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## Becoming Applied: The Transformation of Economics after 1970

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BECOMING APPLIED: THE TRANSFORMATION OF  
ECONOMICS AFTER 1970

BY ROGER BACKHOUSE AND BÉATRICE CHERRIER

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CENTER FOR THE  
HISTORY OF POLITICAL ECONOMY  
AT DUKE UNIVERSITY

# **Becoming Applied: the Transformation of Economics after 1970**

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## **Abstract:**

This paper conjectures that economics has changed profoundly since the 1970s and that these changes involve a new understanding of the relationship between theoretical and applied work. Drawing on an analysis of John Bates Clark medal winners, it is suggested that the discipline became more applied, applied work being accorded a higher status in relation to pure theory than was previously the case. Discussing new types of applied work, the changing context of applied work, and new sites for applied work, the paper outlines a research agenda that will test the conjecture that there has been a changed understanding of the nature of applied work and hence of economics itself.

**JEL Codes:** A10, B20, B40, C00

**Keywords:** Applied economics, theory, Clark Medal, JEL codes, core, policy, computation, data, econometrics

# Becoming Applied: the Transformation of Economics after 1970

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## 1. Introduction

In 1970, Nancy Ruggles edited *Economics*, a volume that formed part of a *Survey of the Behavioral and Social Sciences* under the auspices of the National Academy of Sciences and the Social Science Research Council. Aimed at presenting “a comprehensive review and appraisal” of economics, the book comprised a series of short reports on the main fields within the subject. In her introductory chapter, Ruggles (1970, p. 4) argued that whilst economists were “striving for a theoretical-quantitative discipline which can unify abstract model-building with empirical analysis”, the core of the discipline lay in economic theory. “Economic theory,” Ruggles wrote, “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” (Ruggles 1970, p. 5). The surveys thus began with the core of “microeconomic theory” and “macroeconomics”, followed by chapters on different methods and range of applied fields. Thus although most of the reports focused, in one way

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\* The aim of this paper is not to present definite conclusions but to propose a research agenda in preparation for a conference to be held at the Center for the History of Political Economy, in Duke University, in April 2016. We are grateful to Jeff Biddle, Kevin Hoover, Steven Medema and Roy Weintraub for useful comments on earlier drafts.

or another, on applications, Ruggles left little doubt that economic theory had priority.<sup>1</sup>

Eighteen years later, the editors of the *Journal of Economic Literature* embarked on a comprehensive revision of the codes used to classify economics. Though this aimed explicitly at a representation of the discipline in terms of a “core” and “applied” fields, that the term “theory” had disappeared from all the first-level headings and most of the sub-headings. Even within the “core” of microeconomics and macroeconomics, theory was less prominent: “value theory” was present but it was alongside categories such as “computable general equilibrium models,” “forecasting and simulation,” and “income policy, price policy.” This new structure reflected the explicit agenda of its architects who, from the beginning, had been adamant “not to place the theoretical and empirical research in separate categories, but to integrate them” (quoted in Cherrier 2014). The reason, JEL editor John Pencavel wrote to those helping him revise the classification, was that “good research in economics is a blend of theory and empirical work.” This elicited the response that, “If we don't want to separate theory and empirical analysis, why do we separate theory and policy?” The result was that theory, empirical work and policy advice were combined within the core. The new scheme was implemented in 1991, the beginning of a decade that saw increased recognition of “applied work,” exemplified by the award of the next three John Bates Clark medals to Lawrence Summers (in 1993), David Card (in 1995) and Kevin Murphy (in 1997). So sensitive did the editors of some of the major journals become to the charge that too much space was devoted to economic theory, that during the 1990s their annual reports monitored number of theoretical and empirical articles being published.<sup>2</sup>

Our conjecture is that these two two episodes encapsulate a significant change in

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<sup>1</sup> Though we have chosen to take Ruggles as our focus, on the grounds that her report was as close as one can get to an “official” view, it is worth noting that we might have taken Wassily Leontief’s (1971) presidential address to the AEA as our starting point.

<sup>2</sup> Reference here is to the AER and the Economic Journal.

economists' attitudes towards economic theory and applied work that has taken place since the 1970s and that understanding this change is important for understanding how economics has evolved during this period. The idea of a hierarchy, sometimes explicit but often implicit, with abstract economic theory at the top and various forms of applied work underneath, was challenged. Economic theory was still considered essential, in that empirical work was expected to be grounded in theory and economists often began with a theoretical model but theory was expected to remain closer to its applications.

