

The Age of the Applied Economist: The Transformation of Economics since the 1970s

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The Problem

The twenty-first century is the age of the applied economist. Applied work dominates the top economics journals. Citations of ten out of the last twelve John Bates Clark Medal winners describe the recipients as being “applied” or “empirical,” or as doing work of “practical relevance.”¹ Summarizing his experience of PhD students applying for jobs at Princeton, Angus Deaton (2007) has written: “If the typical thesis of the eighties was an elaborate piece of price theory estimated by non-linear maximum likelihood on a very small number of observations, the typical thesis of today uses little or no theory, much simpler econometrics, and hundreds of thousands of observations (The amount of computing time has remained more or less constant).”

The empirical practices in which economists are now engaged include not only traditional econometric work but also laboratory experiments,

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1. John Bates Clark Medal citations can be found at www.aeaweb.org/about-aea/honors-awards/bates-clark, where all medal winners are listed chronologically.

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randomized control trials, analysis of natural experiments, and the building of databases that can be used in different ways. Economists who work in government, central banks, and international organizations such as the World Bank play a significant role in the discipline. The frontiers between the academic world and IT firms such as Google and Microsoft are increasingly porous. This is in dramatic contrast to the situation a half-century ago, when distinguished economists were complaining about the propensity of economists to develop elaborate economic theories, giving no thought to their applicability to the world. For example, Wassily Leontief (1971, 1) complained that “the weak and all too slowly growing empirical foundation clearly cannot support the proliferating superstructure of pure, or should I say, speculative economic theory.” A common criticism was that, rather than engage with the real world, economists typically made assumptions that enabled them to draw their preferred conclusions.² There are still grounds on which to criticize economics, but such charges would be much more difficult to substantiate today. There has been a remarkable transformation that historians have so far not investigated.

The transformation of economics in the last decades of the twentieth century has increasingly been called an “empirical turn” (e.g., Einav and Levin 2014, 716; Rodrik 2015, 201; Offer and Soderberg 2016, 13, 56, 277).³ The argument typically follows a standard form. Data compiled by Daniel Hamermesh (2013), reproduced in table 1, are used to argue that since the 1980s there has been an enormous growth in empirical research and a decline in theoretical research. The argument is then made that computerization and new, more abundant and better data have enabled this transformation. On this basis, the conclusions such as “the death of theory” or at least a “paradigm shift” are announced.⁴ The result is a straightforward causal story.

However, caution is required, for Hamermesh’s data do *not* show that there is more empirical work in economics: it shows that there has been a turn toward empirical work in three of the top journals, that empirical work has become more *prestigious*. Of course, there may well be more empirical work, if only because there are more journals, and other publication outlets and improvements in information technology have dramati-

2. For a catalog of such criticisms, and a response, see Heller 1975.

3. This terminology is even more frequent in blogs, including www.bloomberg.com/view/articles/2016-08-25/data-geeks-are-taking-over-economics or blog.francetvinfo.fr/classe-eco/2016/09/18/2888.html.

4. For example, Wilson 2016 and Cohen-Setton 2015.

Table 1 Methodology of articles published in *American Economic Review*, *Journal of Political Economy*, and *Quarterly Journal of Economics*

Year	Theory	Theory with simulation	Empirical: borrowed data	Empirical: own data	Experiment
1963	50.7	1.5	39.1	8.7	0
1973	54.6	4.2	37.0	4.2	0
1983	57.6	4.0	35.2	2.4	0.8
1993	32.4	7.3	47.8	8.8	3.7
2003	28.9	11.1	38.5	17.8	3.7
2011	19.1	8.8	29.9	34.0	8.2

Source: Hamermesh 2013, 168, table 4

cally reduced the costs of data analysis, but this is not what Hamermesh's data show. It is important to recall that economics has nurtured a strong tradition in empirical work since at least the beginning of the twentieth century, much of it published outside the major journals. It is enough to mention fields such as agricultural economics (important in the early history of econometrics), business cycle theory (Wesley Clair Mitchell and the National Bureau of Economic Research [NBER]), labor economics, and industrial economics, in all of which empirical work was very strong.⁵ New techniques such as national accounting, cost-of-living indexes, input-output analysis, cost-benefit analysis, and finance have involved both the development of new concepts and tools and also large projects aimed at gathering, recording, and making sense of data.⁶ Sometimes the results of such research were published in the leading journals, but often they were published in books (e.g., the numerous volumes of empirical work published by the NBER and the Brookings Institution) or journals published by central banks, commercial banks, and international organizations such as the International Monetary Fund and the World Bank.

Neither does Hamermesh's table show that theory is dying. Making such a case requires tracing the growth of the number of empirical papers where a theoretical framework is altogether absent. Hamermesh and Jeff

5. Much interwar economics was "institutionalist." Institutional economics was heavily empirical, dominated by a commitment to be scientific, which meant being both realistic and developing ideas that could be used for social control (see Rutherford 2011, 22–27; 1999).

6. On national accounting, see Vanoli 2005; Mitra-Khan 2011; and Hirschman 2016, among others. On cost-of-living indexes, see Stapleford 2009.

Biddle (this volume) have done this. They highlight an interesting trend. In the 1970s a large proportion of microeconomic empirical papers relied on a theoretical framework.⁷ Since then, there has been a limited but significant resurgence of atheoretical work in the 2000s, but this has not yet matched the proportion of atheoretical papers published in the 1950s.⁸ Three conclusions would appear to follow.

First, the period when theory, as that term is now understood, dominated empirical work, which lasted for perhaps thirty years, was an exceptional interlude.⁹ A previous *History of Political Economy* supplement (Morgan and Rutherford 1998) told the story of how a pluralist economics, in which economists used any data that could aid policy relevance, was pushed aside by neoclassical economics. Though Morgan and Rutherford chose, entirely legitimately, to focus on the demise of pluralism, their story could have been told as being about the rise of theory, for it was during this period that economic theory—increasingly understood as mathematical modeling involving the analysis of utility-maximizing households and profit-maximizing firms, usually operating in competitive markets—came to dominate the subject. It was because this theory was so widely accepted that a high status was accorded to general equilibrium theory. As Mark Blaug (1994, 17) put it, “We *knew* [in the early 1960s] that general equilibrium theory was the last word in theoretical elegance, and that input-output analysis and linear programming would soon make it not just elegant but operational.”

Second, there has been a change in the relationship between theoretical and empirical work, but empirical work has not been completely disconnected from theory. And third, the significant decline is in exclusively theoretical papers. As Dani Rodrik (2015, 201) has said, “These days, it is virtually impossible to publish in top journals . . . in development and international economics without including some serious empirical analysis. . . . The standards of the profession now require much greater attention to the quality of data, to causal inference from evidence and a variety of statistical pitfalls. All in all, this empirical turn has been good for the profession.”

