

Economics since the Second World War

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This is a very rough preliminary draft, with many errors and omissions, that is circulated for discussion and comment only. Any references should take account of its preliminary nature.

Introduction

Robert Solow (1997: [page ref required]), one of the most influential figures in the discipline, summed up what had happened to economics between the 1940s and the 1990s as model-building. “Economics”, he argued, “became a self-consciously technical subject, no longer a fit occupation for the gentleman-scholar.” His most direct evidence came from textbooks and college classrooms. In the

1940s, textbooks included pages and pages of prose, uninterrupted by diagrams or other technical devices, and semesters could pass without any analysis of models. In contrast, whereas by the 1990s, arguing in terms of models had become universal practice amongst economists, with the result that textbooks had become full of analytical diagrams and students could no longer avoid models.

This change, Solow pointed out, had nothing to do with “formalism”, as the term is understood in mathematics. Formalist economics does of course exist, and is prominent (its most prominent exponent, Gerard Debreu, is the economist’s equivalent of a household name) but most economists take no notice of it. The essence of a model is not axiomatic treatment of a subject, but strategic simplification, so that logical reasoning can be used to identify salient relationships, and so that theoretical ideas can be confronted with data. This theoretical strategy is pragmatic and linked with data, for data suggest fruitful theoretical models, and theoretical models serve to explain the data.

This relationship between models and data led Solow to suggest a link between modeling, the Keynesian revolution and the increased availability of statistical data. These links go in many directions. Keynesian economics, stemming from Keynes’s *General Theory of Employment, Interest and Money* (1936) provided a framework ideally suited to the construction of theoretical models – a ‘gold mine’ of opportunities; and the availability of national income accounts, which Solow dates to Simon Kuznets’s *National Income and Capital Formation, 1919-1935*, made it possible for Keynesian theory to be linked to reality. At the same time, Keynesian economics provided the conceptual framework around which the national accounts could be constructed. Together, these two developments promoted the development and use of econometric (statistical) techniques. Keynesian economics and national income accounting originated in the Great Depression, “but that is just history”: the coincidence of the spread of model-building and Keynesian economics was an accident with consequences, the consequence being “an interplay among theory, the availability of data, and the econometric method” that led to a spiralling evolution of the subject.

Solow went on to argue that the alleged “counter-revolution” of the 1970s, when the New Classical Macroeconomics and then Real Business Cycle Theory turned Keynesian policy prescriptions upside-

down represented a continuation of this same progressive movement. The controversies of this period and the way they were resolved through the analysis of data, represent a vindication of model-building not a criticism of it. It has caused economics to draw away from the other social sciences, the flow of ideas from economics to other social sciences being much greater than any flow in the other direction.

Since the Second World War, economics has been dominated by the United States, in a way not true of any previous period. The reasons for this are probably too obvious to need discussion: the size of the US economics profession; the wealth and prestige of the US; and its dominance of world politics and culture. So the history of economics is very much the history of US economics, which explains the focus of this paper. However, though the fact of US dominance hardly needs explanation, the way in which the US interacted with the rest of the world does. The rest of this paper, therefore, outlines the history of economics in this period, discussing relatively briefly, in the section entitled “The international dimension” links between the US and the rest of the world.

The United States and the Second World War

The Second World War is more than a conventional dividing line, for it profoundly affected the course of economics in the United States. The inter-war period had been one of pluralism within economics: there was a variety of competing approaches to the subject, none of which was dominant. It was also comparatively easy to discern distinctively “American” trends in economics, which could be related either to the intellectual environment (notably the pragmatism of C.S. Peirce, William James and John Dewey) or to economic circumstances (such as the recent establishment of the Federal Reserve System). Within a decade of the Second World War, if not earlier, this had changed dramatically. Economics was becoming more technical, the foundations of an orthodoxy were being laid, and the position of the United States in relation to other countries was changing. The conventional explanation is the Keynesian revolution, reinforced by the rise of mathematical economics, but there is much more to the story than that.

The key to this picture is the so-called “old” Institutionalism. In the inter-war period, Institutionalism was a very broad movement aimed at making economics more scientific through placing it on firmer empirical foundations. Though its boundaries were very blurred, it is reasonable to see it as covering economists as diverse in their empirical work as Welsley Mitchell, Simon Kuznets, Gardner Means, John Commons and John Maurice Clark. Though they had connections with economists in Europe, it was a distinctively American movement. Mathematical economics is inherently less culturally specific, but even here there were distinctive American approaches to the subject: Paul Samuelson’s early work, under E. B. Wilson, drew on a type of mathematics very different from that used by Europeans. The same could be said about the early econometricians, from Henry Ludwell Moore to Henry Schultz: there were important European parallels, but they were pursuing research in a way that was distinctive. Monetary economics illustrates both the distinctiveness of American economics and the blurred boundaries between different approaches to the subject. Due to the very late establishment of the Fed compared with European central banks, there was much more lively debate over the principles on which it should be run. The result was a rich mixture of arguments spanning the divides between neoclassical and institutionalist, Harvard and Chicago, Banking School and Currency School.