Such a claim raises the question of what the term “applied economics” means. Drawing conclusions from a previous *History of Political Economy* conference, Backhouse and Biddle (2000) pointed out that “applied economics” was a polysemic, ambiguous and yet pervasive term. Economists have used the term “applied” to refer to the application of theoretical tools to specific issues, as a synonym for empirical work, and as referring to the drawing of policy implications from economic research. Multiple meanings arose because the term “applied” was not defined directly but in in opposition to something else—for example, in relation to “pure”, “theoretical” and “abstract” economics. During the decades after the Second World War these different meanings competed within the profession and after 1970 “applied” work in all of these senses of the term, expanded dramatically and increased in prominence. There is perhaps a parallel here with changes that took place in mathematics (see Dalmedico 2001).

The pecking order that, we conjecture, was challenged after 1970 was established only after the Second World War.<sup>3</sup> This makes our project a sequel to the transformation documented in another HOPE conference (Morgan and Rutherford 1998). Before the Second World War, they argue that in the United States, institutionalist and neoclassical approaches

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<sup>3</sup> The extent to which theory lost its primacy is a matter of debate. The claim can be disputed on the grounds that applied work requires theory if it is to be taken seriously. Against this, it can be argued that interest in purely theoretical papers has declined, and that outside certain specialist journals, there has been a shift to papers that combine theory with empirical work.

coexisted in a discipline best characterized as pluralist: no single approach was dominant. In contrast, by 1960, the discipline was dominated by neo-classical economics: other approaches still existed but they were no longer seen as central. Associated with this transition was the emergence of the idea, absent in the 1920s, that economics was about modeling. The economic theorist came to be seen not as someone who drew conclusions based on wisdom typically acquired over a long career but as a modeler who worked out the consequences of abstract assumptions, increasingly using mathematics. Microeconomic theory, defined as working out the consequences of optimizing behavior on the part of individual economic agents, came to have the privileged status described by Ruggles in 1970.<sup>4</sup>

Subsequent scholarship has extended, developed and qualified the picture painted by Morgan and Rutherford. There is now a large literature on period in which the pecking order described by Ruggles emerged, covering the demise of institutionalism and the mathematization of economics (for example, Rutherford 2011; Weintraub 2002; Duppe and Weintraub 2014). In contrast, the literature on the changes that challenged that pecking order since 1970 is much less developed. There is work on specific applied topics and fields but comparatively little attention has been paid to the conception of economics as a whole.<sup>5</sup> This is the reason why we are organizing another HOPE conference, directed at understanding the consequences of this redefinition of applied fields in relation to the rapidly changing body of theoretical, conceptual and empirical tools that have defined the discipline. Our aim is to understand this process and the factors that may lie behind it. These factors could involve: challenges to civil society and what has been called “the challenge of relevance” in the 1970s, the rise of “big data” and computerization; the increased importance of non-academic

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<sup>4</sup> Macroeconomic theory can be fitted into this story but it raises issues to discuss which would require more space than is appropriate here.

<sup>5</sup> Sketches have been provided by Backhouse (2002), Backhouse and Medema (2009), Backhouse (2010).

institutions, such as central banks, the IMF, the World Bank and the National Bureau of Economic Research (NBER); or the displacement of general equilibrium theory as the central theoretical framework by game theory or experimental and behavioral economics. In this paper, intended to set the agenda for the conference, we suggest a number of tentative hypotheses to characterize and explain these changes.

We start by trying to substantiate the main claims we have made. We begin, in Section 2, by considering some historical evidence that relates to the dominant role of economic theory in the postwar period. Then, in section 3, we try to document the changed character of the discipline through an analysis of the winners of the AEA's John Bates Clark Medal which, since 1947 has been awarded every two years to the most promising economist under the age of forty. Because of its focus on younger economists, we would argue that it gives a better indication of trends than the more frequently analyzed Nobel Memorial Prize in Economics which typically recognizes achievements at a much later career stage. We then turn to possible explanations for the rise of applied economics, by considering the technological and the institutional bases for this change (sections 4 and 5). In the final section we list some of the questions that we believe need to be answered if we are to understand these changes.

## 2. **Applied Economics before the late 1960s**

It is now well documented that the Second World War and the Cold War fostered the development of new theoretical and applied work. New empirical techniques included structural estimation at the Cowles Commission, the development of input-output methods, linear programming, and the simplex method. New data sets were gathered: the postwar



stabilization of national accounting was linked to the much more widespread compilation of national income statistics (Mitra-Kahn 2011) and there was widespread gathering of cross-sectional data on many industries. At the *Bureau of Labor Statistics*, a set of new indexes to estimate the cost of living were also developed (Stapleford 2009). Theoretical advances also changed the postwar intellectual landscape: the Keynesian revolution and mathematical modeling of the business cycle; game theory; dynamic modeling involving new techniques such as Bellman's dynamic programming; new models of consumer behavior from revealed preference to expected utility; and general equilibrium analysis.