7. This agrees with the findings of Stafford 1986 in the context of labor economics.

8. At the level of teaching, whether at the undergraduate or graduate level, economics remains dominated by theory.

9. Conceptions of theory have changed throughout the history of economics. For example, the interwar institutionalists used types of theory that few economists would today recognize as economic theory. They included Thorstein Veblen’s evolutionary analysis of the relationship between business and industry and the legal-transactional analysis of John Commons.

However, though there appears to be increasing consensus on there having been an empirical turn, our contention is that it is *not* the right way to characterize what has happened. There have certainly been profound changes in attitudes toward empirical work and how to do empirical work, but there has always been a lot of empirical work. Moreover, theory is still important—seven out of twelve Clark Medal winners since 2000 were also described as theorists.¹⁰ Four of them were also described as doing applied or empirical work, and of the others, one (Matthew Rabin) was described as bringing theory “into closer accord with actual behavioral patterns” and another (Daron Acemoglu) was doing work “always motivated by real-world questions arising when facts are difficult to reconcile with existing theory.” A term that has gained wide currency in recent years is “applied theory.”¹¹ This denotes theoretical models that are formulated in relation to specific empirical problems. The importance attached to “applied theory” and to policy analysis is a clear indication that what has happened recently concerns not just empirical work but encompasses theory and policy relevance. This suggests that economics has not really gone “from theory to data” but rather has experienced a profound redefinition of the relationship of theoretical to empirical work. When combined with the policy orientation of most economic research, the result has been a major change in the discipline. As we explain, the term *applied economics* is problematic (in the same way that the notion of applied science is problematic), but we contend that the developments we have described, combined with economists’ widespread use of the term *applied*, justifies our description of the twenty-first century as the age of the applied economist.

The term *applied* is ambiguous (see Backhouse and Biddle 2000). “Application” can involve tailoring a theory to the study of specific issues by creating a model that is relevant to a specific set of circumstances; it can mean using a theory to explain or analyze a specific set of data; or it can mean participating in the design of public policies or business strategies. Given this, it is helpful to step back from economics and to view the history of the broader term *applied science*. This is the subject of the article by Paul Erickson in this volume. Erickson explains how the distinction between pure and applied was developed by natural scientists during the nineteenth century and was used as a weapon in the attempt to

10. These include (dates of Clark Medal in parentheses) Matthew Rabin (2001), Daron Acemoglu (2005), Susan C. Athey (2007), Emmanuel Saez (2009), Jonathan Levin (2011), Matthew Gentzkow (2014), and Yuliy Sannikov (2016).

11. The word is used in Athey’s and Sannikov’s medal citations. See also Diamond 2014.

achieve two seemingly irreconcilable goals immediately after the Second World War: preserving the independence of scientists in the face of pressures from industrial, military, and governmental interests; and convincing those in a position to provide financial support that “pure” science lay behind many useful inventions and was therefore worth funding. On the other hand, as engineers opened graduate programs and sought a more stable position in universities, they “embraced the rhetoric of ‘applied science’ to describe their work.” But although it is commonplace for scientists to use the rhetoric of pure (or basic) and applied science, the distinction is in practice often difficult to sustain. As Erickson explains, the practices of scientists, economists as well as natural scientists, do not fall into neat compartments.

However, we believe that the ambiguity and polysemy of the term *applied* makes it better, not worse, for our purpose. In accounting for the changes that have taken place in recent economics, which cover transformations in theorizing and policy thinking, we need a term that encompasses a larger set of identities and practices than those that come under the heading “empirical.” It is important to remember that as the relationships between theoretical, empirical, and applied work have changed, so too has the meaning attributed to those terms that economists, like natural scientists, have been using because of their rhetorical value. As Erickson argues, “application” is often about the ability of a theory to produce innovative patterns of collaboration, not just its ability to predict or describe. Our focus on the term *applied* mirrors economists’ growing yet often undefined and ambiguous use of the term, notably in John Bates Clark citations—one that reflects a constantly changing understanding of the relationship between theory and empirical work and of the relationships between economists and their clients.

To sum up, this volume is about how the period in which being a theorist was the most prestigious activity for an economist to engage in (a period that in its most extreme form turned out to be remarkably short lived) evolved into the current situation in which economists take pride in being applied, whether applied theorists or empirical economists who tackle problems of policy. This is a complicated story. Economists sometimes claim that the changes can be explained as the result of advances in information technology; however, as we explain later on, while improvements in computer technology were necessary for the changes that took place, they are not sufficient to explain what has happened. The transformation of the subject played out differently across different fields.

It is, moreover, a story with parallels in other social sciences. The historian Hunter Heyck (2015, 11) has argued that in the period from the mid-1950s to the mid-1970s all the social sciences exhibited a concern with systems and modeling: there was widespread belief in a “universal man” and “an idealization of *formal, instrumental reason*, epitomized by the development of systematic *theory* and formal *modeling*, and by a fascination with procedural logic.” This “high modern social science” as Heyck calls it, went through a crisis in the mid-1970s (see also Rodgers 2011). Economists never abandoned modeling, and the story told here is not a precise fit with Heyck’s, but the parallels are sufficient to suggest that the story is a complex one, reaching beyond the boundaries of the economics discipline. It is about changes in tools, models, and technologies as well as about changes in economists’ standards of excellence and how economists’ identity was defined, and to find explanations we will likely have to turn to not just technology but the political and social factors such as the problems economists were being asked to solve and who was supporting them. Though we take account of other sources, the story we tell is based primarily on the papers presented at a conference held in the Center for the History of Political Economy at Duke University, on April 1–2, 2016, and included in this volume.¹²

Changing Relationships between Theoretical and Applied Economics

Understanding the relationship of theory to applied work requires taking a long view. Theory may have enjoyed a privileged status in the economists’ pecking order in the 1960s, but it had not always been that way. In the interwar period, many economists prioritized empirical work: empirical work was as prestigious as theory. This was the period when the NBER was transforming the statistical knowledge of the American economy and when Wesley Mitchell was aiming at a statistical description of the business cycle. It was the age when economists responded to the Great Depression with empirical analyses of market structures and how these might have led to a breakdown in the competitive process (see Backhouse 2015). For such economists, scientific rigor meant being grounded in data, whether statistical or institutional, not being mathematically rigorous.¹³

12. The agenda was set in a paper titled “Becoming Applied: The Transformation of Economics since the 1970s” (Backhouse and Cherrier 2014), in which we posed many questions about the last fifty years and how the concept of applied economics had been understood during that time.