The Second World War was important for several reasons. The first is that economics became tied up with the war effort. Economists were clearly involved in places such as the Office of Price Administration, the Treasury or the War Production Board. It was in the last of these that national income statistics, first calculated in 1933, were developed into a system of national accounts, providing the basis for planning the massive shift of resources from civilian to military production that took place after 1941. However, perhaps more significantly, economists became involved in fighting the war, primarily through the Office of Strategic Services, forerunner of the Central Intelligence Agency. In the OSS, economists and other social scientists were employed alongside physicists and other scientists in tasks where economics shaded imperceptibly into statistics and engineering. They analyzed intelligence, and became intimately involved in questions of military

strategy and tactics (Leonard 1991), emerging from the war with an enhanced reputation. Economics became much closer to Operations Research: a set of techniques centered on optimization subject to constraints.

At the end of the war, the [give correct title of act] GI Bill, offered scholarships to US ex-servicemen who wanted to go to university. This fueled a dramatic increase in the university system. The number of Bachelor's degrees awarded in US higher education institutions, which had never risen above 187,000 before the war, rose to 271,000 in 1947–8 (317,00 if higher degrees are included too) and 432,000 in 1949–50, many of these choosing to study economics. This accelerated the generational shift that was taking place, providing academic openings for economists returning from government service to civilian life, many of these being in institutions that had not been prominent before the war. Whilst some economics departments continued as before, there was a shift in the profession's center of gravity away from places such as Wisconsin (the leading center for Institutionalism) towards ones like MIT, Berkeley and Stanford (W. J. Barber in Coats 1997; R. E. Backhouse in Morgan and Rutherford 1998). The subject began to be taught using textbooks written by young economists (Kenneth Boulding, Lorie Tarshis and Paul Samuelson) during the 1940s, in place of ones that had their origins nearer the turn of the century.

The Cowles Commission and RAND

A particularly important center for quantitative work in the 1940s was the Cowles Commission, which had moved to Chicago in 1938, and of which Jacob Marshak became Research Director in 1943. He laid out a program of research focusing on the development of new methods to take account of the specific features of economic data, perceived to be simultaneity, the importance of random disturbances and the prevalence of aggregate time-series data. This program proved to be one that brought American economists, many of whom were involved in the war effort, and many of the highly technical European emigrés such as Trygve Haavelmo, Tjalling Koopmans and Abraham Wald. Not surprisingly, the Operations Research side of economics was dominant here, not simply in

obvious ways, such as the work by Koopmans and George Dantzig on linear programming and the simplex method, but in the broader conception of economics as engineering. This was not confined to the Cowles Commission (one can see such an influence at other places such as MIT) but such work clearly centered on Cowles.

The important idea that emerged from this phase of the Cowles Commission's work was that the economic system could be analyzed as a probability distribution, the task of economics being to identify the properties of that distribution. General equilibrium theory, embracing individual optimization within a system of simultaneous equations, provided an account of the structural relationships. Statistical methods, pioneered by Haavelmo and Koopmans, provided the means (2SLS, FIML, LIML) for relating that theory to data that exhibited random shocks in addition to any systematic relationships between the data, not just estimating coefficients but also testing the theory. This has been called a probabilistic revolution (Morgan 1991). Controversy over these new methods erupted in 1947 when Koopmans (1947) challenged, head on, what had previously been considered the scientific way to do empirical economics – the National Bureau's meticulous data-gathering and comparatively informal data-analysis – represented by *Measuring Business Cycles*, by Arthur Burns and Wesley Mitchell (1946). As Rutledge Vining (1949), replying for the NBER, justly claimed, Koopmans had written a manifesto for the Cowles Commission's new methods. In addition to the techniques mentioned above, its fruits ranged from the monetary theory that formed the heart of Don Patinkin's *Money, Interest and Prices* (1956), the leading graduate macroeconomics textbook till the 1970s, and Lawrence Klein's models of the US economy, from which developed much of macroeconometrics.

