A History of the Histories of Econometrics

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> *The understanding procedure*. Another sort of question that the scientist has to answer is: why did it happen? Why did this situation exist? Why did the events follow the course they did? The answer to these questions constitutes the rational part of the investigation. By the power of his mind the scientist tries to bring some reasonable order into the happenings and the things he observed.
>
> —Ragnar Frisch, the Yale Lectures (2011)

Introduction: 1985

Wikipedia (on 10 August 2010) lists the following science-and-technology-related events that happened in 1985:

- 19 February: The first artificial heart patient leaves a hospital.
- 20 February: Minolta releases world's first autofocus single-lens reflex camera.
- 4 March: The Food and Drug Administration approves a blood test for AIDS, used since then to screen all blood donations in the United States.

We would like to thank the participants of the *HOPE* 2010 conference for their contributions and comments; see footnote 7 for their names. We also would like to thank Duo Qin for her many valuable comments, and Marcel would like to thank Mary Morgan in particular for numerous conversations. We also would like to thank Alain Pirotte and the external referees for their excellent reports.

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- 24 July: Commodore launches the Amiga personal computer.
- 20 November: Microsoft Corporation releases the first version of Windows.
- 10 December: Franco Modigliani receives the Nobel Prize "for his pioneering analyses of saving and of financial markets."

However unique and special these events are, they do not necessarily make 1985 a special year.

It is for another list of remarkable events that we have selected 1985 to introduce the subject of this article:

- 19 August: At the Fifth World Congress of the Econometric Society, for the first time, an Invited Symposium on Econometric Methodology was held. The invited speakers were David F. Hendry, Edward Leamer, and Christopher Sims. Adrian R. Pagan (1987) was discussant and Angus Deaton was the chair.
- 17–18 December: A two-day symposium was held to mark Joop Klant's retirement from the Chair of History and Philosophy of Economics, University of Amsterdam. The papers presented were published by Cambridge University Press in 1988 (De Marchi 1988a) and were as follows: "An Appraisal of Popperian Methodology," by Daniel M. Hausman; "The Natural Order," by Klant; "Ad Hocness in Economics and the Popperian Tradition," by D. Wade Hands; "Popper and the LSE Economists," by Neil De Marchi; "The Case for Falsification," by Terence W. Hutchison; "John Hicks and the Methodology of Economics," by Mark Blaug; "Finding a Satisfactory Empirical Model," by Mary S. Morgan; "The Neo-Walrasian Program Is Empirically Progressive," by E. Roy Weintraub; "The Case for Pluralism," by Bruce J. Caldwell; "Thick and Thin Methodologies in the History of Economic Thought," by Donald N. McCloskey; and "Economics as Discourse," by Arjo Klamer.
- 29 December: At the Ninety-Eighth Annual Meeting of the American Economic Association, a joint session with the History of Economics Society was held titled "First Forays into the History of Economics."[1] The papers presented at the session were these: "Cowles' Problems Revisited," by Roy J. Epstein; "Statistics without Probability and Haavelmo's Revolution," by Mary S. Morgan (1987); and

1. That was how it was announced (AEA 1985, 907). According to Morgan, it was probably a misprint; it should have been "Econometrics" instead of "Economics."

"The Development of the British School in Econometrics," by Christopher Gilbert (1986).[2] Edward Leamer was discussant, and David Levy presided over the session.

- 30 December: At the same AEA meeting, another joint session with the HES was held on "Popper and the LSE economists." The papers presented were as follows: "Popper and the LSE," by Neil De Marchi (1988b); "Ad Hocness in Economics," by D. Wade Hands (1988); "Popper Misapprehended," by Daniel M. Hausman; and "Testing and the Information Content of Theory," by Dale Stahl. Bruce J. Caldwell, Alexander Rosenberg, Chris Archibald, and Lawrence A. Boland were discussants, and E. Roy Weintraub presided over the session.

Three other relevant events of 1985 were the publications of Weintraub's book *General Equilibrium Analysis*, Michael McAleer, Adrian R. Pagan, and Paul A. Volker's article "What Will Take the Con out of Econometrics?,"[3] and the founding of the journal *Econometric Theory* by Peter C. B. Phillips. Phillips was not only the founder of *Econometric Theory* but also its editor, starting with its first issue published in 1985 and continuing even today. The first issue contains an extensive explanation of the objectives of the journal's editorial policy. It is remarkable that to four obvious objectives, he added two historical aims:

> 5. To publish historical studies on the evolution of econometric thought and on the subject's early scholars. In its present stage of evolution, the subject of econometrics is still visibly rooted in the historical tradition that slowly took shape in the early years of this century, which gained definite form in the work of Frisch and Tinbergen in the 1930s and crystallized in the studies of Haavelmo and the research of the Cowles Commission during the 1940s, the latter very largely under the inspiration of Marschak and Koopmans. Many of us have much to learn from this historical tradition and from the talents, concerns, and achievements of these forerunners of present day econometrics. Happily, this field of research in the history of thought is now under development. In support of this field, ET wishes to encourage the publication of articles that explore the nature of this historical tradition, examine the evolution of econometric thought from its foundation research, and study the subject's early scholars. . . .