What is significant about these changes for our purposes is that, as theoretical and empirical work became more formal and mathematical, conceptions of economic theory and of its relationship to various types of applied work changed. The perceived relationship between theoretical and applied work becomes apparent in many places: in the published literature and in places locations such as the Ford Foundation, which in 1951 suddenly became one of the largest sources of social science funding, and the AEA, in which debates took place over both the graduate curriculum and over its classification scheme. A widely discussed example is the controversy that erupted in 1946 when Tjalling Koopmans, Vice-President of the Cowles Commission and shortly to become its Research Director, reviewed *Measuring Business Cycles* (Burns and Mitchell 1945), the final volume in a series of statistical studies of the business cycle overseen by Wesley Mitchell at the National Bureau of Economic Research (NBER). His charge that the approach of Mitchell and Burns, characteristic of much empirical work in the 1930s, represented "measurement without theory" placed neoclassical economic theory in a privileged position, as Rutledge Vining (1949) pointed out in his reply. Vining, echoing earlier conceptions of economic theory, contended that empirical work was needed in order to discover the appropriate theory.

Another debate from this period can be read in the same way. When Richard Lester (1946) raised doubts about the increasing trend to use marginal analysis to analyze the behavior of firms he was asserting the primacy of empirical work, citing not just his own survey data but also the extensive interwar and wartime evidence that had been accumulated on how firms behaved when confronted with changed circumstances. In the ensuing debates (see Lee 1984; Mongin 1992; Backhouse 2008) the dominant view was that it was appropriate to assume that firms maximized profits, Milton Friedman's (1953) argument for this becoming one of the most widely read methodological essays in the discipline. It came to be accepted that the empiricism of Lester was naïve and, though Friedman did not hold this view, that Koopmans had been right in his argument that the NBER paid insufficient attention to economic theory as he understood it. The primacy of economic theory implicit in such views was given philosophical justification in debates over methodology that surfaced in leading journals. Even more dramatic was Fritz Machlup's remark, discussing the leading theorist of the period,

I conclude that Samuelson, one of the most brilliant theorists in present-day economics, produces his best work when he deduces from unrealistic assumptions general theoretical propositions which help us interpret some of the empirical observations of the complex situations with which economic life confronts us (Machlup 1964, p. 735).

This was, of course, Machlup's personal view but it is safe to assume that it would have commanded widespread support. It represented a view of the power of abstract economic theory about which most economists in the 1930s would have been skeptical.

The Ford Foundation faced the question of whether it should fund research aimed directly at tackling concrete problems or whether it should take a more indirect, long-term approach. Echoing earlier debates over government funding for the natural sciences, this raised two issues: the extent to which there should be any attempt to move research in directions believed socially useful, and the extent to which funding should be directed at “fundamental” or “basic” research. Whilst the organization of large teams of scientists to tackle immediate practical problems had been immensely successful, producing innovations such as radar and the atomic bomb, such successes relied on prior “fundamental” or “basic” research. It was widely believed that “fundamental” research would not be supported by the private sector, which wanted clear returns on any investment, and that the best way to government could promote it was to create an environment in which scientists were free to follow their own intellectual curiosity. The distinction between fundamental and applied research was not the same as that between theory and empirical work, an issue on which the economists involved in the discussions within Ford were divided some expressing skepticism about theoretical work, and other supporting it. However, economists were united in opposing any attempt to focus research on applied problems that outsiders thought might quickly produce results of immediate policy relevance. Supporting “fundamental” research involved defending theory and empirical work that did not have immediate practical consequences.

In the AEA, the relationship of theoretical and applied economics emerged during discussions of the graduate curriculum. Drawing on a survey of professors, a report produced by Howard Bowen argued that economics departments should not “convert into trade schools”, providing purely vocational training, but “should continue to place emphasis on fundamentals and on scholarship”, which implied a “common core” for all programs (Bowen 1953, p. 2). This core comprised not just economic theory but also economic history, history

of ideas and research methods (*ibid.*, p. 40) but economic theory believed to be the most important component, by a long way. Virtually all professors (98%) thought economic theory essential for doctoral students, compared with statistics (53%), economic history (55%) and history of economic thought (37%) (*ibid.*, p. 105). There was a widespread desire to increase coverage of theory and, most tellingly, very few professors (3%) thought there was any need for closer integration of theory and applied fields. A decade later views had solidified, Richard Ruggles (1962, p. 487) writing that the function of graduate training was ‘to provide a common core of basic economic theory’ that would be used elsewhere in the program, and noting that ‘at a great many universities’ training in mathematics was required. There was disagreement and when the codes used for classifying articles were agreed in 195\* (see Cherrier 2014) each of the main headings covered both theory and applied work.