13. See the references above, n. 4.

After two decades in which institutionalists had challenged abstract theory (e.g., Mitchell's [1925] argument that the progress of quantitative economics would cause economists to lose interest in the type of theory associated with neoclassical economics), the quarter century after the Second World War was one in which the centrality of this type of economic theory came to be accepted. Institutional attitudes had not completely disappeared. The author of a report written for the American Economic Association (AEA) on graduate education, Harold Bowen (1953, 111), deplored the fact that there were universities in which students considered economic theory "as a hurdle—something like the foreign language requirement—which must be surmounted but which is not considered relevant to one's major interest" in some applied field. However, he still wrote that the "common core which should bind the profession together . . . consists primarily of economic theory," attributing skepticism about the relevance of theory to poor teaching (42–43). Ninety percent of professors surveyed believed that economic theory should be part of the core graduate curriculum. By the 1950s the leading journals contained a high proportion of articles that were either primarily theoretical or in which theory was used as the basis for empirical work (Backhouse 1998, 91). Two decades later, a report by Nancy Ruggles (1970, 5) could be even more positive about economic theory: "Economic theory provides much of the unity of the discipline. Every economist is trained in economic theory and applies its concepts and mechanisms to problems in his own special field." Micro and macro theories were coming closer together, and both were related to econometrics, which was permitting theories to be tested. Where direct testing was not possible, perhaps because models were too complex to permit "mathematical solutions," simulation could be used to analyze problems such as the effects of changes in tax regimes (Ruggles 1970, 6–7).

However, there was resistance to the increasing dominance of theory, which meant neoclassical theory based on optimizing agents and, mostly, competitive markets. There were intense methodological controversies in the late 1940s and early 1950s (see Backhouse 2003; Isaac 2010). A particularly significant controversy erupted when Tjalling Koopmans (1947), vice president of the Cowles Commission and shortly to become its research director, criticized *Measuring Business Cycles* (Burns and Mitchell 1945), the final volume in a series of statistical studies of the business cycle overseen by Mitchell at the NBER, as "measurement without theory." He argued explicitly that theory was needed to give meaning

to empirical work. Challenging this, Rutledge Vining (1949), for the NBER, countered that this presumed knowledge of the appropriate theory, and that if one did not know the appropriate theory, empirical work was needed to discover it. In the 1950s and 1960s, though there were economists approaching empirical work differently, the dominant approach was the one advocated by Koopmans and the Cowles Commission, in which utility- and profit-maximizing models were used to frame hypotheses that were then tested against statistical data.¹⁴

Another controversy that helped define the attitude toward theory that prevailed in the 1960s was initiated by Richard Lester (1946). Starting from the gap that existed between “marginalist” theories in which firms were presumed to maximize profits, and the theory of employment and the business cycle, he appealed for more evidence: “Much more evidence must be accumulated before definitive conclusions can be drawn regarding wage-employment relationships” (Lester 1946, 64). The way forward was to accumulate better evidence on wage-employment relationships. In contrast, his critics, setting the tone for most subsequent work on business behavior, argued for the primacy of theory: the assumption of profit-maximizing behavior, and with it certain propositions such as that minimum wages must reduce employment. Despite its claim that the theoretical predictions should be tested, and his use of NBER methods in his monetary economics, the most prominent response to Lester, Milton Friedman’s “Methodology of Positive Economics” (1953), completely ignored the evidence Lester adduced against the predictions of marginalist theory and made it clear that the assumption of profit-maximizing firms was not open to serious question.¹⁵ Though unanimity was never achieved, the outcome of the controversy was to help establish the primacy of profit-maximizing theories of the firm in relation both to nonmaximizing theories and to empirical work (see Lee 1984; Mongin 1992; Backhouse 2009).

Institutional battles were also fought. The first five winners of the Clark Medal were Paul Samuelson (1947), Kenneth Boulding (1949), Friedman

14. Whether recent developments, such as the use of natural experiments and randomized control trials, are in the spirit of Vining’s claim that empirical work may be valuable even if no structural model can be specified or bear continuity with the Cowles method is a matter of debate (see Panhans and Singleton, this volume).

15. Lester’s evidence on the effects of a minimum wage bore a striking similarity to the natural experiment that David Card and Alan Krueger (1994) were later to use to question the effects of a minimum wage. Because wages were initially lower in the South, the national minimum wage imposed in 1940 raised wages more in the South than in the North, but employment rose more in the South.

(1951), James Tobin (1955), and Kenneth Arrow (1957), all perceived to be theorists.¹⁶ In 1958, after a complaint from Morris Copeland (1958, 610) that “purely deductive exercises . . . offer young men prompter and surer professional recognition than does any form of empirical research” and that the incentives facing young economists needed to be changed, the AEA appointed a committee to investigate changing the rules surrounding the award. The committee asked former members of the nominating committee and a sample of young economists whether the medal should be discontinued, maintained, or supplemented with an additional award that would reward applied work (to be named after Mitchell). Most former nominating committee members thought that a balance could be struck between theoretical, empirical, and policy work using the existing award. However, the young economists overwhelmingly (thirty-two out of thirty-five) supported the creation of a new award and complained about the difficulty of getting policy work published in the *American Economic Review* and other top journals. One pointed out that businesspeople and the general public were recognizing the empirical contributions that economics could make and that the AEA’s reward structure needed to catch up. One economist, working on “military, political, social and economic intelligence,” wrote that “the job of maintaining the Free World will be a lot easier and will be done better if we have more data and more empirical research.” However, despite the recommendation of the committee, a new award was never established, and the Clark Medal was continued in its existing form.¹⁷

The 1959 winner was Lawrence Klein, and for the first time, a citation was published, noting that Klein “had insisted that theory be grounded in empirical fact” and that he had extended Keynesian analysis “empirically as well as theoretically.” However, though Klein was cited as an empirical economist, the committee perhaps being mindful of the criticisms made of the award, his work was firmly in the Cowles Commission mold, in which a priori theory was used to identify the relationships to be estimated from the data. The growing complexity of the relationship between theory and application was illustrated by the awards in the 1960s and early 1970s, each of which involved a different relationship between the-

16. The key word in this sentence is “perceived.” Friedman and Tobin had both done significant empirical work, but they were not seen by critics of the awards to be empirical workers.

17. The material is taken from Cherrier and Svorenčík 2016. See “American Economic Association Temporary Committee on Additional Awards,” folder “Committee on additional awards,” box 61, American Economic Association Papers, Duke University.

ory and application. For example, much of Robert Solow's work had been theoretical (though with some highly cited empirical papers), but the 1961 citation stressed "the emphasis he has placed, not on mathematical or statistical method, but on the economic significance of his work, and by his ability to contribute not only to the advancement of economic knowledge but also to the formulation of economic policy." Hendrik Houthakker (winner 1963) engaged in theory that had empirical implications, while Zvi Griliches (winner 1965) was cited as "testing hypotheses on the diffusion of innovations" and developing techniques for measuring the effects of changes in the quality of inputs. In 1971 Dale Jorgenson was praised for "the marriage of theory and practice in economics"; in 1973 it was said of Franklin Fisher that "his work represents one of the finest examples of the interaction of theory and measurement in economics." Empirical work was now receiving ample recognition from the AEA, but, for all the differences in approach, it was seen as being closely linked to theory, with much of this work being in the Cowles Commission mold.