In the late 1940s, the leading focus for such work shifted to a new organization, the RAND corporation, at Santa Monica, California. Having accumulated an unprecedented group of scientific experts (mostly scientists and engineers, but also social scientists) senior figures in the US Air Force were reluctant to see it disbanded at the end of the war, especially as the Soviet threat and the Cold War loomed larger in their thinking. The solution was the establishment of RAND, initially a division

of the Douglas Aircraft Company, but from 1948 a non-profit organization, funded at first by the US Air Force, and later by other bodies, of which the Ford Foundation was the most important. Through H. Rowan Gaither, Chairman of RAND's Board of Trustees and, from 1953, President of the Ford Foundation, these two bodies became closely linked. RAND's overall product was "systems analysis", a broad umbrella under which a range of mathematical work could be sponsored, linked primarily by certain sets of mathematical techniques and a vision of economics centered on rational choice. Though it included much else, from Kenneth Arrow's *Social Choice and Individual Values* (1951) to *Economic Theory and Operations Analysis* by Dorfman, Samuelson and Solow (1958), its main significance was in game theory, bringing together economists from Cowles with economists and mathematicians from Princeton (which included John Nash [check]), the major academic center of research into game theory during the 1950s.

The precise significance of the military involvement in economics is not yet clear. The Office of Naval Research (ONR) provided much funding, especially for game theory research, and the US Air Force was behind RAND. Clearly some projects were directly driven by military imperatives, such as working out a strategy for responding to (or anticipating) a Soviet nuclear strike. There are also clear links between systems analysis/operations research, and the techniques associated with these, to military requirements. Against this, those involved emphasize that researchers at RAND were given great freedom and military sponsorship had little or no effect on what they did (c.f Mirowski 2002). However, even if researchers did have a high degree of freedom, there was certainly selection bias in the types of projects and researchers who received support from these sources, and the scale of such funding makes it plausible to argue that may have had a significant effect on the way the profession developed.

Mathematics, technique and the 'core'

To say that economics has become more mathematical in the postwar period is too obvious to need justification. However, the significance of this process and the way it came about are far less obvious

and need disentangling.

Mark Blaug (1999) has labeled what happened to economics after the 1950s “the formalist revolution”. However, within this lie a number of very different developments. One is the incursion into economics of formalist mathematics. At the outset of *The Theory of Value*, Debreu (1959) wrote that he was approaching his subject with the degree of rigor associated with the contemporary formalist school of mathematics. His work formed part of a broader movement towards placing economic theory on an axiomatic foundation, and comprising the literature on existence, uniqueness and stability of general equilibrium (c.f. Weintraub 2002). Even here, however, it is possible to discern strands that on closer inspection are very different. Ingrao and Israel (1990) distinguished the formal and interpretive branches, associating the former with Debreu and the latter with Arrow. Others have traced differences to disputes over formalism in mathematics (Weintraub 2002). The most eminent mathematician to engage with economics, John von Neumann, was not only a critic of formalism (in the sense of Hilbert): his interest in economics stemmed from a broader concern with artificial intelligence that, Mirowski (2002) has argued, differentiated his views sharply from economists at the Cowles Commission and others pursuing general equilibrium analysis.

More significant than this is the fact that most economics, as Solow (1997) has observed, is not formalistic in this sense. Rather, what has happened is that economics has become more “technical”: he was probably right to argue that axiomatics was of no interest to most economists. Perhaps the most influential exponent of mathematics in economics, Paul Samuelson, whose *Foundations of Economic Analysis* (1947) amounted to a manifesto for mathematical economics. His work, which arose from a mathematical tradition very different from the European traditions out of which von Neumann and Debreu came, sought to be rigorous without being based on axiomatization. Even further from formalism, but equally influential, was the Chicago School, dominated from the 1940s to the 1970s by Milton Friedman. Sharing Samuelson's emphasis on using theory to generate predictions, Friedman favored simple models. Thus Hands and Mirowski (in Morgan and Rutherford 1998) have distinguished three schools in postwar neoclassical price theory – Stanford, MIT and

Chicago. They share a common root in Henry Schultz's work in demand theory, that theory was interpreted in radically different ways.

However, becoming more technical is not synonymous with using mathematics. Another dimension is the separation of theory and application. Though the distinction between theory and applied work is taken for granted by most contemporary economists, the situation was very different before the Second World War. There was much work where it is impossible to draw any distinction between statements that are intended to describe the world and ones that are at the level of theory. In what we would now consider applied work, the practice of clearly separating theory and application is something that emerged only after the Second World War (see R. E. Backhouse in Morgan and Rutherford 1998). This change is reflected in the language economists began to use: they began to talk in terms of models. Though the idea of a model has deeper roots, it is virtually absent from the major economics journals before the 1940s.