By 1970, it was generally accepted that economics was based on a common core of economic theory centered on mathematical modeling of maximizing agents. This view was strengthened by the extension of models based on maximizing behavior to fields that had long resisted it, including extensions of such models to problems traditionally thought lying in the domain of other social sciences—the phenomenon often described as economics imperialism. Econometric modeling had also taking off in the 1950s, and by 1970 formal statistical inference, notably regression analysis, was widespread in applied work applied work. Economics was becoming more technical and methodologically homogeneous, eliciting protests from “heterodox” economists whose analysis was not longer valued so highly. However, the outcome was considerable dis-satisfaction because, for all the improvement in techniques, it was not clear that theory and empirical work had been successfully integrated. For example, Wassily Leontief (1971) complained about the building of an ever more elaborate theoretical edifice on empirical foundations that could not support

it, calling for more attention to the development of new data. The economic theorist, Frank Hahn (1973), complained about elaborating theoretical models that bore no relation to the world in which we live. Economic theory had acquired a prestige that many believed it did not merit.

### **3. New types of applied work? Evidence from the Clark Medal**

#### *The “core” and its many applications*

One way to see the change that has taken place in recent years, is to look at the citations for the AEA’s Clark Medal. While theorists were repeatedly praised for their renewed understanding of economic behavior, bringing in imperfect information (Joseph Stiglitz in 1979 and Michael Spence in 1981) or psychological evidence (Matthew Rabin in 2001), citations increasingly highlighted the significance of new theories for specific fields rather than for economics in general.<sup>6</sup> Spence was portrayed as the leader of the “new economics of industrial organization,” Sanford Grossman’s understanding of contracting under uncertainty and asymmetric information is said to have changed financial economics, and it was made clear that Paul Krugman’s (in 1991) analysis of the implication of increasing returns to scale and imperfect competition transformed international economics. An indication of a changed attitude towards theory was the increasing prominence of the term “applied theorist”, used much more rarely in 1970. Thus the 2007 Clark nomination statement begins with “Susan Athey is an applied theorist.”

Similarly, when the honor was awarded for contributions to econometric theory—Marc Nerlove (1969) for estimating response lags; Dale Jorgenson (1971) for estimating rational

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<sup>6</sup> In parenthesis is the date of the award.

distributed lags; Daniel McFadden (1975) for his analysis of discrete choice and James Heckman (1983) for his work on panel data—it was specified that medalists had also developed “fruitful applications,” that the spirit behind their work was “to explain real economic phenomena”, that they had conducted “large scale empirical inquiries” (McFadden) or “changed the face of labor economics, econometrics and demography” (Heckman). The term “applied econometrician,” was used to characterize Martin Feldstein's (1977) work on insurance and health or Jerry Hausman's (1985) work on energy.

As economic research was transformed by the falling cost and increased availability of computing power, it became routine for applied work to involve formal econometric testing and estimation of models, and this was done using increasingly sophisticated methods. In the early nineties, Lawrence Summers (1993), David Card (1995), and Kevin Murphy (1997) were picked out for their substantial contribution to “applied economics,” in particular labor economics, for having developed new methods, such as the use of natural experiments and having applied them to new carefully constructed data sets. The work of Summers, it was said, exemplified the “remarkable resurgence of empirical economics over the past decade.”

The combination of theoretical and empirical skills was prominently featured in the work of virtually all of the 5 last Clark medalists – Emmanuel Saez (2009), Esther Duflo (2010), Jonathan Levin (2011), Amy Finkelstein (2012) and Raj Chetty (2013). Most of them are explicitly called “applied economists,” a term that had previously been used only once in Clark medal citations to characterize Franklin Fisher's (1973) work on various issues such as the cost of educational loan systems, the petroleum drilling and community television antenna industries.

The dynamics of the John Bates Clark medal award suggests that economics had developed, in the last quarter of the twentieth century, by applying theories and econometric

tools from its core. Indeed, the discipline is today portrayed (by the AEA for instance) as constructed around such an agreed core which is applied in a myriad of fields. These have their own journals, societies and networks born throughout a process of fragmentation of the discipline during the seventies.<sup>7</sup> Little work has been done to explain this process, which involved both unification and fragmentation, or to study these applied fields exploring how their practices and cultures differed. Though economists may be trained in the same techniques, because they tackle different problems, using different types of data, conventions on what constitutes acceptable econometric practices and empirical work, or what constitutes rigorous theorizing, are not the same in, say, macroeconomics, labor economics and environmental economics (see for example, the exchange between David Hendry and Andrew Oswald in Backhouse and Salanti, 2000). We know neither what happened to these tools when they were applied, nor how did the availability of these new tools affect the fields to which they were applied. For example, the nomination statements quoted above suggest that work done in applied fields may have triggered new theoretical and technical developments that have affected the core and that the influence normally runs from core to applied may not be correct.

