Introduction: Telling the Story of MIT Economics in the Postwar Period

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On October 3, 1940, Paul Samuelson's doctoral mentor, E. B. Wilson, wrote to him saying that he had heard that Samuelson, newly appointed as a Harvard instructor, had just received an offer to join the MIT faculty as an assistant professor of economics. In the letter, discussed in detail by Roger Backhouse in this volume, Wilson recalled that he himself, as a first-year assistant professor at Yale, had received an offer to go to MIT as an associate professor, had accepted the offer, and "was never sorry that I did it." He went on to say:

> I have thought a great deal about the situation in economics at Tech. When [Francis Amasa] Walker was President . . . the prospects for economics were extremely good. [Since Walker's death] the [economics] staff at Tech hasn't been notably statistical or mathematical and has in no way adequately capitalized in their instruction [on] the background of their students, which consists of 2 years of required mathematics, 2 years of required physics, 1 year of required chemistry, and a year of required mechanics. . . . It seems a much more powerful course on economics could be given if this background were thoroughly used. . . . I do think it shows extremely keen intelligence on the part of Tech [and Freeman] to try to get you. . . . What will happen at Tech I don't know but they are starting out well if they secure you.

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History of Political Economy 46 (annual suppl.) DOI 10.1215/00182702-2716091

> And in a follow-up letter dated October 14, Wilson concluded:
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> I expect you would go ahead fast at Tech, perhaps fast enough so that you won't be thinking about going anywhere else as was indeed my own experience. If you stay there as the Tech continues to appoint persons like you to its staff then by the time you are in early middle life there may be a very great change in the instruction at Tech and the Tech may have a really distinguished research department as it now has in mathematics, physics, and chemistry.

Wilson's prediction, made in late 1940, was prescient. Samuelson did stay at MIT, and the department, bolstered by a number of appointments of technically strong scholars, became, by the late 1950s, one of the three or four most distinguished research departments of economics in North America. In another decade it would become the most highly regarded economics department in the world. The history of this process, and the important paper that provided the common background understanding for the conferees, was developed by Beatrice Cherrier (this volume) from multiple archival sources. At the same time, MIT's rise to prominence coincided with the remarkable transformation of American economics in the postwar period. Cherrier is the first historian of economics to take up the narrative challenge of situating the particular circumstances of MIT in the context of that transformation.

Over the past twenty-five years the Duke history of economics faculty, together with the collection development librarians (particularly Robert Byrd and Will Hansen) in the David M. Rubenstein Rare Book and Manuscript Library, have been gathering the papers of notable (mostly) twentieth-century economists in what is now called the Economists' Papers Project. Over time that archive has grown and become central to historical research on economics in the postwar period. The papers of Edwin Burmeister, Evsey Domar, Franklin Fisher, Duncan Foley, Lawrence Klein, Franco Modigliani, and Robert Solow, all MIT faculty or students, have attracted scholars from around the world. After Paul Samuelson's death in December 2009, his papers, by prior arrangement, came to the Economists' Papers Project and quickly became a magnet for historians of economics. In response, early in 2010 I was encouraged by my colleagues Bruce Caldwell, Neil De Marchi, Craufurd Goodwin, and Kevin Hoover to plan a conference in the *HOPE* annual conference series to examine the history of MIT economics. After a year's worth of conversations and e-mails, I invited a select group of scholars to consider MIT's role in the

transformation of American economics in the postwar period. That conference, held April 26–28, 2013, at the R. David Thomas Conference Center at Duke University, was sponsored as usual by Duke University Press. However, the very generous financial support of the Alfred P. Sloan Foundation made possible the expansion of the "standard" *HOPE* conference into one that included a larger number of participants and papers. In the end the conferees learned that telling the story of MIT's role in the postwar period required attending to both the particular circumstances that shaped MIT and the various ways in which economics itself was changing.