There were even rumblings of dissent in the Econometric Society, making it clear that objections to the prominence of theory were not the prerogative of those who either did not or could not use mathematics. In January 1953 Oskar Morgenstern wrote Alfred Cowles, suggesting that the Econometric Society constitution be amended so that candidates for the fellowship be required to "have done some econometric work in the strictest sense" and to be "in actual contact with data they have explored and exploited for which purpose they may have even developed new methods." There was considerable support for this proposal, stemming in part from a view that *Econometrica* contained too little empirical material, though influential members of the Cowles Commission, including Jacob Marschak and Koopmans, opposed it strongly. They complained that there was insufficient funding for fundamental research, by which they meant theory.¹⁸

The early 1970s saw a spate of presidential addresses attacking economics for being too theoretical and too divorced from reality; Leontief's was the most prominent, perhaps because he backed it up with statistical analysis of the leading journals and repeated his attack in an article in *Science* (Leontief 1982). Such criticisms came from theorists (Hahn 1970)

18. Morgenstern to Cowles, June 9, 1953; R. L. Cardwell to Fellows of the Econometric Society, September 18, 1953, Folder "Econometric Society, 1948–1952"; and "Econometric Society, 1952," box 39; Oskar Morgenstern Papers, Rubenstein Rare Book and Manuscript Library, Duke University.

as well as from applied economists (Phelps-Brown 1972). The editor of the *American Economic Review*, in a defensive statement of editorial policy, wrote: “Anyone who has read the *American Economic Review* for the last twenty-five years realizes that great changes have taken place in the subject matter and methodology of economic research. Articles on mathematical economics and the finer points of economic theory occupy a much more prominent place than ever before, while articles of a more empirical, policy-oriented, or problem-solving character seem to appear less frequently” (Borts 1972, 764).

He affirmed that this was not the result of any editorial preference for mathematical or theoretical articles. It is perhaps ironic that these criticisms were happening at just the time that the AEA was selecting a series of economists who, the Clark Medal citations claimed, were proving the relevance of theory to applied work. To the citations of Jorgenson and Fisher mentioned earlier could be added Daniel McFadden (winner 1975), described as “one of the most complete economists of his generation,” “complete” covering theory, econometrics, and empirical studies and Martin Feldstein (winner 1977), whose work spanned “a multidimensional spectrum embracing concrete policy issues, applied econometrics, statistical methods for econometrics, and economic theory.”

The way empirical work was informed with theoretical frameworks is shown in a detailed bibliographic study of labor economics, covering the period 1965–83 (Stafford 1986, 392, table 7.2). This showed that the proportion of theoretical papers had roughly doubled from 1965–69 to 1980–83. However, the author, Frank Stafford, argued that the most significant changes were the near-doubling of the proportion of empirical papers that contained a “meaningful” theoretical section (a section based on standard theory), from 17 percent in 1965–69 to between 33 percent and 36 percent for the rest of the sample period. Even in 1980–83, when the proportion was lowest, 71 percent of papers were empirical, with or without a theoretical basis. In short, labor economics was becoming more theoretical, but much of that theory was informing empirical work; in other cases it was still driven by empirical problems. Stafford estimated that around half the purely theoretical papers had empirical potential even if no empirical work was reported.

In that there was a focus on aggregate time-series data rather than cross-section and panel data on individuals, macroeconomists used techniques different from those generally used in labor economics, but, as in labor economics, theory was dominant. The main theme in macroeco-

nomics in the 1980s and 1990s was the need for a certain type of microfoundations.¹⁹ This was done in the belief that the use of explicitly stochastic theoretical models made it possible to integrate theory and econometric work in a way not previously possible.²⁰ However, despite the importance of theory, macroeconomics remained a field in which applied and theoretical work coexisted: much effort was devoted to developing new empirical techniques and analyzing data; and policy concerns were *never* far away. As a leading protagonist put it, “Major changes in the analysis and practice of monetary policy over the years 1973–98 [can be attributed to] a *combination* of theoretical and empirical influences” (McCallum 2000, 135; emphasis added).

Thus, in 1988, when the *Journal of Economic Literature* initiated a major revision of the codes that it used to classify economics, the decision was taken to integrate theory and applied work. As the journal’s editor, John Pencavel, put it, “Good research in economics is a blend of theory and empirical work” (Cherrier, forthcoming). Policy work was added on the assumption that most of what economists produced was ultimately policy oriented. Shortly afterward, when honoring 1993 Clark Medal winner Lawrence Summers, the AEA could refer to “a remarkable resurgence of empirical economics over the past decade,” claiming that “this research has restored the primacy of actual economies over abstract models in much of economic thinking.” Describing Summers as someone whose influence had “permeated all areas of applied economics,” the citation claimed that “his work has inspired a new generation of economists . . . who are now reconstructing the empirical foundations of the discipline.” By now the AEA was honoring a mixture of theoretical and applied economists and, perhaps most significant, many economists who were combining theoretical and empirical work.

However, though there might appear to have emerged an equilibrium in which theory and applied work went together, the situation did not last, at least in microeconomics (DSGE modeling continued to dominate macroeconomics, even after the global financial crisis of 2007–8). The development of new techniques associated with behavioral and experimental

19. “Microfoundations” is put in quotation marks to indicate that it was understood in a very specific way, typically involving formal modeling of optimizing, infinitely far-sighted representative agents. This was, of course, a very narrow view of microfoundations (see Hoover 2012).

20. No comment is made on whether this belief was justified. The argument against it is that the relationship between theoretical and empirical macroeconomics had always been close. www.aeaweb.org/about-aea/honors-awards/bates-clark/lawrence-summers, accessed July 14, 2017.

economics, and the use of “quasi-experimental” methods, transformed parts of the subject (the so-called empirical turn discussed above). Though structural econometrics remained an active field, there was a reorientation of the relationship between theory and applied work. The Cowles structural econometrics program involved combining theory and empirical work, but new approaches to econometric work moved away from testing theory to finding ways to identify effects in the data, often using large and previously unexplored data sets, as described in the quotation from Deaton at the start of this article. The question with which this volume is concerned is how and why this transition took place.

New Modeling Strategies, New Data, New Technologies

A first explanation for the applied turn in economics is the rise of new and diverse techniques to confront models with data. In the early 1980s, econometrics went through a period of self-examination, from David Hendry’s (1980) likening of certain econometric practices to alchemy to Edward Leamer’s (1983) view that the “con” needed to be taken out of econometrics. It was widely believed in the 1970s and early 1980s that econometric methods could be used to produce whatever results the investigator wanted to find. Spurred on by this, new macroeconometric techniques (vector autoregressive models [VARs], Bayesian estimation, calibration) developed in the 1970s were spread alongside the new models they were used to estimate (by Christopher Sims, Finn Kydland, Edward Prescott, Thomas Sargent, and others).²¹ Another technique increasingly being used was experimental economics. The Economic Science Association was founded in 1986, and such methods were recognized by the award of the Nobel Memorial Prize to Daniel Kahneman and Vernon Smith in 2002. Economists often focus on experiments as a device to test theory, as when experiments were used to evaluate whether subjects make choices consistent with the assumptions of rationality, but, as Andrej Svorenčik (2015, 15) explains, experimental economists pushed a more fundamental redefinition of the relationships between theory and data: “By creating data that were specifically produced to satisfy conditions set by theory in controlled environments that were capable of being reproduced and repeated, [experimentalists] sought . . . to turn experimental

21. See De Vroey 2015.

data into a trustworthy partner of economic theory. This was in no sense a surrender of data to the needs of theory. The goal was to elevate data from their denigrated position, acquired in postwar economics, and put them on the same footing as theory.”