### *Policy relevance and policy design*

Another striking feature of recent nomination statements is that they make the notion of “applied economics” more complex by tying it to the problem of policy. Finkelstein is praised for her ability to identify “policy-relevant yet tractable research questions,” while Chetty’s research agenda is identified as “tax policy, social insurance and education policy.”

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<sup>7</sup> See for instance how the scope of economics is picture on the AEA website: <http://www.aeaweb.org/students/Fields.php>

Duflo “has been a leader in using randomized field experiments to address important questions concerning public policy.” Saez’s work on optimal taxation theory has “brought the theory of taxation closer to practical policy making.” Economists have always been concerned with policy, but these statements hint that there may now be a belief that, economists were now in a position to do something to which previous generations could only aspire. This suggests that the concern with the implementability of theory had become stronger among economists, possibly influencing the development of theoretical insights and empirical tools. It presumably reflects the increased confidence economists appear to have had that they could not only study but they possessed the theoretical and empirical tools necessary to create markets and design institutions that would achieve specified objectives: that they could, to use John McMillan’s (2002) phrase, “reinvent the bazaar”.

#### **4. The changing context for applied work**

If economics did change in the way that our analysis of the Clark medal winners suggests, what was the reason? Though our list is far from comprehensive, we sketch three possible changes in the context in which economics was undertaken: computerization; the rise of new economic and social problems, and emergence of new sites for economic research and the changing relationships between existing ones.

##### *Computerization and computational issues*

From the 1940s to the 1960s, computation was a major issue in economics. Even a simple problem, such as George Stigler’s diet problem (finding the minimum cost of a diet



involving 9 dietary elements and 77 foods while meeting nutritional standards) took 120 man-days to compute by hand (Klein 1991). The simplex method for solving a linear problem was very efficient, but even with its help, great importance was attached to finding even better methods because it still took too long to solve interesting problems. RAND economists explored ideas such as that tâtonnement processes could be used to solve programming problems, or that programming problems could be reformulated as games that could then be played, the revealing the solutions. Economists used approximations and substitutions to simplify their models as much as possible. During the 1950s, econometricians developed programs that made it possible, for the first time, to use estimation methods that had been known since the 1940s but which had been computationally too demanding to implement. Thus Klein's models grew from around 10 equations to over 300 equations by the 1960s. In this process, the development of efficient algorithms was a major factor behind the use of computers. Klein pictures a world in which, in the sixties and seventies, economists were engaged in a computation race which involved the expansion of computation facilities and the improvement of computation methods on the one side, and, on the other, the new problems raised by developments such as stochastic simulations (in the wake of the simulations that Irma Adelman and Frank Adelman conducted using the well-known Klein-Goldberger model), scenario analysis, sampling experiments using Monte Carlo methods, and the rise of cross sectional analysis.

Numerical methods were being widely used but they had not settled down and were unreliable. Software packages that should have produced the same results as each other did not always do so. This lack of standardization was one factor behind the difficulties economists faced in replicating each others' results: the apparent unreliability of the econometric estimates, as reported in a widely cited study of articles in the *Journal of*

*Money, Credit and Banking* (Dewald et al 1986), especially when combined with the critiques mentioned earlier, raised questions about the credibility econometric work. The advent of the personal computer in the 1980s did not at first change this situation. However, by the end of the century, a combination of increased computing power, the stabilization of routines for numerical calculations, and the development of software packages that did not require users to learn a programming language, transformed estimation into something that was entirely routine (some of these issues are discussed in Renfro 2011). It became plausible to regard the unreliability of econometric results analysed by Dewald and his colleagues as a temporary phase—as the growing pains of the new discipline.

There was continual interaction between progress in computing and the development of new econometric techniques. The ability to undertake far more rapid calculations made it possible to develop new tests and estimation methods and to implement new strategies for econometric work. The battery of statistical tests with which econometricians could appraise their models increased, Monte Carlo methods being used to discriminate between different statistics, even when analytical methods were unable to do so. There was a renaissance of data-driven methods, from vector autoregressive modeling to general-to-specific modeling, and new methods whereby theory could be tested against data. Maybe procedures that had previously required that the investigator be led by his or her own judgement were turned into routine decisions that could be made on the basis of computer-generated statistics or even incorporated into the computer package itself. Algorithms be used could select as well as estimate models.

The spread of more advanced computer technology was also connected to the development of new sources of data. The Penn World tables, dating from the end of the 1970s and regularly extended, provided a set of internationally comparable national accounts

data. Economists also began to collect and analyze much larger microeconomic data sets, with survey and panel data increasing in quantity and becoming available to increasing numbers of economists. The study of finance was transformed with the analysis of data produced by the automated systems used in financial markets: from having data on opening and closing prices, and daily average data, economists could access data on individual transactions.