The Historiographical Challenge

Stories in the history of science engage both narratives of continuity and narratives of disruptive change. Historians of economics, who have long employed such a distinction, appear to agree that the 1940s saw a major break between an older economics and a newer economics. Before World War II, even as economics encompassed diverse approaches, methodologies, and theories, most economists still employed the same kinds of tools, studied the same texts, examined and reexamined a stable canon, and shared common educational goals and practices. Rhetorically, they were indistinguishable. Aside from the early issues of *Econometrica* (and later the *Review of Economic Studies*), most books, journals, articles, and reviews published in the 1930s exhibited their literary nature even as they incorporated occasional geometric and descriptive statistical arguments. By the mid-1950s, in contrast, most prominent published works had a "scientific" character and featured mathematical or econometric arguments in support of conjectures and hypotheses. These changes were sufficiently well recognized at the time that the community of economists sought to impose new standards for those seeking professional credentials as economists.

Over the next half-century historians of economics sought to characterize this disruption and explain its sources and consequences. Different historians examined this transformation in different ways, not all of them consistent with one another. The historiography is additionally complicated because the 1940s are recent enough that a number of the central figures themselves provided accounts of the transformation to the historians. But as is true for many histories of contemporary science, the tension between historians' and participant observers' interests complicates the process of constructing compelling accounts of the postwar

period. Telling the story of modern economics must begin by interpreting the discontinuity in both the body of economic knowledge and the image of economic knowledge held by economists in the postwar period.[1] But there are many interpretations.

If the specific task here is to tell MIT's story while employing (or rejecting) the discontinuity stories, it will be useful first to look away from MIT to describe how scholars have addressed the general history of the postwar period and the changes both in economic knowledge and in the community of economists between the 1930s and the 1950s. After framing those issues, we can better see how the conferees addressed the ways in which the MIT story is consistent with, and yet different from, those often-told tales.

Competing Narratives

There are three well-established narrative approaches to telling the story of how economics changed in the postwar period. Probably the oldest story of the transformation talks about the world before John Maynard Keynes's 1936 book *The General Theory of Employment, Interest, and Money* and the world after Keynes's book appeared. An old example of this is G. L. S. Shackle's 1967 volume *The Years of High Theory*. Certainly Lawrence Klein's *The Keynesian Revolution* ([1947] 1966), an extension of his doctoral thesis written at MIT under Paul Samuelson, contributed to this way of thinking, as did Keynes himself in his 1936 book, which opened by characterizing "classical" economists as every economist before him. Samuelson himself, who had learned Keynesian economics from Alvin Hansen at Harvard, was a member of a generation that saw, in Keynes's theory, a way out of the Great Depression. Thus by the time Thomas Kuhn's *Structure of Scientific Revolutions* appeared in 1962, economists had been referring to the Keynesian revolution for two decades and were able to appropriate Kuhn's arguments as fitting the Keynes case: the story became one in which the normal neoclassical science through the 1920s faced the crisis of the Depression, and Keynes's revolutionary paradigm-changing theory created a new kind of normal economic science by the 1940s. This story required that economics

1. The distinction between the body of knowledge and the image of knowledge was developed by the historian of mathematics Leo Corry (1996; following Yehuda Elkana) in his study of Bourbaki and algebra based on several of his earlier papers. I have employed it to discuss the interconnection of mathematics and economics in Weintraub 2002.

divided sharply into before and after Keynes. Even arguments about the changing nature of econometrics between the 1930s and 1950s, changes deplored by Keynes and his associates, have been cast in terms of this Keynesian revolution (Louça 2007). Similarly, the development of national income accounts by Simon Kuznets, James Meade, and Richard Stone was made necessary by, and facilitated, the Keynesian revolution, while wartime planning employed Keynesian categories, and statistical data collection post–World War II reflected these new features of economic life. The Keynesian revolution in this standard account produced a change in the nature of economic policy, as economists became embedded in governments as technicians and analysts. In the United States, the Employment Act of 1946 reified this transformation with the creation of the Council of Economic Advisers and the Economic Report of the President.

Several articles in this conference volume engage this master narrative to a greater or lesser degree. Perry Mehrling argues that the Keynesian allegiances of Samuelson and Modigliani generated a particular kind of monetary analysis and policy. The growth theory that grew from Robert Dorfman, Samuelson, and Solow's 1958 volume on linear programming, and Solow's 1957 paper, was neoclassical in the sense of the neoclassical synthesis, but that synthesis itself involved Keynesian theory, for it was that which was "synthesized" to neoclassical theory. This work at MIT is discussed in the articles of Verena Halsmayer and of Mauro Boianovsky and Kevin D. Hoover.