As the discipline changed, so did the curriculum, something in which the AEA became involved. During the 1940s, partly in response to demands of wartime, and partly because of broader uncertainty about how economics should be taught, the AEA established committees on undergraduate education, the main outcome being a report in 1950. Out of this rose the suggestion to review graduate education, resulting in a report by Howard Bowen, sponsored by the AEA and funded by Rockefeller, which appeared in 1953. On the grounds that “technical knowledge” of economics would be useful for those working as economists in government, business and education, this argued that “there should be a ‘common core’ for all students who are to be awarded advanced degrees in economics” (Bowen 1953: 2). This core consisted “primarily of economic theory including value, distribution, money, employment, and at least a nodding acquaintance with some of the more esoteric subjects such as dynamics, theory of games and mathematical economics.” No one, it was argued, had claim to an economics PhD without “rigorous initiation” into these and economic history, history of economic thought, statistics and research methods (Bowen 1953: 43). Interestingly, mathematics was placed alongside Russian, German and Chinese: it was important to have some

economists with knowledge of it, but it was not necessary for all do so.

In Bowen's report, the core was still very broad – a statement of the range of knowledge that economics PhDs should be expected to have. Over the following two decades it came to be used more narrowly. For example, Ruggles (1962: 487) wrote of the function of graduate training being “to provide a common core of basic economic theory” that would be used elsewhere in the program, and observed that “at a great many universities” training in mathematics was required. However, this was still discussed alongside language requirements. Though such questions had been raised as early as the Bowen Report, it was in the 1960s that the AEA meetings hosted debates over the role of economic history and history of economic thought in the graduate curriculum. Gordon (1965) conducted a survey implying, as Bowen had found a decade earlier, that though most graduate schools still offered the subject, history of economic thought was declining, and that there was pressure for it to decline further, particularly from younger faculty. [discuss Ruggles 1970 here]

These trends continued to the end of the century. They are best summed up by saying that economists were increasingly being trained, at PhD level, as technicians rather than as scholars in the traditional sense. In the 1940s, when concerns were raised about this in AEA meetings, it was still plausible to respond the demands of scholarship, and breadth of education, were compatible with mastering the necessary technical skills, by the 1970s, this was becoming more and more difficult. The demands of technique were pushing courses that provided breadth, symbolized by history of economic thought, out of the curriculum. By the end of the 1980s, this had gone so far that some Liberal Arts professors claimed that PhDs from the leading graduate schools were no longer equipped them to teach at undergraduate level. There were even signs that PhD students themselves were skeptical about the value of the hurdles through which they were jumping (Colander and Klammer 1987, 1990). In response to these concerns, the AEA established a Commission on Graduate Education in Economics, which reported in 1991 (Krueger 1991; Hansen 1991; see Coats 1992a for a comparison of this and Bowen's report). Though recommending some changes, these were minor and had little effect (Colander 1992). When Colander (2005) repeated his earlier survey a decade and a

half later, he found much lower levels of dissatisfaction, though concluded that the students had adjusted to the more technical syllabus, not the other way round.

Economic analysis

The way the use of mathematics spread within economics was inextricably linked with developments in economic analysis. It was not simply a matter of making earlier, less rigorous, analysis more precise. To be able to use the mathematical techniques in the way they did, the basis on which economics rested had to change. For the Institutionalists, very broadly interpreted, being scientific meant basing economics firmly on evidence about how the world worked. It was because he believed that empirical research would establish accounts of human behavior that were more complex than those offered by economic theorists that Mitchell (1925) had predicted that economists would lose interest in an abstract, artificial man. The result was that the 1930s and 1940s saw a wealth of empirical work on industrial organization, pricing, labor markets and many other aspects of economic behavior. But mathematical theory, given the techniques then available to economists, necessitated working with simpler assumptions, in which agents were maximizers operating in markets where competitive structures were precisely defined, and if possible were perfectly competitive. It is perhaps because of the strong institutionalist element in American economics, that the debates through which these simplifying assumptions were established were dominated by American economists.

The clash between institutionalism and new, technical approaches explicitly came to the surface in Koopmans's review (1947) of *Measuring Business Cycles*, by Arthur Burns and Wesley Mitchell (1945). Koopmans presented his approach as building on the work of those like Burns and Mitchell who simply measured: it was necessary to pass beyond that to the "Newton stage" in economic theorizing, where theory and data analysis informed each other. Data would be used to test theory. Representing the old view, Rutledge Vining (1949) pointed out that the Cowles Commission methods involved more than this: that they presumed a specific type of theory and empirical methods. If one did not know what theory was suitable, different empirical methods were required. Burns and

Mitchell, Vining argued, were concerned with discovery as much as with testing, for in the absence of empirical work, economists did not know what form theory should take.

Substantially the same issue arose in the so-called “marginalist” controversy, provoked by Richard Lester’s article in the *American Economic Review* (1946). Though Lester was portrayed by critics as drawing naïve conclusions from surveys, and as presenting a radical challenge to profit maximization, he is better seen as arguing that economics needed to be based firmly on the mass of evidence that had been accumulated during the previous decade or more on how firms behaved and on how labor markets worked. Controversy here was more prolonged and more complex, spilling over into discussion of industrial organization, where Lester’s critics included economists on both sides of the Harvard–Chicago divide (see Lee 1984; Mongin 1992). Fritz Machlup changed the debate into one about marginal analysis and, together with Friedman, established the principle that economics was about explaining behavior, not explaining how decisions were made. Though he did not intend it that way, Friedman’s (1953) methodology of positive economics, with its emphasis on testing predictions, not assumptions, could be taken as vindicating economic theory’s neglect of its empirical foundations.