There were many new techniques for economic analysis based on the use of computers. Calibration methods as used by real business cycle theorists were viewed as a new method (even though such methods have a much longer history). Agent-based modeling involves simulation methods to model behavior in situations which, due to the complexity of the interactions involved, would be incapable of analysis using methods traditionally used in economic theory. Behavioral and experimental economics makes use of on computer technology to ensure the conditions needed for what are believed to be valid experiments to take place.

### *Economists, social ills, and public policies*

The rise of applied economics from the mid-60s onward has often been tied to the social agitation and ills of the times, including pollution, the urban crisis and the rise of crime, the Civil Right movement, the Vietnam War, the energy crisis, stagflation and the rise of radicalism and neoconservatism alike. Yet, their exact influence on economic scholarship is still unclear. One way to approach such influence is through its effect on students' demands. Fleury 2012 studies the consequences of social agitation on students' growing dissatisfaction with the way economics was made and taught, which raised raised a “relevance” challenge.

Not everyone agreed on what “relevant” meant, however. During the seventies, students had in mind scholarship bearing on citizens' everyday life. For researchers in the eighties and nineties, relevance rather meant tied to the real-world through observation and empirical work in easily understandable ways. This may explain why the use of natural experiments (Card and Krueger 199\*) became fashionable at that time. Another way to assess the impact of social troubles on science is through funding. Reflecting on the changes within social sciences at large, Solovey (2013) points that the Ford Foundation's Behavioral Science Program was initially aimed at funding basic research, but was soon re-oriented toward research useful to cure social diseases such as juvenile delinquency. An exhaustive account of how RAND's scholarship was reoriented toward applied work and social welfare research beginning in the mid-60s is also provided by Jardini ([2013] 1996). Jardini explains how Johnson's response to social unrest, his *War on Poverty* and *great Society* programs of 1964, entailed the import of the Planning-Programing-Budgeting System, designed by Hitch and Mckean for defense planning purpose, into other governmental bodies for rational policy planning. The use of cost-benefit analysis as a basis for policy design and evaluation was challenged by public figures such as Paul Ylvisaker at the Ford Foundation, who thought that policies should be decided by local communities through Community Action Programs, but nevertheless voted and implemented. The application of a tool shaped for defense to social welfare program proved difficult, as criteria, cost and benefits were more difficult to identify and quantify, in particular in urban, poverty and health fields.

Jardini's narrative thus suggests that a major channel whereby social unrest and economic and political disturbances may have weighted on economic research was through governments' responses, political and legal. A detailed case study of such process is provided by Banzhaf's (2009) account of how Cost-Benefit Analysis was used for water resources

management. He shows that competing ways of using cost-benefits tools for water resources management rested upon diverging conceptions of welfare and of the economist' role in the policy-making process. As the Water Resources Council proposed that multi-objective benefit-cost analysis be legally introduced into water agencies' practices in the 60s, two irreconcilable visions clashed. A team led by Harvard economist Arthur Maas, Robert Dorfman, and Otto Eckstein, argued that traditional cost-benefit analysis couldn't take into account the many objectives Congress members had in mind, efficiency but also inter-household equity, regional development, protection from flood, ect. and handle non-markets costs and benefits. They argued that it was the economist's task to identify the various objectives at play, formalize them, optimize water resource systems for these multiple objectives and proposed different systems for different objective trade-off from which the Congress could choose. Other economists, such as Robert Robert Haveman and Myrick Freeman, associated with *Resources for the Future*, opposed such practices on the ground that multiple objectives could be collapsed into a social welfare function using market or shadow prices, and that scientists should refrain from interacting with the political decision process . This historical scholarship indicates that the tools developed by economists and the way they were applied to specific issues were largely shaped by how scientists and politicians envisioned the policy-making process, from policy-objectives to the design of solution and their evaluations.

#### *Sites for competing traditions in applied research*

Historians of economics have traditionally paid much attention to schools of thought, frequently associating these with specific institutions. This raises the question of whether

such changes as have taken place in recent years are associated with particular institutions: with specific sites where economic research takes place. Looking at the sites where these applied economists were trained and undertook their research also highlights key transformations of the discipline. Chicago economists have argued, citing the list of Nobel Memorial Prize winners, that success in economics is highly correlated with having some connection with the University of Chicago. At Chicago, there was, for a long time, an emphasis on solving problems using simple price theory and it is probably no coincidence that “economics imperialism”, the notion that economic techniques could be applied to problems arising on other social sciences, and “Freakonomics”, the application of such methods to everyday puzzles, were more closely associated with Chicago than with any other institution. Carnegie Mellon, dominated for many years by Herbert Simon, encouraged a distinctive approach to economic theory. In contrast, it can be argued that economics at M.I.T., one of the most influential institutions after the Second World War, in which economics was closer to engineering, represents a different approach to applied work based on more complex models. This raises the question of whether different institutions have fostered different conceptions of “applied work”.

