both by persuading businesspeople that they could do better under a government that took the macroeconomy seriously (as Balisciano suggests) and by translating Keynes-

ianism into the same technical form as neoclassical economics, the first step to the American “neoclassical-Keynesian synthesis.” In a similar way, old-fashioned institutionalist monetarism was made compatible with American academic neoclassicism, as Perry Mehrling recounts in this volume. Economists could safely argue about the same old things, but their debate had become an insiders’ technical argument, not one open to the public gaze. The public debates of the 1930s became internal technical disputes in the 1950s and 1960s.

It is clearly difficult, without a lot more research, to assess the impact of McCarthyism on the transformation of American economics. Yet the counterfactual question “What would have been the history of American economics without the cold war?” indicates the potential answers to our puzzles about timing and about the degree of belief in the efficacy of the market. Remember, the puzzle is that although the institutionalists were strongly evident as a grouping within the general pluralism of the 1930s (but failed to strike the advantage offered by this position to coalesce around a single program), the neoclassical grouping did not really become evident until the 1950s (see Rutherford 1997). The relatively sudden shift from the institutionalist (indeed general) belief in government intervention to the more neoclassical belief in the free market can be explained when we consider seriously American society’s views in the cold war. The reinforcement of the technical turn in economics (and not just in neoclassical economics) emerges as an important, although clearly unintended, consequence of the same causal factor, namely, the political climate.

Thus a transformation to a tool-kit version of economics in general, and to the beliefs of neoclassical economics in particular (which was only one of many strands in the interwar period), was reinforced and given impetus by the social values of the time, so that by 1960 American neoclassicism was well established. The extent to which these moves are evident in the journals gives us some backing for our account. Backhouse’s essay suggests that formally expressed economics was on the increase beginning in the 1930s, but the real change came after 1945. Empirical econometrics developed a little later, with the real change occurring after 1950. Thus once again, the war was a watershed: After this time we see both modern style and modern research categories beginning to emerge in considerable strength, concurrent with the move by economists to a self-defensive technocratic approach.

The New Style and Its Implications

Those who find modern neoclassical economics unconnected to the real-world economy are apt to blame increasing formalism in some form or other for this state of affairs and to blame the profession for this move. But instead, as we have argued, formalism should first be seen as the outcome of various external contingencies, not the cause of internal ones. We have already discussed how the demand for economists to solve policy problems led to the increasing use of technical tools both in the Great Depression and, more particularly, in the war. We described this as tool-kit economics rather than neoclassical economics, for it is important to remember that not all tools were associated with neoclassical economics; not all econometricians were neoclassical economists; and planning of whatever type demanded numbers and statistical work on a large scale. We have also discussed the way in which this technical turn was strongly reinforced by economists' and patrons' need for self-defense during the cold war. Here it was not so much what the technical tools would do for you but rather that the language of mathematics and statistics appeared to be more neutral and objective, and more difficult for the layperson and politician, leaving the economists less open to outside attacks about matters of belief. Formalism therefore offered economists both tools for practical usage and neutral language for expression and for safe professional argument.

But there are second-round and more subtle effects of these changes which are perhaps best understood through the case study that Mehrling gives us, and this is why formalism does indeed bear some of the blame for the waning of pluralism. In his account of disputes within monetary economics, Mehrling argues that the joint adoption of neoclassical beliefs and mathematical expression created a kind of monetary Walrasianism within which monetary arguments became no longer matters of belief, or even of empirical evidence, but technical matters of modeling. We can understand from this example just how the changes in language and the form in which economics was expressed narrowed down what could be said and flattened what could be questioned. The real underlying arguments about money might remain, but they could no longer be expressed fully and explicitly within the new sort of formalized economics. This process worked by integrating awkward empirical findings or theories from the other types of economics into the formal framework of neoclassical explanations. This is one way to

interpret the fate of Gardiner Means's administered-pricing thesis, discussed by Lee (1997), and the developments in cost theory discussed by Naples and Aslanbeigui (1997). In all these examples, the transformation into formal economics involved changes in language, form, and tools. This new style became a set of mores that reduced in itself the possibility of pluralism within economics.