2. This is also the title of his DPhil (Oxon) thesis defended in 1988.
3. This was a response to Leamer 1983.

> 6. To publish high-level professional interviews with leading econometricians. . . . These in-depth interviews will offer the opportunity of a wide ranging personal commentary on major schools of thought and reveal individual insights into the evolution and the present state of econometric research. Through these interviews the econometrics community will be able to learn more about the human side of research discovery and come to understand the genesis of the subject's main ideas from some of its finest minds. Most particularly, those readers who have not had the benefit of personal contact with some of our leading econometricians may now have the opportunity to hear their voices, not only on matters concerned with their own research but also on their intellectual background and influences and on their methods of teaching and research. It is hoped that these interviews will awaken an intellectual excitement in new and prospective generations of econometricians and will encourage them to make the fullest use of their own talents. (Phillips 1985, 4)

These many facts show that in 1985 there was a close mutual interaction between history of economics, philosophy of science, and econometrics. This interaction still continues today: in 2006 a new Palgrave series, Handbook of Econometrics, was inaugurated with a volume on econometric theory (Mills and Patterson 2006). Strikingly, this volume starts with a (historical) overview (by Aris Spanos) of different approaches "that should encourage thought, discussion, and perhaps some controversy"; a chapter (by Kevin Hoover) on methodology, a subject that was "generally absent from textbooks" on econometrics, introductory and advanced; and two on the history of econometrics (one by Richard W. Farebrother and the other by Christopher Gilbert and Duo Qin).

As a matter of fact, the present volume on the history of econometrics also originated because of a close interaction with econometricians. The North American Summer Meeting of the Econometric Society at Duke University in June 2007 had—for the first time—a session on the history of econometrics, organized by Weintraub. The session was a success: "The history session was well attended and the discussion was lively, although the audience was almost entirely practitioners rather than historians" (Hoover 2010, 20). An outcome of this discussion was that the editors of *HOPE* and the session participants started to consider whether it would be useful and feasible to have a conference solely on the history of econometrics.

To investigate its feasibility, Marcel Boumans started to survey the literature on the history of econometrics to see who the earlier contributors were and whether they were still active; his goal was to find out if a critical mass of historians would want to "turn up the heat and bring the history of econometrics back to the forefront of the field" (Hoover 2010, 19). It soon became clear to Boumans that interest in the history of econometrics has arisen primarily from within econometrics itself and that its histories have been written mainly by econometricians. Such is the case with the accounts produced, for example, by Roy Epstein (1987) and Duo Qin (1993). Only a few papers on econometrics have been written by historians of economics, and there exists only one book-length treatment of the subject by a historian: Morgan 1990.

To put things another way, the situation today is the same as it was in 1985, the year that saw so many events suggesting that econometrics might begin to finally receive the serious historical attention it deserves. The assessment then by Roy Weintraub (1985, 10) of the state of historical studies of econometrics still holds true today: "It is obvious that historians of economic thought must be comfortable with modern work in economics. That is, the history of economic thought should not exclude an interest in the tools of mathematics, econometrics, and 'modern high theory.' It is a minor scandal that there is no comprehensive history of either the rise of econometrics or the mathematization of economics."[4] Although occasionally a history of econometrics appears in the *Journal of the History of Economic Thought* or in *HOPE*, the number of articles on the topic remains very small with respect to the total number of articles on the history of economics.