However, whilst focus on universities and traditional economics departments may have been appropriate in the past, that is no longer true. Like many of their contemporaries, many Clark medallists, were closely associated with extra-academic bodies. It has become increasingly common for authors of journal articles to list more than one affiliation, one of these frequently being a research institute outside the traditional university structure. Martin Feldstein was the architect of a profound transformation of the structure and philosophy of the NBER that reflects a conception of applied work very different from that held by its founders. Clark medallists have been associated with The World Bank, the IMF, the UN,

which employ large numbers of economists and their role in certain fields (notably development, monetary, international), the role of which in economics is large and under-researched (but see Coats 1986; Coats 1997; Alacevich 2009). As long as the focus of historical research is on theory rather than applied work, the role of these will not be fully understood. Other have served the Council of Economic Advisers, and many economists have worked at central banks, which are active sites for applied research. A brief analysis by Bennett McCallum (in Backhouse and Salanti 2000), backed up by his own experience, argued that the character of their work having become much closer to work carried on in academia. If correct, this change in the relationship between central banks and academia suggests that there is scope for more systematic historical research that, because of the nature of central banks, seems certain to relate to the question about applied economics. Within academia, business schools have become more important. Outside academia there is also the transformation of the think tank landscape since the 1970s with the rise of what political scientists term “advocacy” think tanks, most of which undertake policy-oriented applied work.

Though sites of research in a different sense, mention should also be made of the proliferation of journals. There has been a massive expansion in the number of academic journals, many of which specialize in applied fields. The AEA, which in 1945 published a single journal, the AER, now publishes no fewer than seven journals. The Royal Economic Society augmented its *Economic Journal* with the *Econometrics Journal*. Journals can promote attitudes towards applied work. The AER and the Economic Journal, in response to the charge that they published too much theory, monitored the proportion of their articles that contained theoretical and empirical work. The *Journal of Public Economics* (established 1972) had a team of 20 associate editors, balanced between those favoring theoretical and

empirical work; theoretical work dominated in the early years but the proportion of empirical articles rose. In addition, working paper repositories have vastly increased the availability of “unpublished” material. This poses issues of fragmentation, given that the volume of literature is such that it has become impossible to read everything that has been published. To this could be added the rise of scholarly societies and academic networks in applied fields. The development of internet search engines poses further questions about what economists read and the potential fragmentation involved when economists can select which types of literature they get to see. In such a world, the classification of research, in systems such as the JEL classification, becomes a potentially significant issue (see Cherrier 2014).

## **6. A research agenda**

Economics has changed significantly since the 1970s. Our conjecture is that these changes involve a changed relationship between economic theory and applied economics, with consequent changes in the way economics is itself conceived, hence the title, “Becoming applied.” Economists no longer view economic theory as standing above applied work in the way as they had by the end of the 1960s and economics has developed in many completely new directions, raising the question of whether there has been a change in what it means to do applied work. The project will inevitably be seen as a successor to *Toward a History of Applied Economics* (Backhouse and Biddle 2000). However, we suggest that it is more helpful to consider it as extending the argument in *From Interwar Pluralism to Postwar Neoclassicism* (Morgan and Rutherford 1998), which proposed the view, now widely accepted in the historiography of economics, that the period centered on the Second World War involved a change from the a pluralist approach to economics, centered on the institutionalism, itself a very broad movement, towards a less open, more technical



neoclassical economics. There was a specifically American story, for in the 1930s and 1940s there were very substantial differences between economics in the United States, Britain, Germany, France and other European countries. Our story, though still dominated by developments in the United States, is more international, reflecting the changes explored in *The Internationalization of Economics since 1945* (Coats 1997). We start from a position on the role of economic theory that is a development of the Morgan-Rutherford story about the establishment of neoclassical economics, conjecturing an interpretation of what has happened since.

Whilst we believe we have adduced significant evidence for it, our interpretation of the period since the 1970s remains a conjecture. Further work is necessary to test that conjecture and to develop the historiography. For example, what we have said about the spread of computing in economics may seem obvious, and many economists will naturally augment it with a simple causal story, possibly involving pent-up demand for computing power and a response to an increased supply for reasons that are exogenous to the discipline. However, there are reasons to believe that it may be more complicated than this. Renfro (2003, 2011) has approached the economics-technology nexus by studying the effect of the development of diversified software packages in micro- and macro-econometrics, financial and spatial econometrics on economists' practices. Even where common mathematical and statistical techniques are used, he argues, the diversity in the software used reflects the variety of applications in which the relationship of science to technology can play out differently. Hardware cannot be used without software, and software needs to perform the functions required of it, which will not necessarily be the same across different fields. He pointed to the possible differences of purposes and standards between the programmers who developed the software and its academic users, thereby raising the possibility that the development of

technology needs to be considered alongside the development of economics, allowing for the possibility that this may work out differently in different fields. It may not be right to consider technology as passively supporting scientific progress.