Another connected contextual factor needs to be brought in: the role of mathematics in economics. In providing the intellectual context for the work of the early mathematical economist G. C. Evans, E. Roy Weintraub's essay in this volume discusses the changing relationship between mathematics and science. This early proponent of mathematical economics, along with many of those committed to the econometrics movement in the interwar period, believed that successfully using mathematics meant hooking it onto the economic world, a view current in the sciences in the late nineteenth century. By the mid-twentieth century, economists were more likely to be enthralled by a turn in mathematics itself that viewed mathematics as a way of writing down consistent theories. Mathematics became the language for the expression of abstract and general theory rather than a tool for uncovering and writing down true descriptions of the economic world. Thus at around the time the main body of American economists began to prefer the language of mathematics on defensive ground, the scientific role of mathematics was itself coming free of its connections to the scientific (economic) world.

If early mathematical economists in general shared Evans's views, then the shift in the perceptions about mathematics in science helps explain both the formation and the collapse of the econometric movement, conceived in the interwar period as the integration of mathematics and statistics into economics. If mathematical descriptions must hook onto the world, they need to link the observable or measurable to the hypothetical. Around 1950 this dream of the econometric tradition that mathematical economic theory had to match something observable (even if not actual statistical data) collapsed, and econometrics split into mathematical economics and econometrics as we now know it. It is pertinent that another element of the late 1940s "measurement without theory" debate between institutionalists and neoclassicals focused on the correct role of mathematics in economics. Under the older commitment to a mathematics that hooks rigorously onto the world, it would not have been so easy for the neoclassicals to use mathematics to con-

solidate their position against the institutionalists, for as Mehrling suggested (during discussions of Weintraub's essay), American institutionalism shared some of the methodological prejudices of the nineteenth-century math. The more recent tradition of twentieth-century mathematics fits better with the prejudices of neoclassical theory, and so the shift in the role of mathematics in relation to economics tended to support the emergent neoclassical hegemony. The waning of institutionalism and the rise of neoclassical economics were shaped by the evolution of mathematics in relation to the sciences.

We have used the term "style" to describe the differences implied in American economics as it emerged through the cold war world of the 1950s and 1960s. Our term style involves, first of all, a language, one that narrowed what could be said, a language not so closely connected to the world or easily accessible except to professional economists and their students. The term could be extended to the idea of a "laboratory economics" (at Bateman's suggestion) in which the tools of mathematical modeling and statistical econometrics, though no longer fully integrated, could be rejoined in an alliance more tenuously connected to the world. (Here we should note that the term "lab" was also used as a label for the professional-level "applied-theory" training that the University of Chicago developed at this time.) These changes are epitomized in the development of consumer theory, described in this volume by Philip Mirowski and D. Wade Hands. Here we have something like the archetypical neoclassical American economics, concerned with formal puzzles created by the use of mathematical representations, using mathematics to express worries and venturing, but not too far, into statistical data. Within this, technical disagreement was expressed in professional-level debate: the pluralism of neoclassical economics. Yet nothing hangs directly on the argument — no income subsidy will be granted or abolished, although maybe someone will get a grant to do further research!

But the notion of style involves more than language and forms, for the laboratory ideal was also a practical one with practical tools. Indeed, the institutionalist tradition lives on in American applied economics, which has a reputation for care and thoroughness in its empirical inquiries. Weintraub's notions of the two roles of mathematics might also, he suggests, be seen as underlying our modern labels of theoretical versus applied economics, categories that become cogent in Backhouse's survey only in the postwar period. Thus in sorting out

style from blanket descriptions of neoclassicism versus pluralism, we would do well to remind ourselves once again that this is not a history of thought but a history of a discipline, with people and institutions offering services and other people and institutions demanding those services. Patrons wanted economists to be able to solve real problems (not mathematical puzzles, high theory, or historical themes) in a professional (i.e., expert) style. They wanted usable economic science, not something esoteric, whether it was called neoclassicism or Keynesianism. This makes the outcome something more than a change of language and the adoption of certain forms and tools, something more akin to a change of approach or style.

The economics that emerged was one in which economists learned to cut up the problem into something small enough that it could be solved but still realistic enough that people could relate to it. The style change involved no great new methodological or theoretical commitment, yet it was an important part of the changing face of American economics during the period. And although such a change in approach did not necessarily require any initial changes in beliefs, it did have implications for beliefs in the long run. As Mehrling describes, an initial continuity of ideas may, through a change in methods of expression, gradually bring about changes in content and beliefs.

The Waning of Institutionalism

Our concentration on the importance of the contexts of economic, political, and scientific history has mostly focused on why formal and neoclassical economics found themselves strengthened by the course of events, but much of what has been said also touches on the relative decline of institutionalism. It is worth pulling together some of the strands of this story as it applies to institutionalism, particularly as a number of the essays in this collection provide insight into the detail of the processes involved.