It is no coincidence that these controversies took place in the pages of US journals, as did a less prominent one a few years later on the role of mathematics in economics (Novick 1954 and ensuing discussion; c.f. Mirowski 2002: 402-5). There were parallels in other countries, but it was in the United States, where in the 1930s Institutionalists and Neoclassicals had vied with each other, that the most marked cleansing of approaches that could not be formalized so easily was taking place. In the 1950s and 1960s, formal modeling based on maximization and, increasingly, competitive markets spread throughout the discipline. Price theory became more formal and increasingly dominated what came to be known as microeconomics. Keynes’s behavioral microfoundations were replaced with optimizing ones and macroeconomics came to be seen through the lens of utility maximizing agents, as in Don Patinkin’s *Money, Interest and Prices* (1956). General equilibrium analysis, summed up by Debreu’s *Theory of Value* (1959), though never more than a minority activity, came to be seen as the fundamental theory on which more workaday theorizing rested.

During this period, there were limits to the application of formal theory. Strategy and industrial structure still remained outside its purview, empirical work dominating industrial organization. Though formal microfoundations could be provided for many of the functions, macroeconomics was seen as separate, not entirely reducible to formal microeconomic theory. Development was also *sui generis*. However, from the 1970s, things changed. Formal methods, based on individual optimization, were used to analyze problems of uncertainty and information, the most prominent exponent of this being Joseph Stiglitz. These ideas were applied to labor markets, finance, and many other fields. Macroeconomics turned to rational expectations, abandoning what Lucas called “free parameter” models – ones containing parameters that were not based on optimizing behavior. Public choice theory, which emerged at the boundaries of economics and political science, brought government and much organizational behavior within the scope of rational choice (see Medema 2000). Initially, this was not widely considered to be economics, with the result that public choice scholars found it hard to publish in the major journals, leading James Buchanan, Gordon Tullock and their associates to establish the Public Choice Society, and to develop their own journals. However, fairly soon, the main economics journals opened up to such work. Methods were found to build models of general equilibrium with monopolistic competition, these enabling trade theory to move away from assumptions of perfect competition that were thought unrealistic in many contexts. Game theory was introduced to analyze problems of strategy, first transforming industrial organization and later being extended to almost every other field of economics. Development economics, like macroeconomics, ceased to be considered as resting on principles different from those in the rest of the discipline.

Most of these developments were international in their reach, but all were centered, squarely, in the United States. In none of the cases just mentioned would it be conceivable to write the story without discussing the role of American economists and economists based in the United States, whereas it would be possible (if not always the full picture) to do so without mentioning work in the rest of the world. The collective effect of these developments was to transform a discipline in which, though

rational, maximizing behavior was central, numerous exceptions and special cases existed, to one where it could plausibly be argued that economic theory was simply working out the implications of maximizing behavior. Economic theory could be seen as resting on a single behavioral postulate.

Faced with this scenario, in which economic theory in the United States became, methodologically, narrower, some economists rebelled. Radical economists, stimulated by the Vietnam War, began to argue in the late 1960s, that economists were systematically ignoring questions such as power, class, and income distribution. Frustrated by their inability to persuade their more orthodox colleagues to take their ideas sufficiently seriously, they formed the Union of Radical Political Economy, setting up networks, conferences and a journal (Coats 1992b; 2001). Shortly afterwards, inspired by Joan Robinson's Ely Lecture at the AEA in 1971, Alfred Eichner, Jan Kregel and others organized what developed into the grouping known as Post Keynesian economics (Lee 2000). "Austrian economists", influenced by the work of Ludwig von Mises, Friedrich Hayek, and Ludwig Lachmann, encouraged by Hayek's being awarded the Nobel Memorial Prize in 1974, also began to organize themselves. The case of Public Choice has already been mentioned. In all cases, the organization of these groups was motivated by the sense of exclusion they felt from the mainstream, represented by the meetings of the American Economic Association and the leading economics journals, who regarded their work as generally of low quality. These movements remained small, with strengths in particular institutions (Austrians at New York and Auburn; Post Keynesians at Rutgers and Tennessee; Public Choice in Virginia; Radicals at Amherst and the New School).