The period when experimental economics became firmly established coincides with the development of a different set of techniques, the history of which is documented by Matthew Panhans and John Singleton in this volume. They argue that the development of research design, going under the general heading of “quasi-experimental” methods, brought about a sea change in the relations between theory and empirical work, emancipating the latter from the former. In the 1970s and 1980s, economists were using data to test and operationalize economic theory (see Stafford 1986). In contrast, in the 1990s economists were turning away from structural econometrics, which involves formulating a fully specified probability model for the problem under consideration, and using simpler methods to identify effects of interest. They date the change to a paper by Joshua Angrist and Karl Krueger (1991), which makes use of the fact that compulsory schooling laws mean that the date in which children are born can determine the number of years’ schooling they get, dividing otherwise identical groups of children into “control” and “treatment” groups. The analysis can be done without specifying any optimization problem or structural model. The institutional feature that makes the experiment possible comes from outside the model. In many cases, the questions that economists try to answer with such methods are not even linked to economic theory. It is, Panhans and Singleton claim, a move from models to methods. The use of such methods, they write, has changed teaching and, institutionalized in schools of public policy and public health, has influenced the way economists interact with other disciplines.

Other techniques blurred the demarcation between theory and applied work by constructing real-world economic objects rather than studying them. That was the case of the combination of experimental economics and mechanism design described in Kyu Sang Lee (2016) or Edward NIK-Khah (2008) or of financial economics (see MacKenzie 2006 and Mehrling 2005). Contingent valuation is yet another technique in which economic values are constructed rather than observed. In his historical account of the development and controversies surrounding contingent valuation, Spencer Banzhaf (this volume) explains that “a CV [contingent valuation] survey literally *speaks* economic values into being,” which, like experimental economics, calls into question traditional linear

and deductive relationships between theoretical and empirical work. Initially developed to give value to recreational and environmental public goods for which no market exists, it was aimed at measuring a specific theoretical object, namely, “willingness to pay.” The idea was to design a survey whereby economic agents would participate in a hypothetical but credible market. The resulting bids they were asked to make would subsequently be used as inputs into cost-benefit analysis. As observed bids came to diverge with what the theory of rational economic behavior predicted, economists reacted differently, thereby showing diverging conceptions of applied work. For proponents of the method, careful survey design ensured that the method actually created rational valuations. For others, like Kahneman, the anomalies observed disqualified the standard theory of individual behavior and confirmed his prospect theory. For experimental economists like Vernon Smith or Charles Plott and econometricians, among them Jerry Hausman, the whole method did not live up to econometric standards and was flawed.

A second explanation for the recent applied turn is the “data revolution” (Einav and Levin 2013). Though the recent explosion of real-time, large-scale, multivariable digital databases is mind-boggling and has the allure of a revolution, the availability of economic data has also evolved constantly since the Second World War.²² Notable events included new micro-economic surveys that were started in the 1960s (the Panel Survey on Income Dynamics) and the 1970s (the National Income Longitudinal Survey). Additionally, administrative databases were increasingly opened for research. The availability of tax data, for instance, transformed public economics. A decade later, technological advances allowed a redefinition of the information architecture of financial markets (see Pardo-Guerra 2010), and asset prices, as well as a range of business data (credit card information, etc.), could be recorded in real time. The development of digital markets and economists’ increased interest in generating their own data eventually generated new, large databases on a wide range of micro-economic variables.

Rather than a revolution in the 1980s or any other decade, the history of economics therefore seems to be one of constant adjustment to new types of data. The historical record belies Liran Einav and Jonathan Levin’s

22. There is a large literature on the making of public statistics (see Porter 1996), such as national accounting (see the bibliography at asociologist.com/national-income-bibliography/) and the cost of living indexes produced by the BLS (Stapleford 2009).

(2014, 716) statement that “even 15 or 20 years ago, interesting and unstudied data sets were a scarce resource.” In 1970, for instance, Jorgenson explained that “the database for econometric research is expanding much more rapidly than econometric research itself. National accounts and interindustry transactions data are now available for a large number of countries. Survey data on all aspects of economic behavior are gradually becoming incorporated into regular economic reporting. Censuses of economic activity are becoming more frequent and more detailed. Financial data related to securities market are increasing in reliability, scope and availability” (61). In the same volume, Guy Orcutt (1970, 67–69), the architect of economic simulation, explained that the current issue was that “the enormous body of data to work with” was “inappropriate” for scientific use because the economist was not controlling data collection. With a very different qualitative and quantitative situation, they were making the same statements and issuing the same complaints as today.

This dramatic improvement in data collection and storage has been enabled by the improvement in computer technology. Usually seen as the single-most important factor behind the applied turn, the computer has affected much more than just economic data. This is undoubtedly important—improvements in computing and the new sources of data that computers have made possible have clearly been *necessary* for the changes that have taken place in applied work—but they are not sufficient to explain what has happened. Roger Backhouse and Béatrice Cherrier (this volume) make this point. The rise of computers could have affected economics in many different ways, and the evolution that took place was the result not just of hardware availability but of the way economists chose to respond to the opportunities facing them. The use of computers to do things that economists already wanted to do (e.g., invert the large matrices in input-output models) is part of the story, but more important was the development of new techniques and methods as economists saw the potential to do things they had not done before. Software, as important as hardware, was developed by economists in response to their ideas about how economics needed to be done. It was not inevitable that computers would favor empirical work over theory: indeed, a major use of computers was simulation, a technique (or perhaps family of techniques) that blurred the distinction between theory and applied work.²³

23. See, for instance, Peter Galison’s (1996) account of how simulations created a “trading zone” between theory and applied work in physics.

That computerization was far from sufficient to ensure the success of applied approaches is shown by the fate of computable general equilibrium (CGE) or applied general equilibrium (AGE) modeling, the history of which is covered by Charles Ballard and Marianne Johnson (this volume). They use the work of Herbert Scarf, who provided algorithms for finding the solutions to general equilibrium systems, whose existence had been proved by Lionel McKenzie, Arrow, and Gerard Debreu, to reinforce the point made by Backhouse and Cherrier, that simulations blurred the distinction between theory and application. Ballard and Johnson distinguish several types of applied general equilibrium model:

- Descendants of the Johanson model (discussed by Verena Halsmayer, this volume)—highly detailed models focusing on policy analysis, often associated with governments or international organizations, many of which tackle policy problems related to international trade;
- Jorgenson-style models—smaller, though getting larger, and based on parameters many of which are the result of econometric estimation;
- Models in the tradition of Scarf—starting from Arrow and Debreu, these involve specified functional forms, often with coefficients established through calibration.