As Goodwin shows, the decline of institutionalism was not rapid, and even as late as 1948 economics was still pluralistic, offering abstract theory, high empiricism, and institutional studies. Nevertheless, in general terms, the same factors that gradually strengthened neoclassicism had an opposite effect on institutionalism, so that institutional economics, so important in the interwar period, began to wane as neoclassical economics strengthened. This, however, is very much an overall view, and

we should remind ourselves of the diversity within institutionalism. Different parts of the movement were impacted in different ways and at different times.

The changing concepts of science and of scientific objectivity are cases in point. In the interwar period, the NBER showed a nice combination of the two notions of objectivity discussed previously. Objectivity as evenhandedness and as recognition of differences of opinion can be seen in the NBER's appointing directors who represented different points of view and different constituencies. Objectivity as technique is embodied in the quantitative methods designed to establish facts impartially and in Mitchell's determination to keep a clear separation between scientific fact-finding and the use of facts in policy advocacy. Mitchell's desire to separate science from advocacy was not shared by other institutionalists, and the difference between Mitchell and other institutionalists on this matter may explain the ability of Mitchell's program at the NBER to sustain its relatively high standing for as long as it did. By way of contrast, Hamilton suffered attacks for his proposals for the coal industry, which may have affected his standing at the Brookings Graduate School (Ross 1991, 417), and throughout his career, Commons was at the center of political controversy. Such a professional stance was exactly what Goodwin suggests would have been at odds with the new scientific style increasingly demanded by funding agencies and other consumers of economic analysis in the postwar period.

Other aspects of the fate of the Wisconsin school of institutionalism and of the "old" style of law and economics are examined in this volume by Jeff Biddle and by Medema. Biddle examines the hypothesis that University of Wisconsin graduates went into government in larger numbers than the graduates of more orthodox schools, so that Wisconsin-style institutionalism failed to reproduce itself within the academic world. This might have been a reflection of the ideological slant and particular training provided by Commons and others at Wisconsin. Biddle finds only limited support for his hypothesis, but it is certainly true that Commons's students did little to advance his conceptual scheme involving the legal and economic system and were more attracted to his work on labor economics and to some of his specific reform efforts. Commons's students replicated his more concrete and problem-oriented kind of work, a type of work that gradually lost ground to toolkit economics. In this respect, imagine how hard Commons's type of institutionalism in all its complexity was to teach and to learn. It did not

look technical (it wasn't quantitative), but it relied on detailed study, knowledge of law as well as of economics, an understanding of personalities and situations, experience in mediation, and personal integrity — a skill set that contrasts rather sharply with that imparted by the applied-theory workshops developed at the University of Chicago and described by Emmett.

The Commons-Hamilton type of law and economics, pictured by Medema as multifaceted, pluralistic with respect to methods, interdisciplinary, and based on a concept of the law and the economy as mutually determined and determining, was at odds with the changing temper, so that the old type of law and economics was in serious decline even before the development of the new type of law and economics at Chicago. The new type of law and economics, with its very different views on competition, antitrust, and other policy issues, both reflected and helped advance the ideological move of the profession away from the reform agenda of institutionalists.

Other sections of the institutionalist movement were damaged by the experiences of the Great Depression and the New Deal. This is particularly the case among institutionalists such as Tugwell and Means who advanced the “structuralist” or proplanning view during the early phase of the New Deal. As Mayhew and Balisciano make clear, the outcome of the New Deal experience was not only a move away from structural planning and to a Keynesian style of macroeconomic policy but also a move away from a regulatory approach to industry and to a more pro-competitive stance involving the enforcement of the Sherman and Clayton acts.

These developments, involving a turn away from planning and regulation and toward the market and competition as instruments of control, could only be reinforced by the ideological impact of the cold war.

Conclusion

Implicit in this account of the transformation of American economics is that the decline of pluralism in American economics was neither a simple nor an obvious result of the development of neoclassical economics and vice versa. No logical relation says that this must have been so, nor does the evidence support such a direct causal story. We have also tried to avoid basing our account of the transformation on two other polar positions.