This raises many questions, some of which we hope that future research, including papers selected for the HOPE conference in 2016, will address. Underlying all of these questions is the problem one relating to the meaning and significance of applied economics. This question may prompt others which are not listed below.

- How have economists conceived the notion of applied economics, and how has this affected their ideas of what economics is? What is the relation of applied economics to other types of research?

Here, it may be useful to look outside economics, for historians of science have explored the origins and meanings of the notion of applied science. There has been extensive debate over what has been termed the “linear model” of the relationship between pure, or fundamental, science, and technology (e.g. Alexander 2012, Edgerton 2004). Lucier (2012) has surveyed the uses of the notions of “pure science” and “applied science” throughout the nineteenth century, and points that they became increasingly used as the possibilities to commercialize scientific knowledge rose, leading to concerns of corruption. Those scientists who wanted to emphasize their distance from the market place relied on “pure,” while those scientists who accepted patents and profits on their research were “applied.” This raises the question of whether such factors have been at work in economics in recent decades, a question that seems highly relevant given the increased commercialization of society and the rise of private forecasting bodies, from bodies such as Data Resources Incorporated, founded by Otto

Eckstein, to the research departments of commercial banks and other firms in the financial sector. Is it true that “applied economics” has sometimes meant “a commercially valuable product”? More generally, it has been argued by Bud 2012 that “applied science” was a concept developed in the US and UK to serve the social purpose of scientists. He suggests that the word was used by those scientists who insisted on remaining “pure” to name those who were willing to get involved in public affairs, with the danger of having their science colored with ideological beliefs. Have such fears played a role in shaping applied economics as it developed in the midst of the social problems and ideological battles that have dominated the period under consideration?

*Processes of unification and fragmentation*

- What has happened to the notion of core and how has this been connected to changes in the discipline? How have changing notions of the core affected the relationships between theory and application since the 1970s? Is there a preexisting pure science? Otherwise, what are the relationships between core and applied from the seventies onward
- How have applied fields evolved since the 1970s? Are there differences between older fields that have been reshaped (e.g. development economics; public finance/public economics; labor economics) and new fields dating from the 1960s and 1970s (e.g. urban economics, health economics, the economics of education, law and economics) and even newer fields such as behavioral or experimental economics?

*The rise of new techniques and their consequences*

- How has the rise of experimental and behavioral economics affected the way economists have conceived of applied economics and of economics more generally?
- What has been the relationship between new econometric techniques and the fields in which they have been developed? How have new econometric techniques affected specific fields (e.g. microeconomics, macroeconomics, finance, development economics) and the discipline as a whole? Related to this, what has been the role of computer technology in shaping applied economics and the way it is viewed? How far has what Agrist and Pischke (2010) have called the credibility revolution provided an effective answer to the skepticism about econometric techniques articulated in works such as Leamer (1983)?<sup>8</sup>
- What have been the causes and implications of the production, use and commercialization of big data? How has this affected fields such as finance and labor economics? Potential subjects include the origins and influence of the Penn World tables, interactions between government and academia in large scale surveys, the origins and use of family spending and household surveys, or surveys such as the National Longitudinal Survey of Youth.

*The sites where applied work has been undertaken*

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<sup>8</sup> See also Backhouse and Durlauf 2009.

- How have the sites in which economic knowledge has been produced since the 1970s changed and what has been the effect of this on the way applied economics has been conceived? What has been the effect of economists working in business schools, institutes of public policy and regional studies units, as well as bodies outside universities mentioned above?
- Does the claim that those scientists wanting to emphasize their distance from the market place focused on “pure science,” while those scientists who accepted patents and profits on their research favored “applied science” apply to economics? How far has the growth of applied economics been linked to the multiplication of commercial forecasting institutes and consultancy firms (e.g. Eckstein’s DRI model, or the involvement of economists in hedge funds), or to the employment of economists in business (including major private banks).
- What happened to the NBER under Feldstein? What have been the implications of the growth of research organisations that are primarily the centers of networks rather than homes for in-house researchers?
- What have been the implications of the growth of organizations such as central banks, the World Bank, the IMF as centres of economic research?
- What has been the role of patronage, including government policy, in shaping applied economics?

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