The Keynesian narrative both frames discussion of the transformation of economics and enables new kinds of inquiries. For example, important new work in economics would be located in those academic institutions that more quickly adopted Keynesian ideas. Assimilation of Keynesian ideas at the textbook level created a new generation of elementary textbooks beginning with Lorie Tarshis's and Samuelson's, published in 1947 and 1948, respectively. This is the subject of Yann Giraud's article in this volume. The "new" economics can be traced back to Keynes and to those who surrounded him at Cambridge, at least according to historians with some connection to Cambridge. The complex but confusing relationship between these two Keynesian networks is the subject of Roger Backhouse's article deconstructing the Cambridge capital theory controversy.

A second framing narrative emerged from the important *HOPE* conference volume edited by Mary Morgan and Malcolm Rutherford titled *From Interwar Pluralism to Postwar Neoclassicism* (1998). A number of scholars had become dissatisfied with histories characterizing the

twentieth-century transformation of economics in the United States as a struggle in which institutionalism lost adherents with respect to an ascendant neoclassicism. Scholars like Rutherford (2003) and Yuval Yonay (1998) began to reconstruct the interwar period as one of theoretical diversity. The lacunae in previously told stories were that neither institutionalism nor neoclassical economics had been monolithic. Demand theory and production theory and the theory of the firm had been contentious and contested in the interwar years. By the mid-1950s, however, they were settled chapters in intermediate and graduate microeconomics textbooks. For historians of economics, the emergent question was, "How did postwar microeconomics become stabilized?" "Keynes" could not be the answer to this question. From an interwar economics in which a large number of themes and threads in economic theory were not necessarily consistent with one another, the 1950s presented a coherent vision of what had become microeconomics. The stability of neoclassical economics became the end point to be explained. Accounts of that particular stabilization process became a more or less convincing set of arguments according to which historians narrated the transformation of economics. The most prominent of these arguments was that mathematical models and the use of econometric techniques to provide empirical tests of those models untangled the various theoretical bafflements, puzzlements, and conundrums. Simple geometric models and descriptive statistics no longer sufficed to establish an economist's claims. The postwar microeconomics was stabilized by the increased use of mathematics and statistics in economic analyses (Weintraub 1991, 2002). The role of models became more important, and the role of value judgments and ethical concerns became less important in undergraduate teaching, graduate curricula, and the socialization of new entrants into the economics profession. The postwar period witnessed a new rhetoric of economics. The articles in this volume by (again) Halsmayer and Harro Maas engage these important matters.

Perhaps the increased importance of more formal, more explicit models in this period was associated with the growing epistemological awareness, even self-consciousness, of economists in this time of methodological change. Samuelson's references to Percy Bridgman's "operationalism" and Karl Popper's "falsificationism" as presented to economists by Terence Hutchison required something theorylike, which in economics could only mean a model, to be subject to tests in order to claim scientific status (Morgan 2012). Despite the residual differences among national economics communities (Fourcade 2009), economics became an international dis-

course whose problems and answers were stabilized by an accepted set of techniques. Historians of economics working within this conceptual space would focus on the emergent methodologies associated with the new scientific economics and on the increased technical nature of the subject.

Since MIT's economics department was identified with this new technical economics, the conferees spent time detailing the paths along which these analyses changed the intellectual values and the rhetorical strategies of the community of economists in the postwar period. The articles by Andrej Svorenčík and Pedro Garcia Duarte, in particular, examine the movement of both people and ideas inside and outside the MIT economics department.

A third historiographical framework employed to explain the transformation of economics in the postwar period grew from political and sociological concerns. Stories told from this awareness recognized that prior to the war, economics had been connected in a disciplinary fashion to particular national traditions in countries that had been producing economics and economists. As a result of the wreckage of World War II, the United States became the dominant producer of non-Marxist economic analyses. America's hegemony stabilized economic discourse by replacing various national traditions with the emergent traditions of the American economics community. Stephen Meardon's article on Charles Kindleberger engages these matters. Only the United States had the resources to educate, hire, and train economists, and to publish research in economics on a massive scale. Over time, economics stabilized in the sense that disparate national traditions were submerged and marginalized with respect to an American mainstream economics. Many of those traditions lived on, of course, as heterodoxy. Two particular examples were the UK Post Keynesian group around Joan Robinson at Cambridge (Backhouse, this volume, "The Other Cambridge"), and the Austrian, neo-Austrian, and Chicago group around Friedrich Hayek that found a home among the small band of neoliberals connected to the Mont Pelèrin Society. Aside from Chicago economics, the heterodox traditions were not well regarded and in fact produced few if any serious connections to the reward structures of mainstream economics, and were not represented at MIT.