So, on the one hand, one can be pleased that the histories of econometrics are at least written by the econometricians themselves, but, on the other hand, this leads therefore to all kinds of unintended but nevertheless unavoidable historiographical biases. As we discuss below, one reason for econometricians to be interested in history is that history can be used to delineate econometrics, that is, to set its disciplinary boundaries, with respect to its aims, its methods, its scientific values, and so forth. The risk is that history is used to legitimize set boundaries, while actually these boundaries should be historicized. Another problem with using history to delineate is that most histories are written from the perspective of econometrics as a separate science, and so they are constructed

4. He mentions, however, "some work in England by Mary Morgan, sponsored by the Social Science Research Council and guided by Professor David Hendry" (140). It should be noted that Weintraub 1985 was on the history of mathematical economics only; so even that work did not rectify the "minor scandal" he had identified.

apart from the histories of connected disciplines like mathematics, economics, statistics, computer science, physics, engineering, biometrics, psychometrics, genetics, accountancy, actuarial sciences, and so forth. Moreover, a delineating history has a blind spot for applied research, where disciplinary boundaries are usually blurred, and a blind spot for what is happening outside the geographic areas of appointed founding fathers. Most histories give the impression that econometrics is a European–North American science, ignoring developments in the rest of the world. The conference aim was to rectify these historiographical biases by inviting contributions on these issues; the subsequent chapters are a result of this invitation.

Chart Session

Another goal of the conference was to gain some understanding of the historical background of these histories of econometrics, to get at a history of the histories of econometrics, so to speak. Questions such as where this interest of econometricians in the history of econometrics comes from, and why—or why not—historians of economics might be interested in econometrics, were questions we wished to have addressed at the conference. One way to deal with these questions was to take advantage of the fact that several of the people involved in the events of 1985 had or still have their home offices at Duke University, which has been the site of practically all previous *HOPE* conferences, including the one represented by this volume. The original idea was to have a "witness seminar" with the 1980s witnesses. In such a seminar, "several people associated with a particular set of circumstances or events . . . meet together to discuss, debate, and even disagree about their reminiscences" (Tansey 1997). But because a witness seminar focuses on a "particular set of circumstances or events," we started to doubt whether we actually could have such a seminar. Our main goal, after all, was to investigate why there was such an interest in the history of econometrics in the 1980s. A decade, however, is not an "event"; what, then, would we focus on? We decided therefore to have something slightly different, a session with the Duke people (currently called the *HOPE* group) and the other conference participants that would stimulate their collective memories. We called the session not a "witness seminar" but a "chart session."[5] Morgan and Boumans had pre-

5. The whole session was videotaped and will be available online.

pared several charts to stimulate the participants' memories.[6] These charts showed diagrams and graphs of "relationship among disciplines," "types of histories," "number of publications," "kinships," "PhDs and training," "institutions/journals," and "geography."

The surprising result of the session was that after a warm-up stage, participants started to recall several events that appeared to be important catalyzers for several works on the history of econometrics.[7] This is the reason why we started above with listing the 1985 events. It seems that for this kind of oral history, even if it is not, properly speaking, a witness seminar, events are essential.

As revealed in the chart session, two events of the 1980s turned out to be crucial. The very early (8:00 a.m.) Sunday morning session, "First Forays into the History of Economics" (by which was really meant "Econometrics"), was very well attended and inspired De Marchi and Gilbert (1989) to publish the papers in a special issue of the *Oxford Economic Papers*, of which Gilbert had been general editor from 1979 to 1988. A. W. Coats (1987, 62), in an account written for the *History of Economics Society Bulletin*, mentioned this session explicitly: "Incidentally, the new interest in the history of econometrics—exemplified by the excellent History of Economics Society session organized by Neil de Marchi at the December, 1985 AEA meeting—is one of the most exciting current developments in the subject, for it promises not only to enhance our knowledge and understanding but also to widen the professional audience and support for the history of economics." The other event was the 1989 Capri conference, where several of the characters involved in this history publicly distanced themselves from a Lakatosian historiography. The consequences of this step are discussed below.

Is Econometrics Science?

The reason for the convergence of econometrics, history of economics, and philosophy of science in the 1980s was the accusation made in the

6. Because of the ash clouds from the Icelandic volcano Eyjafjallajökull, Morgan and Dupont-Kieffer were not able to attend in person; they participated via the Internet.

7. For writing this introduction, we benefited from reminiscences shared by the participants (in order of sitting): Hoover, Jim Wible, Weintraub, Eric Chancellier, Aiko Ikeo, Robert Dimand, Gilbert, Hsiang-Ke Chao, Charles Renfro, Allin Cottrell, Qin, Daniela Parisi, John Aldrich, Jeff Biddle, Tom Stapleford, Bernardo Coelho, Sofia Terlica, Alain Marciano, De Marchi, Paul Dudenhefer, and (online) Morgan and Dupont-Kieffer. This chapter is a reflection of this session.

1970s that econometrics was unable to meet the high expectations of the 1950s as producer of reliable predictions and policy advice. David Hendry especially, in his London School of Economics (LSE) inaugural lecture "Econometrics—Alchemy or Science?," used the opportunity to revisit the famous Keynes-Tinbergen debate as a backdrop to reiterate the scientific possibilities of econometrics. This lecture was published in 1980, the same year in which Sims's "Macroeconomics and Reality" was published in *Econometrica*, the article in which his vector autoregression (VAR) approach was introduced.