These self-consciously "heterodox" groups were but part of a wider fragmentation of the discipline. Technological changes meant that economic print runs for books and journals fell during the period, and the costs of travel and communications fell. Together with the increases size of the economics profession, these developments made it easier for sub-fields of economics to organize, represented most clearly by the rapid rise in the number of specialist journals. The changing character of the profession was reflected in the Allied Social Science Association, the main professional meeting of American economists. At the outset, this had involved the AEA and the American Finance

Association, but by the end of the century it had become an umbrella comprising not just these large societies, but a plethora of smaller ones, each organizing its own programme.

The 1970s and 1980s were arguably years of integration, when the American economics became more homogeneous, the core of microeconomics based on individual optimizing behavior being applied to more and more. There was, more than ever before, certainly dramatically different from the situation before the Second World War, an orthodoxy. However, in the 1990s this began to break down. The main reason was arguably experimental economics, considered esoteric as late as the 1970s, but given respectability with the debates over preference reversals in the pages of the AER and the award of the 2002 Nobel Memorial Prize to Daniel Kahneman and Vernon Smith. This rapidly spread throughout the profession. When “behavioral” economics started being taken seriously in finance, a field where predictive power was always paramount, it was a sign that alternatives to conventional views of rationality were being taken very seriously. Bounded rationality, on which Herbert Simon had been working since the 1950s at Carnegie Mellon, and for which he got the Nobel Memorial Prize in 1978, moved from being something idiosyncratic, if respected, to being a mainstream technique. Evolutionary game theory and complexity theory offered new ways to think about economic change that expanded the boundaries of what was accepted in the subject. By the end of the century, though rational choice models remained immensely strong, it became much harder to describe economics as dominated by an orthodoxy. Once again, though these developments were international in their scope, they were centered on the United States, just as were the developments of the 1970s. Equally radical ideas, such as the competing views of consumer theory associated with Werner Hildenbrand, whose main supporters were in Europe, had far less influence.

Economists, ideology and policy

Ideology was never far from the surface. In the 1940s concern with “Reds” was common in the United States, though economists might consider the problem only to dismiss it. After 1945, as the Cold War developed, these concerns with communism grew, reaching their peak with Joseph

McCarthy's House Un-American Activities Committee. Economists had frequently been viewed with suspicion amongst businessmen, some of whom were important patrons of higher education, but whereas those with radical views would previously have faced local problems, in the 1940s or 1950s such problems were more widespread. Planning was suspect, a legacy from the days of the New Deal, and Keynes provided a convenient focus. Keynes was a more real threat than Marx – the Englishman who ruled America according to the *Chicago Tribune* (see C. D. W. Goodwin in Morgan and Rutherford 1998 on these episodes). Prominent Keynesians, from Galbraith to Samuelson were vilified and labelled Marxists or communists. Textbooks, such as those of Lorie Tarshis and Samuelson, that discussed Keynesian theory were attacked and sometimes removed from syllabi under pressure from aggrieved sponsors (Colander and Landreth 1998).

The cases where economists were forced out of academic positions because of real or alleged communist sympathies are comparatively easy to document (Goodwin 1998; Lee 2004). What is much harder to prove is the effect this had on how economics pursued their work. There were certainly great pressures to be technical, for arcane communications between specialists were much less likely to be considered suspect than ideas that reached out beyond academia. Using an evolutionary model, Goodwin 1998:79) distinguishes between “conceptual variation” and “intellectual selection”, arguing that the attitudes of economists’ patrons must have influenced the latter. However, doubts about its closeness to communism did not prevent Keynesianism from becoming widely accepted in academia, though that may have contributed to its being expressed in more careful, technical language than might otherwise have been the case (see Samuelson in Colander and Landreth 1996: 172).

This bias towards becoming more technical chimed with another pressure – to be seen as doing science. When the National Science Foundation was established in 1950, the inclusion of social science was controversial and did not take place till 1956. If economists were to obtain support, they had to ensure that their work was seen as scientific. Given prevalent beliefs about science at the time, this favored narrower, more technical work, and worked against the pluralistic interdisciplinarity that

had been more common in before the war (Goodwin 1998:65-7). Similar issues arose in the context of support by philanthropic foundations, of which Sloan, Russell Sage, Rockefeller and Ford were the most important. Here, concern with being rigorous was intertwined with suspicion of planning and doubts about Keynesianism.

Similar considerations affected the body that brought economists into the heart of the US government, the Council of Economic Advisers (CEA) established by the Employment Act of 1946. This was intended to be a conservative institution, providing expert advice with minimal government interference. Its first chair, Edwin G. Nourse, shared this view: unlike his colleagues on the CEA, he was careful to avoid being seen as an advocate for White House policy (Bernstein 2001: 110-11). Unlike his successor, Leon Keyserling, he viewed economics as providing technical, disinterested expertise. Despite criticism, the CEA survived, achieving its greatest influence in the Kennedy administration, when Walter Heller, Kermit Gordon and James Tobin applied Keynesian demand-management policy to the problem of reducing unemployment.