These three framing narratives of the postwar change in economics—the Keynesian revolution, the rhetorical stabilization of mathematical and econometric model-based economics, and the Americanization of economics—are of course neither mutually exclusive nor exhaustive. Thus there was open space in the last decade for constructing alternative

narratives. Some work by a small number of historians of economics as well as some members of the larger science studies community built from the Americanization theme and had as one of its necessary implications that the economics discipline's postwar emphasis on building and analyzing models was, in Andrew Pickering's (1995) terms, a forced move and not a free move.[2] A few scholars strongly critical of both mainstream economics and mainstream economists went farther and characterized postwar economics as an intellectual disaster shaped and controlled by Cold War ideology. Philip Mirowski, particularly in his *Machine Dreams* (2002), and Sonja Amadae in her *Rationalizing Capitalist Democracy* (2003) argued that the contingency of the Cold War determined the nature and institutions of postwar economics. Of course no one denies that economists, brought into service during the war to analyze resource allocation problems—shipping paths, antiaircraft fire, convoy movements, antisubmarine warfare, logistics, military inventories, quality control of military production lines, and so forth—helped create a new kind of analysis soon to be termed operations research. But Mirowski and Amadae deride RAND, Cowles, and foundations such as Ford and Rockefeller as witting or unwitting contributors to the American Cold War project. From this fourth narrative's perspective, game theory, which would eventually unify large areas of what had become neoclassical economics, was military Cold War in origin and instantiated assumptions about human behavior that made it impossible to countenance disparate and more generous visions of human action. This historiography emphasizes the role of money flows from the military, grants, contracts, and personal networks of Cold Warriors in the emergent economics. In an article in this volume rejecting a number of Mirowski's claims, William Thomas examines how operations research developed at MIT. By establishing that it did not develop at all in MIT's economics department, Thomas refutes Mirowski's central claim.

A fifth narrative line, not often explored by historians of economics, likewise developed from the contingency of World War II. The sleepy backwater that was the American university prior to the war was transformed not only by the new world of Vannevar Bush and science funding connected to a Cold War interest but also by the most remarkable piece of social legislation in the immediate postwar period. The Servicemen's

2. A free move is an activity "in which scientists display choice and discretion [which are] aspects of human agency. . . . [A forced move is] where the discipline asserts itself . . . [and] where scientists become passive in the face of their training and established procedures" (Pickering 1995, 116).

Readjustment Act of 1944, known informally as the GI Bill, reshaped American higher education. Colleges and universities had been moribund during the Great Depression as they reduced staff and compensation and postponed thoughts of hiring new faculty and admitting more students. In the prewar period they had trained graduate students in such small numbers that there were few instructors available to teach the postwar flood of fee-paying undergraduates applying for admission.[3] It was at this time, for instance, that Lionel McKenzie, recently demobilized and facing a half year of unemployment before he could take up his Rhodes Scholarship to Oxford, used his Princeton master's degree in economics to secure a position teaching industrial economics to MIT undergraduates. New economists were needed as teachers, as government analysts, and as business economists. Economics graduate programs proliferated. The large numbers of undergraduate economics students forced changes in styles of instruction as well: tools and techniques were teachable in a way that a moral philosophy–based economics was not. Pedro Teixeira's article deals precisely with these matters, as does Giraud's. The GIs were on average older and more in need of workplace credentials than had been the stereotypical elites who were thought to populate the Ivy League establishments or the fraternity brothers and sorority sisters who inhabited many American state institutions before the war. These new students were also "better" than earlier cohorts: the best students from a large pool of applicants will be at least as good as the best students from the included smaller pool. This influx of students not only called forth a new generation of college teachers but also had profound implications for the kind of economics done. Universities became instrumental in securing employment for returning veterans fearful of being underqualified for employment. The returning veterans sought credentials and useful knowledge. A classical humanities–based education was a luxury that these older students could not afford. The demand for economics courses and for business courses exploded (Augier and March 2011). From a prewar liberal arts education that looked down on economics as a practical subject fit only for those unqualified by breeding or availability of leisure to study the humanities, postwar economics became an important academic discipline.