What started as a critique by John Maynard Keynes (1939) of Jan Tinbergen's first League of Nations volume (1939) soon became a more general debate about the role of econometrics and what it might be able to achieve. Tinbergen was commissioned in the late 1930s by the League of Nations to perform statistical tests on business cycle theories. The results of this study were published in a two-volume work, *Statistical Testing of Business-Cycle Theories* (1939). The first volume contained an explanation of this new method of econometric testing as well as a demonstration of what could be achieved, based on three case studies. Keynes's (1939) critique was that the technique of multiple correlation analysis that had been adopted by Tinbergen was solely a method for measurement. It contributed nothing in terms of either discovery or testing. The implication was that if the economic theorist does not provide the modeler with a complete set of causal factors, then the measurement of the other causal factors will be biased. Moreover, Keynes argued that some significant factors in any economy are not capable of measurement, or may be interdependent. Another of Keynes's concerns was the assumed linearity of the relations between these factors. He also noted that the determination of time lags and trends was too often based on trial and error, and too little informed by theory. And last but not least was the problem of invariance: would the relations found also hold for the future? These questions remained central to the subsequent debate.

Taking into account all of these concerns and Tinbergen's responses to them, Keynes (1940, 156) came to the conclusion that econometrics was not yet a scientific approach: "No one could be more frank, more painstaking, more free from subjective bias or *parti pris* than Professor Tinbergen. There is no one, therefore, so far as human qualities go, whom it would be safer to trust with black magic. That there is anyone I would trust with it at the present stage or that this brand of statistical alchemy is ripe to become a branch of science, I am not yet persuaded. But Newton, Boyle and Locke all played with Alchemy. So let him continue." Hendry labeled Keynes's list of concerns as "problems of the linear regression model," which, according to him, consisted of using an incomplete set of determining factors (omitted variables bias); building models with unobservable variables (such as expectations), estimated from badly measured data based on index numbers; obtaining spurious correlations from the use of proxy variables and simultaneity being unable to separate the distinct effects of multicollinear variables; assuming linear functional forms not knowing the appropriate dimensions of the regressors; misspecifying the dynamic reactions and lag lengths; incorrectly prefiltering the data; invalidly inferring causes from correlations; predicting inaccurately (nonconstant parameters); confusing statistical with economic significance of results; and failing to relate economic theory to econometrics. To Keynes's list of problems, he added stochastic misspecification, incorrect exogeneity assumptions, inadequate sample sizes, aggregation, lack of structural identification, and an inability to refer back uniquely from observed empirical results to any given initial theory.

Hendry (1980, 402) admitted that "it is difficult to provide a convincing case for the defence against Keynes's accusation almost 40 years ago that econometrics is *statistical alchemy* since many of his criticisms remain apposite." The ease with which a mechanical application of the econometric method produced spurious correlations suggests alchemy, but according to Hendry, the scientific status of econometrics can be regained by showing that such deceptions are testable.

There are two issues of particular interest here. First, Hendry, needing to say something about what science is, referred to and used mainly Popperian literature (Chalmers 1976; Popper 1968, 1969; Lakatos 1974), but he also mentions Kuhn 1962. Second, research for Hendry's LSE lecture was financed partly by a grant (HR6727) from the Social Science Research Council to the Study in the History of Econometric Thought at the LSE. This grant was also used to finance Mary Morgan's PhD research on the history of econometrics that she, in 1979, had just started at the LSE, supervised by Hendry.[8] In an *Econometric Theory* interview, Hendry explained how he became interested in the history of econometrics:

> Harry Johnson and Roy Allen sold me their old copies of *Econometrica*, which went back to the first volume in 1933. Reading early papers such as Haavelmo (1944) showed that textbooks focused on a small

8. She finished her PhD in 1984. Her 1990 book originated from this thesis.

> subset of the interesting ideas and ignored the evolution of our discipline. Dick Stone agreed, and he helped me to obtain funding from the ESRC.[9] By coincidence, Mary Morgan had lost her job at the Bank of England when Margaret Thatcher abolished exchange controls in 1979, so Mary and I commenced work together. (Ericsson 2004, 779)

Soon afterward, Qin started to study the more recent history of econometrics through to about the mid-1970s, which led to her PhD thesis in 1989, supervised by Hendry, Gilbert, and Stephen Nickell.[10] This interest in the history of econometrics also led to a series of seminars at Nuffield to discuss history with John Aldrich, Gilbert, Morgan, and Qin (Ericsson 2004, 780).