Given that Lyndon Johnson would not let it compromise his Great Society program, the escalating war in Vietnam led to rising federal deficits. CEA members warned the President about the consequences of this, but its Keynesian policies were nonetheless blamed for the inflation and dislocation that followed during the 1970s. During this decade, as the academic profession slowly moved away from Keynesianism, prominence in policy making shifted from the CEA to the Federal Reserve, the Treasury and other government agencies. Under Paul Volcker and Alan Greenspan, short term stabilization policy centered increasingly on the Fed. This marked not a decline in the influence exerted by economists, but a change in its structure. There was a convergence between research done in academia and in central banks (see McCallum 2000: 123). Economists increasingly saw their role not as engineers advising on how to operate fiscal and monetary levers, but as designers of institutions and of systems that would achieve desired outcomes in a world where policy makers were seen as part of the system rather than outsiders manipulating it. Frequently this involved creating new markets, or “re-inventing the bazaar” (McMillan 2002).

There were also more conscious attempts to impose an ideological agenda on economics. RAND, the most influential think tank in the 1950s, became closely involved with the Ford Foundation (these arguments are developed in Amadae 2003). It was explicitly a non-political organization, directed towards impartial research. However, under the chair of its board of trustees, H. Rowan Gaither, also president of the Ford Foundation after 1953, RAND focused on “systems analysis”, based on principles of rational action. Rational choice, central to the work of RAND since its inception, can be seen as providing an justification for a free society, and an alternative to communist collectivism. RAND’s ideology, like that of Ford, was one of technocratic management, by experts using rigorous quantitative techniques. This ideology became prominent in government in the 1960s when applied by Robert McNamara, who came from the Ford Motor Company, as Secretary of Defense.

Others had a more explicit ideological agenda. The American Enterprise Institute (1943), The Foundation for Economic Education (1946) and the Liberty Fund (1960) were established specifically to propagate free-market ideas. There were followed in the 1970s by a series of think tanks specifically to develop and apply such ideas to policy. The Heritage Foundation (1973) was specifically seen as providing a counter-weight to the Brookings Institution (established 1927), which had come to be seen as part of finely-tuned Liberal policy-making machine. The aim of its president, Edwin Feulner, was to create “a new conservative coalition that would replace the New Deal coalition which had dominated American politics for half a century” (L. Edwards, quoted in Backhouse 2005: 371). When Ronald Reagan took office, the Heritage Fund provided policy ideas ready to put before the new administration. Hayek, who had moved to Chicago in 1950, played a particularly influential role in stimulating such organizations, within the United States and elsewhere, being part of an influential network centered on the Mont Pelerin Society, an international group of libertarian thinkers established in 1947, whose founders included four Chicago economists and representatives from the Foundation for Economic Education.

Businessmen and conservative foundations also sought to stimulate free market thinking within economics, many of them effectively targeting specific institutions and programs. Though tiny

compared with the big foundations such as Rockefeller and Ford, the Volker Fund (which supported Hayek and Mises), the Earhart Foundation (with a program of one-year fellowships), the Scaife, Bradley and Olin foundations (which between them targeted support at, *inter alia*, at the Chicago's Law and Economics program, and various centers of public choice theory in Virginia) managed to achieve influence out of proportion to their size (see Backhouse 2005).

The international dimension

After the Second World War, the United States dominated the economics profession. German economics had not only been decimated by the Nazi Party, but the resulting emigration contributed enormously to the expansion of American economics. The United States was not the only home for German and other European exiles, many moving to Britain, but it received more than any other country [statistics from Hagemann?] Britain experienced no such loss, but its university system was too small for it to be a serious rival. The result was that many ideas that had originated in Europe rapidly came to be associated with the United States. The clearest examples of this are general equilibrium theory and econometrics, where European emigrés, led by Jacob Marshak, were instrumental in developing ideas that rapidly lost any close connection with their European origins. For example, in the 1930s, general equilibrium analysis had been an almost exclusively Austrian phenomenon, whereas by the 1950s its leading practitioners were an American and a Frenchman, but both based in the United States.

Another clear example of this process is Keynesian economics, central to the evolution of American economics from the 1940s to at least the 1980s. This clearly originated in Britain, and British economists such as John Hicks and James Meade played important parts in the subsequent Keynesian revolution. However, Keynesianism was rapidly Americanized. The key figure here was Alvin Hansen, the force behind Harvard's fiscal policy seminar, and later author of the influential *A Guide to Keynes* (1953). As has been argued by Mehrling (1998), Hansen's "conversion" did not involve a rejection of his earlier ideas; rather, Keynesianism provided a vehicle through which his

ideas on policy, rooted in the American Institutionalist tradition, could be developed. Lawrence Klein (1968[1947]) provided another, interpretation of Keynesian economics, relating it to with the econometric approach emanating from the Cowles Commission. Samuelson (1948) integrated Keynesian ideas into a textbook aimed at American students. During the 1950s and 1960s, the most influential work on macroeconomics was, with few exceptions, undertaken in the United States. Friedman's work on the consumption function provides another example of Keynesian ideas being assimilated into an American tradition (the empirical studies of the National Bureau).