3. One example will suffice to tell the tale. The number of economics students at the University of Michigan jumped 120 percent in 1945–46, and jumped another 89 percent in 1946–47, peaking at 9,100 students in 1947–48. Prewar, that university had produced on average two PhDs in economics per year since 1920, which meant that there was no supply of classroom-ready economics teachers after 1945 (Brazer 1982, 206–7).

While there have been few sustained contextualizations of the changes in postwar economics that draw extensively on these educational and institutional matters, and fewer still by historians of economics, two recent contributions along these lines have drawn some attention from both historians and economists. The sociologist Marion Fourcade (2009) examined the professional discipline of economics in the United States, Britain, and France from the 1890s to the 1990s, and wove the different social and educational institutions of each country into her narrative of change in economics. More recently, scholars have engaged with an excellent discussion of these issues with respect to business education. The organization theory scholar Mie Augier and the education theorist James March, in their *Roots, Rituals, and Rhetorics of Change: North American Business Schools after the Second World War* (2011), tell a new story of a changing economics in the postwar period through a history of the growth of business schools in that period. Many of the topics alluded to in the various historiographies of economics were present as well in business education, and the prewar-postwar break was even more significant in business than it had been in economics. But of course the fate of economics and business education were intertwined.

The MIT economics department was not insulated from more general challenges that faced both universities and the community of economists. Its response to the profession's shifting away from history is the subject of Peter Temin's article on the role of economic history in the MIT program. With respect to the discipline's response to past discrimination against black applicants to many graduate schools, and the resulting absence of black economists on research universities' faculties, MIT's attempts to address this were, despite good intentions, no more successful than those of other "top" economics departments. William Darity Jr. and Arden Kreeger's article provides a detailed account of this attempt and its aftermath.

Any one or several or all of the five narratives—three older and two newer—can "explain" the growing importance of the MIT economics department in the postwar period. The Keynesian revolution? Paul Samuelson, check. The emergent technical nature of the discipline? The technical training of MIT students, nascent scientists and engineers, and a faculty brought in to teach such students, check. Cold War military funding and the engagement of economics faculty and institutions with both sources of support and professions of national need? The history of MIT and the Rad Lab, and the explosion of defense spending that moved through MIT in this period, check. The international hold of American

economics and institutions in this period? The connection of a primarily technological institution to a postwar world hungry for the training in engineering and technology that MIT could provide, check. An influx of students on the GI Bill transforming both the mission and the nature of MIT as an institution? The creation of an actual economics program at MIT in exactly this immediate postwar period, and the stirrings that would create the Sloan School, check.

Paul Samuelson (2000), in fact, employed two of these narratives in his own accounting:

> Two factors explain our success. One, MIT's renaissance after World War II as a federally-supported research resource. Two, the mathematical revolution in macro- and micro-economic theory and statistics: this was overdue and inevitable—MIT was the logical place for it to flourish. What was not inevitable was that the MIT Economics Department maintained throughout its explosive development a deserved reputation for collegial amiability.

The idea that MIT emerged from "nowhere" in the 1930s to its place as one of the three or four most important sites for economic research by the mid-1950s would appear overdetermined with respect to these several narratives. It would not be inappropriate to develop the story of MIT's meteoric rise to prominence along any of these five narrative axes. And as the conference made clear, there is even a sixth narrative that supports any story of MIT's quick success, namely, that the immediate postwar period saw a collapse—in some places slower, in some places faster—of the barriers to the hiring of Jewish faculty in American colleges and universities. More than any other elite private or public university, particularly Ivy League universities, MIT welcomed Jewish economists. Both Backhouse's discussion of Samuelson's move to MIT, and my own article's discussion of the issue, opened this topic for the conference's consideration.

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Part 1
Beginnings