These approaches, largely distinct up to that point, were brought together at a conference in 1981, after which there was a massive increase in the literature and in the use of such models.

Though the availability of large-scale computing was crucial to the development of applied general equilibrium models, as with macroeconomic models of the same period, the lack of standard procedures for coding and differences in operating systems meant that there were great problems in replicating results. Models were criticized as being “black boxes” of dubious value. The response was the development and increasing use of standardized software packages. Standardization was also assisted by the emergence of the World Bank and the Center of Policy Studies at Monash University as the main centers of applied general equilibrium modeling. Becoming a more routine technique, along with other explanations proposed by Ballard and Johnson, may help explain why from the 1990s, applied general equilibrium modeling came increasingly to be confined to field journals. Conversely, the implementation of some of the quasi-experimental techniques mentioned in Panhans and Singleton (this volume), notably randomized control trials, demanded little computational power beyond what had been possible a decade or more earlier.

Throughout the postwar era, economists have repeatedly claimed that the availability of new data sets and the computing power to analyze them have transformed certain fields.²⁴ For example, in his 1964 AEA presidential address, George Stigler (1965, 16) claimed that “the age of quantification is now full upon us. We are armed with a bulging arsenal of techniques of quantitative analysis, and of a power—as compared to untrained common sense—comparable to the displacement of archers by cannon. . . . The desire to measure economic phenomena is now in the ascendent. [. . .] It is a scientific revolution of the very first magnitude.” Changes in the availability of data and the way data were used were not only more frequent than is consistent with the story of a recent “revolution”; they were more diverse across modeling strategies, fields, and sites than is usually acknowledged (see following section).

However, in some fields, data remained a constraint on what could be achieved. In her history of the Norwegian multisectorial growth model, Halsmayer (this volume) shows that Leif Johansen began by writing down a one-sector growth model that provided “binding rules” for the larger models subsequently implemented during the 1960s. It imposed requirements of balanced macroeconomic accounting and working with a neo-classical production function with long-term equilibrium growth values. It therefore presented “the economy” as a system of sectors following what Johansen called development path, that is, one that was efficiently managed though not necessarily balanced because sectors might be growing at different rates. At the same time, Halsmayer highlights the trade-offs between theoretical rigor and the need to accommodate the type of public data Norway produced (input-output data were developed very early, enabling the modeling of growth in several sectors) and the local computing capacities. Johansen simply could not increase the sophistication of his growth models in the way he wanted.

The relationship between theoretical and empirical work underpinning the contingent value method was a different one, and the practice of experimentalists described by Svorenčík in this volume mixed laboratory experiments, *field* observation of how committees made decisions, and

24. For example, Ruggles (1970, 6–7) referred to the effects of survey research methods and the ability of computers to process large quantities of data. Stafford (1986) also draws attention to the importance of new sources of data in the early 1980s. Claims that computerization was dramatically transforming economics were made by Leontief in the 1940s, econometricians in the 1960s, RAND alumni in the 1970s, and by statisticians in the 1990s (see Cherrier and Backhouse, this volume).

historical records of past allocations made by such committees. The history of development economics in the past fifty years is also one of unstable relationships between theoretical and applied work. Michele Alacevich (this volume) tells how, starting from a belief that neoclassical theory was largely irrelevant for understanding the problems of developing economies, development economists ended up believing that policy prescriptions derived from field learning and inductive analysis had failed. From the 1960s onward, therefore, the field was thus largely rebuilt through the import of cost-benefit analysis, input-output analysis, and the standard theory of international trade. At the same time, the field retained a strong focus on understanding institutions, though this worked out very differently in the work of Albert Hirschman, Douglas North, and Lance Taylor. Development also turned to concepts such as multiple equilibrium, imperfect markets, and efficiency-wages, at one point becoming highly theoretical. Development economics was one of the fields in which randomized control trials emerged, though it was much challenged from within.

There was therefore great variety in the relationship between modeling strategies, data, and technologies. In addition, the growing use of simulations (e.g., agent-based modeling) contributed to a blurring of the distinction between theory and application. However, in all the stories outlined in this volume, the diversity of applied economics practices is the result not only of idiosyncratic combinations of modeling techniques, datasets, software, and hardware: it was also a consequence of the variety of sites, old and new, where applied economics was practiced, and the changing demands patrons and clients were imposing on economists. This, of course, is connected to the policy orientation of the applied turn.

The Influence of Patrons and Clients of Economics in These Transformations

Stigler titled his 1964 presidential address “The Economist and the State,” writing that “our expanding theoretical and empirical studies will inevitably and irresistibly enter into the subject of public policy, and we shall develop a body of knowledge essential to intelligent policy formulation.” The social and political context of this address has been extensively researched already.²⁵ Throughout the 1960s and 1970s, a series of crises, social ills, and resulting unrest unfolded: pollution, the urban crisis, and

25. For a summary and bibliography of the history research on this period, see Popp Berman (this volume).

the rise of crime, the civil rights movement, the Vietnam War, the energy crisis, the perceived rise of poverty, and stagflation. The resulting War on Poverty launched by Lyndon B. Johnson, and the associated sprawling legislation designed to improve social policies, brought all sorts of policy experts, including economists, to the government. Beginning in the 1970s and 1980s, however, governments gradually moved toward policies involving deregulation. Economists participated in this process, but their input was mediated by layers of bureaucracy and public decision making; the constraints under which economists operated differed across countries and across agencies and changed constantly (see articles in this volume by Elizabeth Popp Berman, Halsmayer, and Svorenčík).

The funding structure for economics was changing as well. Though private foundations, such as the Ford and Rockefeller Foundations, continued to fund the social sciences, their contribution was increasingly topped by that of the US government (Heyck 2006, 430, table 3). There was also a diversification in the sites where economic research was conducted. A crucial site for empirical research since the 1920s, the NBER was thoroughly restructured by Martin Feldstein (2002) with the explicit purpose of improving empirical work in microeconomics—introducing more theory-informed microeconomics. Alongside traditional independent research institutes like Brookings, new ones emerged, such as RAND (supported by the US Air Force, but developing close links with Ford and changing the direction of its research in the 1960s) and Resources for the Future (see Banzhaf, this volume), also supported by Ford. International organizations such as the International Monetary Fund and the World Bank (re)opened or enlarged their research departments, and central banks hired many economists.²⁶ Economists had testified before Congress for decades, but they were increasingly used as expert witnesses in court as well.²⁷ In addition, the spread of computing and associated techniques

26. See Coats 1986. The role of the World Bank in the rise of the applied economists is mentioned in Ballard and Johnson (this volume) and is the topic of Alacevich (this volume). On the IMF, see Chwiero 2010. That the number of economists with PhDs sitting on the Federal Open Market Committee exploded after the 1960s is explained by Justin Fox (2014). There were similar developments in other countries (see Coats 1981, 2000; Fourcade 2009, 27). See Banzhaf (this volume) as well as Maas and Svorenčík (forthcoming) on the Exxon-Valdez case. For a history of how game theory came to be used in antitrust and industrial relations cases, see Giocoli 2013. For a broader account of how economists increasingly interacted with law scholars and professionals, see Mercurio and Medema 2006.