What was happening here is that economics was becoming more international, but centered on the United States, a development made possible by the openness of the American system at a time when the profession was expanding and opportunities for immigrants were great. The United States dominated, not simply because of its size, but because of its resources. During the inter-war period, the Rockefeller Foundation had been instrumental in building up economics in key European institutions in Britain, Scandinavia and many other countries [ref Goodwin ?]. After 1945, given the close American involvement in Europe that resulted from the war and reconstruction, this influence increased, accelerated by the reduced cost of international travel. In country after country, the economics profession changed in several ways. Academic systems became more open and competitive, with increased emphasis on publication in journals. There was a movement away from publication in the native language towards publication in English. Journals moved away from being national organs to ones that published articles by economists from a wide range of countries. Graduate education moved towards the American model, away from the traditional European model of a major thesis, publishable as a book, towards a PhD comprising advanced coursework and a short thesis that could be the basis for three journal articles. The mathematical demands made of students rose progressively. Many economists either undertook postgraduate study in the United States or spent sabbaticals in US universities.

The speed and extent of these changes varied enormously (see the case studies in Coats 1997). For example, in the UK, the proportion of staff with a degree from an American university rose steadily

from 1950 to the 1990s. The highest proportion was at the London School of Economics, where it reached 45% by the mid 1990s, whereas in other universities it was only 5%. In Belgium, CORE at the Université Catholique de Louvain, was an important center for economists with strong US connections. Similarly, there was variability in the speed with which PhD requirements changed, some British universities adopting the American model in the 1950s and 1960s, whilst others did not require any coursework beyond undergraduate level till the 1990s. In continental Europe, there was the complication of language, and in many cases of academic systems that were much more rigid and less rapid to change, but many of these changes still took place. Outside Europe, there was the further factor of decolonization. At the end Second World War, many countries were still closely linked to former colonial powers, and the changes involved a switch from those to the United States.

There is dispute over whether this process should be labeled “Americanization” or simply “internationalization” (c.f. Coats 1997: 395-9). The process certainly did involve internationalization, and it was arguable that many changes (such as the move towards advanced coursework) was necessitated by the rising technical demands made by the subject. As has already been explained, many of the ideas on which the period’s economic was based were European, not American in origin. Arguably, the US appeared to dominate what was primarily an international system simply because of its size. However, there are strong reasons for considering the process as involving Americanization. In many cases, in Europe and elsewhere, the United States provided an example that was deliberately copied. In other cases, changes were brought about through through connections with US universities. Harry Johnson, Canadian, but a professor at Chicago and Geneva, was important in bringing about changes at LSE in the 1960s. Chicago economists developed close links with Latin American countries, consciously exporting Chicago economics to Chile: Chilean students studied in Chicago, and Chicago staff taught at the Catholic University of Chile (see A. Harberger in Coats 1997; Valdes 1995). Similar developments took place in Brazil, though involving a much wider range of universities: Chicago, Berkeley, Harvard, Yale, Michigan, Illinois and Vanderbilt (see M. R. Loureiro in Coats 1997). The US Agency for International Development and the Ford Foundation provided a

significant role in funding several of these inter-university agreements.

Similar remarks could be made about the US role in the international organizations that emerged after 1945, notably the International Monetary Fund (IMF) and the World Bank: they were vehicles for the internationalization of economics, along a model dominated by the United States.

Conclusions

Since the Second World War, economics in the United States has been transformed as dramatically as in its previous half-century. This transformation involved not only a transformation of the ideas held by economists, but also transformations in the size of the American economics profession, its place in American society, its relationship to government and its relationship to economics in other parts of the world. Establishing causal links between such changes is notoriously difficult, not least because of the complexity of these developments, so any conclusions must of necessity be very cautious. In part this is because there are many stories that can be told. Economics became more technical because this enabled economists to solve problems that they would otherwise have been unable to solve. However, this begs certain questions, such as why it became technical in the way that it did, and why economists accepted those problems as ones that were worth solving. To probe beneath this story, one can explore the backgrounds and settings of the economists themselves, and of their patrons, their ideologies. [Material on funding regimes, citing Mirowski and Sent to be added.] There is also the question of the role played by sheer historical accident in determining the way in which the profession evolved.

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