Hendry referred to Keynes's use of the term *alchemy* to discuss the scientific nature of econometrics. Another way to denote this discussion is to see how much econometrics differs from "economic tricks" (or "economystics" or "icon-ometrics"; see Hendry 1980, 388). This characterization is based on a story by Carl F. Christ (1967, 155): "I once had an urgent letter to dictate and, the Economics Department secretary being unavailable, I prevailed upon the secretary of the neighbouring Political Science Department to help. When I proof-read the typed copy, 'econometrics' had been transformed to 'economic tricks.'" This story was probably the motivation for Leamer (1983) to contribute to the debate about the scientific character of econometrics under the title "Let's Take the Con out of Econometrics."[11]

Leamer's paper is very much about the "myth" of science that empirical research is (randomized controlled) experimentation and scientific inference is objective and free of personal prejudice. Though Leamer is ultimately proposing a Bayesian approach, he is using Lakatosian and Kuhnian notions to advocate such an approach. The problem of nonexperimental settings (usually the case in economics) compared with experimental settings ("routinely done" in science) is that "the misspecification uncertainty in many experimental settings may be so small that it is well approximated by zero. This can very rarely be said in nonexperimental settings" (Leamer 1983, 33). Traditional econometrics seems not to admit

9. In 1983 the Social Science Research Council had changed into Economic and Social Research Council.

10. Morgan and Aldrich were her thesis examiners. Her 1993 book originated from this thesis.

11. *Con* here, of course, means to trick or swindle, so to take the con out of something is to take the tricks away. McAleer, Pagan, and Volker (1985) have a similar interpretation.

this "experimental bias," and as such, misspecification uncertainty functions as "Lakatos's 'protective belt' which protects certain hard core propositions from falsification" (34). So, projecting the image that econometrics is like agricultural experimentation (randomized controlled experimentation) is not only "grossly misleading" (31) but also leads, in Lakatosian terminology, to protecting a degenerating program, which means pseudoscience like alchemy.

In addition, "the false idol of objectivity has done great damage to economic science" (36). If we want to make progress, according to Leamer, "the first step we must take is to discard the counterproductive goal of objective inference" (37). Inference is a logical conclusion based on facts, but because "the sampling distribution and the prior distribution are actually *opinions* and not *facts*, a statistical inference is and must forever remain an *opinion*" (37). Moreover, Leamer considers a fact as "merely an opinion held by all, or at least held by a set of people you regard to be a close approximation to all" (37). In a footnote he refers to Kuhn 1962 and Michael Polanyi's *Personal Knowledge* (1964) as a philosophical backing up for this notion of fact as "truth by consensus."

The problem with using opinions, however, is their "whimsical nature." An inference is not "believable" if it is fragile, if it can be reversed by a minor change in assumptions. It is thus the task of the econometrician to withhold belief until an inference is shown "to be adequately insensitive to the choice of assumptions" (43).

To prevent econometrics from becoming alchemy, Hendry, Leamer, and Sims developed their own methodologies: the general-to-specific approach, the Bayesian approach, and the VAR approach, respectively, leading to the debates of the mid-1980s. Besides the already mentioned symposium at the Fifth World Congress of the Econometric Society, according to Dale J. Poirier (Hendry, Leamer, and Poirier 1990), these debates took place at sessions of the North American Summer Meeting in June 1986 at Duke University and the Australasia Meeting in August 1988 at the Australian National University. The 1986 North American meeting had Hendry's Walras-Bowley Lecture with a subsequent panel discussion with Robert F. Engle, Daniel McFadden, John W. Pratt, Eugene Savin, and Sims as panelists and John Geweke as moderator. The 1988 Australasian meeting had a symposium on economic methodology with Dennis S. Aigner, Clive W. J. Granger, Leamer, and M. Hashem Pesaran as contributors. Disappointed "in the value-added of public panel discussions" (Hendry, Leamer, and Poirier 1990, 172), Poirier organized a "dialogue" in *Econometric Theory* between himself, Hendry, and Leamer (Hendry, Leamer, and Poirier 1990).

This *Methodenstreit* fits nicely into a longer tradition of debates about the most appropriate empirical scientific method: the already earlier mentioned Keynes-Tinbergen debate and the measurement-without-theory debate between Tjalling Koopmans (Cowles Commission approach) and Rutledge Vining (National Bureau of Economic Research approach). These well-known debates are all strikingly related to econometric methodology. The advantage of debates is that they are often less technical and so receive more attention from outside their field.