27. See, for instance, Medema 2000 and Fleury 2010 on the communities of scientists inside and outside economics who spread these tools to the analysis of discrimination and poverty, the key role played by Becker, and the case of public choice.

radically transformed business models and made economic expertise on market analysis and market creation even more relevant. The articles presented in this volume therefore describe how economics was practiced in a variety of sites, and though a common feature is the academicization of extra-academic bodies, they also highlight how economists were faced with various, sometimes contradictory, demands, creating new ways of doing “applied” work and additional layers of complexity in the relationships of these practices to theoretical work.

A first example is how macro modeling in Norway during the 1960s and 1970s shaped and was shaped by public demands for planning. Halsmayer (this volume) explains that “as an artifact, it [Johansen’s model] bears traces of previous knowledge but, due to its new shape and material, it provided a new reification of the economy, opened up a new way of seeing this entity, and nurtured specific forms of actions to steer it.” The lines between theory and application, she contends, become blurred when economic theory and data come together in the context of not only available mathematical techniques and computer technology but also “institutional arrangements, the aims and hopes of economic planning, images and visions of ‘the economy.’” Computer modeling could not be separated from the administrative apparatus for planning: the epistemic authority of the model made planning possible; at the same time, the planning process lent further authority to the model. This is significant for our conceptualization of what happened to economics in that “macroeconomic planning” ceased to be thinking about goals and aims but came to comprise methods and techniques that were conceived independently of the political setting. It survived political changes. Planning became an organizational task that could be represented in terms of flowcharts reminiscent of cybernetic systems. Even office space was reorganized to accommodate how the model worked, with model construction and national accounting units sharing a floor, and new personnel recruited to handle the computer infrastructure and devise the visuals necessary to communicate the model’s predictions to decision makers.

In the same period, policy regimes underwent gradual changes in the United States. Though the consensus is to characterize this change as a move toward deregulation, Popp Berman (this volume) shows that, as in Norway, this was a complex process in which policy goals beget new forms of expertise, themselves engendering new institutional layers of governmental consultancy and decision making. The indirect influence of these bureaucratic offices extended far beyond the president, who had set

them up and facilitated the flow of ideas between academe and the world of policy. It was not merely a matter of interventionist Keynesian economists being displaced by neoliberals opposed to intervention.

The key development came under Gerald Ford and involved deregulating industries in the context of trying to reduce inflation (economists who were consulted did not agree that it would achieve this goal, but most could nonetheless agree that deregulation would be beneficial). Deregulation was seen as the government's contribution to disinflation, and those industrial organization scholars specializing in the topic gradually supplanted the system analysts previously in charge of implementing the Planning-Programming-Budgeting System. Concern then spread to social regulation—to estimating the costs and benefits of regulations relating to the environment, health and safety at work, and equal opportunities—where the emphasis was not on removing regulation but on balancing benefits and costs. This increased dramatically during the Carter administration, and, paradoxically, despite the anti-analytical bias of the Reagan administration, it was then institutionalized through an executive order making cost-benefit analysis of regulation compulsory. This fed back into academe, as agencies commissioned studies to quantify the benefits of regulation.

Berman concludes that applying economics did not just mean applying specific models but using an economic way of reasoning and creating institutional channels, such as the Office of Information and Regulatory Affairs, through which such reasoning could spread. This is what happened in the 1970s, establishing the context for many of the developments that John Macmillan (2003) describes as “reinventing the bazaar.” One economic tool nevertheless stands out in this as well as many articles in the volume, namely, cost-benefit analysis. Banzhaf (2009) has already narrated the water resource management origins of the techniques and has explained that although rooted in welfare theory, applications of the techniques generated substantial debates among environmental economists.

Contingent valuation likewise derived from the need to input values for nonmarket goods into cost-benefit equations, and Banzhaf (this volume) as well as Maas and Svorenčík (2017) all show that as the tool was endorsed by courts, it generated an increasing amount of controversy. Debates already existed regarding the theoretical foundations of the objects surveys were measuring, but faced with the contingent valuation of the environmental damages caused by the 1989 Alaska oil spill, Exxon decided to fund research aimed at demonstrating that the whole method

was flawed. Surveys were organized, showing problems with embedding, scope insensitivity, and differences between willingness to pay and willingness to accept. Those economists who rejected contingent valuation, from Charles Plott and Vernon Smith to Daniel Hausman and Peter Diamond, and those who endorsed it, Alan Randall and Michael Hanemann, therefore exhibited diverging epistemological postures regarding what counted as good applications, as did various patrons. The blue ribbon panel in charge of evaluating contingent valuation, which included Arrow, Solow, Leamer, and Roy Radner, concluded that contingent valuation was hardly “a clean application of economic theory; it is messy, but good enough for government work,” Banzhaf concludes. This speaks to the kind of trade-offs economists make in applying their tools to policy issues.

Since its inception, cost-benefit analysis has been tied to the need for *ex ante* and *ex post* policy evaluation. When development economists endorsed the neoclassical framework beginning in the 1960s, Alacevich (this volume) explains, one of their first endeavors was to adapt cost-benefit analysis tools to the evaluation of industrial policies in developing countries. Yet the growing demands for such expertise during the 1960s and 1970s also induced economists to develop alternative tools and models to evaluate policies. For instance, it pushed Heather Ross, then a Massachusetts Institute of Technology graduate student, to undertake with Princeton’s William Baumol and Albert Rees a large randomized social experiment to test negative income tax in New Jersey and Pennsylvania. The motivation to undertake experiments throughout the 1970s and 1980s was less to emulate medical science than to allow the evaluation of policies on pilot projects. Daniel Breslau (1997) likewise shows that, when faced with the task of evaluating job training programs in the 1980s, those researchers hired by the government again relied on social experimentation, while academic labor economists favored structural econometrics. As Panhans and Singleton (this volume) remind us, the unification of what counts as a “credible” method for the identification of a policy’s effects was achieved under the influence of economists such as Orley Ashenfelter, who, as an academic, was willing to participate in the development and promotion of the “clean” identification strategy offered by difference-in-differences, and who also understood, as former director of the Office of Evaluation at the US Department of Labor, that it would be considered “credible” by policymakers. In that sense, both the demands of policymakers and the institutional structures that mediated them influenced the development of new tools in economics.