One is the Whiggish progress account. It is not clear that the evidence can support a history that neoclassicism won out because it offered better theory and better explanations. To argue that such an outcome was inevitable, that neoclassical economics offered “better science,” clashes with our claim that the changing notions of science and scientific objectivity were part of the transformatory process. As we have argued, neoclassical economics grew in dominance as the notion of science changed and the two developments were connected. This being the case, there were no stable internal criteria on which to offer a historical judgment about “progress.”

The other account we have eschewed is the conspiracy theory, in which neoclassical economists in positions of power ganged up on the heterodox. Since this view assumes that heterodox science might have won the battle but for the social power of neoclassical economists, the account similarly implies that one group has historically measurable claims to be a “better science.” This is as problematic as before.

Furthermore, both of these polar positions are premised on some recognizable duality of institutionalist (or heterodox) and neoclassical groups that we believe cannot be identified in the interwar period. This is not to deny that one can point out individual institutionalist and neoclassical economists or that they had differences of opinion but to suggest that it is difficult to make either the progress or the conspiracy theory work, because the pluralism of the interwar period cuts across individual beliefs. The war was a watershed in which the transformation process suddenly resolved itself, so that after that time we can sensibly begin to talk in terms of such groups.

Nor do we wish to deny either that there were individual battles or that institutional power mattered to the outcomes. But such power plays took place within structures involving patrons and hierarchies operating within the context of a political and economic society that supported calls for economic intervention in the interwar period and for free markets in the postwar period. These wider economic and political beliefs are never just backgrounds against which individuals (and institutions) fight science wars; they provide content for the debate and are integral elements in any power struggle.

In seeking to provide a general account of the transformation in American economics, we have concentrated on explanatory factors within which the individual substories could be placed. Our primary

aim has been to provide an account of the transformation consistent with the timing and character of changes suggested in the individual essays in this volume: They each have their own history to tell, with separate contingent circumstances. In making one account into which they all fit with ease, we have concentrated on the contingencies of the world outside: scientific, political, and economic. These have formed the basis of our explanations. It was this world that created the circumstances to which American economists adapted and within which their economics was transformed.

The story of the transformation is far from closed. Many holes must be filled, and many parts of our account remain speculative, requiring substantial historical research to turn them into documented history. Whatever historical strength lies in our account is drawn from the essays in this volume; the speculations and errors remain our own.

References

- Coats, A. W. 1992. Economics in the United States, 1920–70. In *On the History of Economic Thought: British and American Economic Essays*. Edited by A. W. Coats. Vol. 1. London: Routledge.
- Craver, E. 1986. The Emigration of the Austrian Economists. *HOPE* 18.1:1–32.
- Daston, L. J. 1995. The Moral Economy of Science. *Osiris* 10:3–24.
- Furner, M. O. 1975. *Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science, 1865–1905*. Lexington: University Press of Kentucky.
- Gigerenzer, G., et al. 1989. *The Empire of Chance: How Probability Changed Science and Everyday Life*. Cambridge: Cambridge University Press.
- Hagemann, H., and C. D. Krohn, eds. 1991. *Die Emigration deutschsprachiger Wirtschaftswissenschaftler nach 1933*. Stuttgart: Universität Hohenheim.
- Johnson, H. G. 1977. The American Tradition in Economics. *Nebraska Journal of Economics and Business* 16:17–26.
- Landreth, H., and D. Colander. 1997. The Formalist Revolution in American Economics. Paper presented at *HOPE* Annual Conference.
- Lee, F. S. 1997. Administered Price Hypothesis and the Dominance of Neoclassical Price Theory: The Case of the *Industrial Prices* Dispute. Paper presented at *HOPE* Annual Conference.
- Naples, M. I., and N. Aslanbeigui. 1997. The Cost Controversy: Interplay between History and Ideas, 1920s–1950s. *HOPE* conference paper.
- Porter, T. H. 1995. *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*. Princeton, N.J.: Princeton University Press.
- Ross, D. 1991. *The Origins of American Social Science*. Cambridge: Cambridge University Press.

- Rutherford, M. 1997. American Institutionalism and the History of Economics. *Journal of the History of Economic Thought* 19.2:178–95.
- Samuels, W. Forthcoming. The Transformation of American Economics: From Interwar Pluralism to Postwar Neoclassicism: An Interpretive Review of a Conference. *Research in the History of Economic Thought and Methodology*.
- Solberg, W. U., and R. W. Tomilson. 1997. Academic McCarthyism and Keynesian Economics: The Bowen Controversy at the University of Illinois. *HOPE* 29.1:55–81.