Delineation of Econometrics

This worry about developing an econometric methodology to help prevent economics from becoming a pseudoscience, the underlying motivation of the 1980s debate, is not so different from the motivation of the founding fathers of econometrics to design a program for turning economics into a science.[12] The beginnings and subsequent development of econometrics are closely related to attempts to find the most appropriate scientific empirical methodology for economics. According to Ragnar Frisch, who coined the term *econometrics* in his very first paper in economics, "Sur un problème d'économique pure" (1926), the term means the unification of economic theory, statistics, and mathematics: "Intermediate between mathematics, statistics, and economics, we find a new discipline which, for the lack of a better name, may be called *econometrics*. Econometrics has as its aim to subject abstract laws of theoretical political or 'pure' economics to experimental and numerical verification, and thus to turn pure economics, as far as possible, into a science in the strict sense of the word" (Frisch 1971, 386). In his first editorial of the newly established journal *Econometrica*, Frisch (1933, 1) explained the term *econometrics*:

> Its definition is implied in the statement of the scope of the Society, in Section I of the Constitution, which reads: "The Econometric Society is an international society for the advancement of economic theory in its relation to statistics and mathematics. The Society shall operate as a completely disinterested, scientific organization without political, social, financial, or nationalistic bias. Its main object shall be to pro-

12. For a more detailed history of the founding of the Econometric Society and the "scientization" of economics, see Bjerkholt and Qin 2011a.

> mote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems and that are penetrated by constructive and rigorous thinking similar to what has come to dominate in the natural sciences."

It is apparent that the underlying motivation for this "new discipline," from its beginning, was to turn economics "into a science," that is, to penetrate economics by the "constructive and rigorous thinking" dominant in the natural sciences. Presumably, this is the reason Frisch and Tinbergen were awarded the first Nobel Prize in Economics. As is well known, the Nobel Prize in Economics is not a real Nobel Prize but the Central Bank of Sweden Prize in Economic Science in Memory of Alfred Nobel, instituted at the tercentenary celebration of the Swedish Central Bank. The Central Bank approached the Nobel Foundation and the Royal Swedish Academy of Science (the awarding authority for the prizes in physics and chemistry) for agreement on the conditions and rules for the prize. "There was, however, a certain skepticism towards the new prize idea among some natural scientists in the Academy—partly because of a general reluctance to extend Nobel prizes to new fields, partly because of doubts whether a social science, such as economics, would be 'scientific' enough to warrant a prize of this kind on an equal footing with prizes in 'hard sciences' like physics and chemistry" (Lindbeck 1985, 37–38). Because of this skepticism, it is interesting to see that the first prize was awarded to two founders of econometrics: "Their aim has been to lend economic theory mathematical stringency, and to render it in a form that permits empirical quantification and a statistical testing of hypotheses. One essential object has been to get away from the vague, more 'literary' type of economics" (Lundberg 1992, 3).

Econometrics was originally defined in accordance with its scientification program. This was, however, not the only way econometrics was originally delineated. While designing its program, Frisch together with Charles Frederick Roos and Irving Fisher were also drawing up a list of potential charter members for the Econometric Society in formation. The list consisted of twenty-eight names from eleven countries (Divisia 1953; Bjerkholt and Qin 2011a).[13]

13. Luigi Amoroso, Ladislaus von Bortkiewicz, Arthur L. Bowley, Thomas N. Carver, Gustav Cassel, John Bates Clark, John Maurice Clark, Clément Colson, François Divisia, Griffith C. Evans, Mordecai Ezekiel, Corrado Gini, Keynes, Hans Mayer, Henry L. Moore, Jacques Moret, Bertil Ohlin, Warren M. Persons, A. C. Pigou, Umberto Ricci, Jacques Rueff, Henry Schultz, Joseph A. Schumpeter, Eugen Slutsky, Alfonso de Pietri Tonelli, Gustavo del Vecchio, Harald Westergaard, and W. Zawadski.

Another way to delineate a discipline is to appoint forerunners. With respect to this kind of delineation, it is remarkable that from its first issue in 1933 (until the 1960s) *Econometrica* regularly published articles on denominated forerunners: Emile Borel, Augustin Cournot*, Francis Ysidro Edgeworth*, Francesco Fuoco, William Stanley Jevons*, Hans von Mangoldt, Johann Heinrich Von Thünen*, Vilfredo Pareto*, Léon Walras, Knut Wicksell*.[14]

Gerhard Tintner (1953) used this strategy of appointing forerunners as one way to "define" econometrics. The forerunners he identified were Gregory King, William Petty (in particular, his political arithmetic), Edgeworth, Pareto, and Henry L. Moore (in particular, his synthetic economics). He concluded that the preferred definition of econometrics (still) is "a combination of economics, mathematics and statistics" (31).