The deregulatory movement in the United States also fostered, if not the development, at least the spread of other tools, such as experiments. Experimental economists were asked to devise and test mechanisms to regulate the price of public television broadcasts, natural gas, or waterways transportation. As recounted by Svorenčik (this volume), the Airline Deregulation Act of 1978 resulted in the prospect that a growing number of new entrants would soon increase the shortage of airports' take-off and landing slots. It was in this context that David Grether, Marc Isaac, and Plott were asked to propose new schemes to allocate slots. To provide policy recommendations, it was necessary to move back and forth between experiments, theory, nonexperimental evidence, and observations of the target phenomena. Though slot auctions were recommended very early, partly because of unexpected events, such as Ronald Reagan's sacking of striking air traffic controllers, dramatically increasing the number of airports with slot limitations, it was only in 2010 that such an auction took place. The story of the rise of the use of experiments in solving the problem of how to allocate airport slots was primarily a client-driven problem.

The influence of Reagan's policies on ways to practice applied economics was also felt in more indirect ways, notably through the National Science Foundation (NSF). Though the NSF's economics funding remained minimal throughout the 1960s, it climbed substantially only after the establishment of the Research Applied to National Needs office in the early 1970s. Tiago Mata and Tom Scheiding (2012) explain that this office funded research on social indicators, data, and evaluation methods for welfare programs. It closed in 1977, however, after Herbert Simon issued a report emphasizing that the applied research funded was "highly variable in quality and, on the average, not impressive" (Mata and Scheiding 2012, 429). The NSF continued to fund research in econometric forecasting, game theory, experimentation, and development of longitudinal data sets, but in 1981 Reagan made plans to slash the Social Sciences NSF budget by 75 percent, forcing economists to spell out the social and policy benefits of their work more clearly. Lobbying was intense and difficult. Kyu Sang Lee (2016) relates how the market organization working group, led by Stanley Reiter, singled out a recent experiment involving the Walker mechanism for allocation of a public good as the most promising example of policy-relevant economic research. Klein, Arrow, and Griliches were asked to testify before the House of Representatives. The first highlighted the benefits of his macroeconomic models for the information industry; the second explained that economic tools were badly needed

at a time when rising inflation and decreasing productivity needed remedy. The NSF requirement that “policy benefits” be emphasized in grant application may have encouraged, if not the substantive development of applied economics, at least the kind of rhetorical habit discussed by Erickson (this volume), which may have in turn contributed to transform economists’ identity.

All this suggests that the turn to applied economics, which involved changes in both economists’ practices and their perception of their identity, was shaped, to some extent, by policy demands and the institutions that mediated them. Alacevich (this volume) shows that nongovernmental patrons were equally able to influence the form taken by applied economics in other fields. The kind of inductive and field-based knowledge promoted by development economists such as Hirschman was largely nurtured within the World Bank, but by the end of the 1950s, the institution’s top officials nevertheless found the generalizations offered too theoretical, and the research department was closed. When social research was brought back to the bank in the late 1960s, it was under the guise of a programming and budgeting department set up by Robert McNamara. Its focus had therefore evolved into adapting the tools of cost-benefit analysis to developing economies, and, under Hollis Chenery, those were augmented by input-output analysis, linear programming, and computable general equilibrium models. By the turn of the twenty-first century, the World Bank research department had evolved into a major player in the field. Because its task was development assistance rather than the mere production of knowledge, the notion that knowledge on development emerged from fieldwork remained, creating a permanent tension toward the import of tools and abstract models and the academicization of its research department.

Conclusion: The Age of the Applied Economist

The 1950s and 1960s could be described as the age of the economic theorist. Unlike in the pluralist interwar period in which institutionalism was strong, economic theory had become the property of younger economists who had a more rigorous training in mathematics than their elders. There was much applied work being done—it is probably safe to say that the vast majority of economics was applied in some way—and, as computers and specialist software packages gradually overcame the barriers to large-scale calculations, econometric work was increasing in quantity and in

sophistication. However, theory was prominent in the leading journals and was increasingly structuring the way economists thought about economic problems. As one observer noted, the tribe's priestly caste comprised the mathematical economists (Leijonhufvud 1973, 329), and chief among them were general equilibrium theorists.

As we have shown, the situation gradually changed, as new techniques and new sources of data made it possible to derive results that economists considered more robust. However, for many years, economic theory retained its primacy. Applied work was perceived to need a theoretical basis, with econometrics being used to test formally specified theoretical hypotheses. Several fields, such as development and public economics, were reshaped around the neoclassical theoretical core. But the 1970s were also a period in which models of individual rational decision were applied to a new set of problems previously thought outside the boundaries of economics, from health to education, international relations, political behavior and law, family, marriage, crime, discrimination, prostitution, and drug dealing. These endeavors were largely encouraged though not created by Johnson's need for rational social policy tools. Part of them fall under what is usually called "economic imperialism," the title of a book chapter Gordon Tullock published in 1972. Six years later, Gary Becker, the initiator of this current of analysis, published a synthesis under the equally telling title *The Economic Approach to Human Behavior* (1978).

But at the same time, the turn to game theory and the development of theories of imperfect and asymmetric information were greatly expanding the theoretical toolkit available to economists, providing formal theories of phenomena about which economists had previously been able to speak only informally. As this happened, the role of theory changed. No longer was it possible to hope for a single, all-encompassing general theory: instead, the best economists could hope for was a range of theories, each of which could be used in a different set of circumstances. This is the background to the increased use of the term *applied theory*. The growing and complex ties with policymakers and a variety of clients also made it harder for theory to be applied in a standardized way. As most articles in this volume show, public and private demands alike pushed economists to tailor theories and tools to specific uses, profoundly changing them in the process, sometimes even disregarding them altogether in favor of survey-based methods or new techniques such as field experiments. Understanding more specifically how theoretical models were tied to specific application and empirical work across different fields through researching

field history is still a huge task ahead for historians. Further work is also needed to understand changes in the relationship between theory and application in macroeconomics.

In the 1990s there emerged new ways of doing applied microeconomics, in that quasi-experimental methods made it possible to do convincing empirical work independently of economic theory. This affected fields where extensive data were available, or could be generated, on individuals: labor, household behavior, and development. But this was only one dimension of the rise to prominence of applied economics. As governments turned to market solutions to economic problems, from the allocation of airport slots to the right to emit pollutants into the atmosphere, the scope for applied work increased. Even technically inclined game theorists could find themselves doing applied work, contributing further to the increased currency of the term *applied theory*. The “new economy” of the “weightless” world posed problems that prompted technological giants such as Microsoft and Google to turn to economists. Their work could involve the new-style, theory-free empirical work, but it could also involve theory and simulations. Algorithms could be developed to solve problems formulated theoretically as much as ones involving statistical data. It had become the age of the applied economist.

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