From 1939 on, articles on "founding fathers" were published whenever the occasion arose (anniversary or decease): Oskar Anderson, Bernard Chait, Clément Colson*, Georges Darmois, François Divisia, Luigi Einaudi, Irving Fisher, Eraldo Fossati, Frisch*, Yehuda Grunfeld, Leif Johansen, Keynes, Oskar Ryszard Lange, Ta-Chung Liu, Moore*, Hans Peter, Roos, Henry Schultz*, Joseph A. Schumpeter*, Eugen Slutsky*, Abraham Wald*, Frederik Zeuthen.

This tradition of bringing the history of econometrics to the econometrician's attention and awareness more or less died out in the 1960s. Note, for example, that *Econometrica* did not pay any attention to the fact that its first editor, Frisch, and Tinbergen received the very first Nobel Prize in Economics in 1969. As we showed earlier, it was *Econometric Theory*, established in 1985, that continued this tradition. This delineation of econometrics by listing forerunners and founding fathers continues a longer tradition of producing bibliographies of "mathematico-economic writings." Jevons (1878, 398) produced a "bibliography of works on the mathematical theory of political economy," which was published in the *Journal of the Statistical Society of London*, for two reasons: first, "with the purpose of discovering such forgotten works and memoirs," and second, "in the hope that suggestions may be thereby elicited for its extension and correction."[15] The list contained 71 publications. Walras (1878) extended Jevons's

14. The names with an asterisk also had their photograph printed in *Econometrica*. In volume 2, number 4 (1934), there is also a "specimen" printed of Walras's correspondence.

15. Jevons ([1879] 1957, xliii) did not start this tradition: "I should add that in arranging the list I have followed, very imperfectly, the excellent example set by Professor Mansfield Merriman . . . in his 'List of Writings relating to the Method of Least Squares.'"

list to 98 writings. He published this list with a translation of Jevons's call for extensions and corrections in the *Journal des économistes*. The lists were sent around for further "completion" (Jevons [1879] 1957, xx). This resulted in a "list of mathematico-economic books, memoires, and other published writings" in the second edition of Jevons's *Theory of Political Economy*. That list contained 146 publications. In the preface to that edition, he explained how the selection was done: it should contain "an explicit recognition of the mathematical character of economics, or the advantage to be attained by its symbolic treatment. I contend that all economic writers must be mathematical so far as they are scientific at all, because they treat of economic quantities, and the relations of such quantities, and all quantities and relations of quantities come within the scope of the mathematics" (Jevons [1879] 1957, xxi). According to Jevons, the list could be decomposed into four distinct classes. A first class of publications is made up of those "who have not at all attempted mathematical treatment in an express or systematic manner, but who have only incidentally acknowledged its value by introducing symbolic or graphical statements" (xxiv). The second class contains those "who have abundantly employed mathematical apparatus," but misunderstood "its true use, or being otherwise diverted from a true theory" (xxv). The third class contains those "who, without the parade of mathematical language or method, have nevertheless carefully attempted to reach precision in their treatment of quantitative ideas" (xxv). The most important class, however, is the fourth class, those "who have consciously and avowedly attempted to frame a mathematical theory of the subject, and have . . . succeeded in reaching a true view of the Science" (xxviii). These are Jules Dupuit, Cournot, and Walras. But a "truly remarkable discovery in the history of this branch of literature" was Hermann Heinrich Gossen (xxxii).[16]

This tradition was subsequently continued by Irving Fisher. In his doctoral thesis published in the *Transactions of the Connecticut Academy of Arts and Sciences* in 1892, Fisher had appended a "bibliography of mathematico-economic writings." Fisher (1892, 120) had selected out of Jevons's 1888 list "those 50 which are either undoubtedly mathematical or are closely associated logically or historically with the mathematical method." To this list a second list was added, with 66 publications up to 1892. This bibliography was "superseded" by a "bibliography of

16. In the third edition (1888), a list of books was added by Jevons's wife, Harriet Ann Jevons. This list contained 196 writings.

mathematical economics" up to 1897, containing 327 publications appended to the translated 1897 edition of Cournot's *Researches into the Mathematical Principles of the Theory of Wealth*.

This tradition was also continued in *Econometrica* by the editorial aim of organizing surveys of "the significant developments within the main fields that are of interest to the econometrician" (Frisch 1933, 3). The idea was to have four surveys each year, one each on the following fields: (1) general theory, (2) business cycle theory, (3) statistical technique, and (4) statistical information. Surveys were written by Johan Åkerman, Frisch, J. R. Hicks, Nicholas Kaldor, Gabriel A. D. Preinreich, and Tinbergen on field 1; Alvin H. Hansen and Herbert Tout, and Tinbergen on 2; Paul Lorenz, Horst Mendershausen, Paul R. Rider, Roos, and W. A. Shewhart on 3; and Constantino Bresciani-Turroni, Jakob Marschak, Roos, and Hans Staehle on 4. These surveys did not only inform the reader of *Econometrica* about "significant developments" but also promoted new theories, tools, and methodologies. This tradition came to an end after six years.

Besides the already mentioned histories, bibliographies, and surveys, there are several contributions on the history of the Econometric Society by Divisia (1953) and Christ (1983), and on the Cowles Commission by Christ (1952, 1977, 1985, 1994), Clifford Hildreth (1986), and Epstein (1987).[17]

The boiling points of interest in the history of econometrics were initiated by attempts to delineate the discipline, that is, to define its boundaries and to identify the proper scientific approach. What we see is that this delineation is closely related to the images of science adhered to in each period of interest. For Jevons and Fisher a scientific theory should be mathematical. Frisch's image of science, and of his contemporaries, can be identified as the scientific worldview of (logical) positivism. The science images playing a role in the methodological debates of the 1980s were those of Karl Popper, Imre Lakatos, and Thomas Kuhn. These science images in each period played an important role in delineating econometrics and therefore cannot be detached from the written histories in each of these periods.

17. Phillips was the supervisor of Epstein's PhD thesis "Econometric Methodology in Historical Perspective" (1984). "I started to develop serious questions about the general scientific status of empirical econometrics. . . . I felt it was important to explore the problem more deeply in the context of the studies and methodological arguments developed by the researchers who were most influential in shaping econometrics as we know today. To my surprise, I discovered a long history of substantive debates over methodology that complements, and even extends, the critiques put forth recently by some of the most respected modern practitioners" (Epstein 1987, v).

Weintraub's "Long Struggle of Escape" to Find the Most Appropriate Historiography

It seems that thinking about a historiography equipped for econometrics requires taking account of the econometricians' ideals of science. Philosophy of science is a discipline that studies these ideals. So, it seems obvious that a historiography focused on econometrics should be connected to philosophy of science. Despite the obviousness of this connection, it has not been investigated as much for econometrics as it has been for mathematical economics: in his various publications of the history of mathematical economics, Weintraub has written extensively about a historiography for mathematical economics in relation to philosophy of science. Weintraub's (1985, 140) history of general equilibrium analysis sympathized with the developments in econometrics to look for "historical evidence pertaining to the development of the work [methodologists] evaluate." For that reason he quoted Lakatos's (1971, 91) paraphrase of Immanuel Kant: "Philosophy of science without the history of science is empty; history of science without philosophy is blind." Influenced by the philosophical views of that time, that is, of Kuhn and Lakatos, scholars believed that methodology should be historically informed, as De Marchi and Gilbert (1989, 9) also emphasized in their introduction to the special issue of the *Oxford Economic Papers* on the history *and* methodology of econometrics:

> In fact methodology is inquiry into why the accepted is judged acceptable. But standards alter; and while methodology can be suggestive, its suggestions are offered only on the basis of an understanding of past and current practice and the reasons for it. Methodology thus shades imperceptibly into historical inquiry, and indeed cannot do otherwise. To be self-aware, however, practitioners must look to methodology of this historically informed sort.

But their philosophical framework itself was fixed, namely, the developments were characterized as taking place within "Research Programmes in the strict (Lakatosian) sense" (5). The same applies to Weintraub's (1985, i) historical study of general equilibrium analysis, where he "argues that previous methodological investigations have been distorted by the use of inappropriate models taken from the philosophy of science that were developed to appraise work in physical sciences," but nevertheless he took for granted that Lakatos's model of research programs was appropriate for the study of mathematical economics.

According to Lakatos, however, the methodology used to reconstruct a development had to be normative. He was quite explicit about this. Taking its cue from the above paraphrase of Kant's dictum, Lakatos's (1971, 91) paper on the history of science intended to explain

> *how* the historiography of science should learn from the philosophy of science and *vice versa*. It will be argued that (a) philosophy of science provides normative methodologies in terms of which the historian reconstructs "internal history" and thereby provides a rational explanation of the growth of objective knowledge; (b) two competing methodologies can be evaluated with the help of (normatively interpreted) history; (c) any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) "external history."

As a result, "each internal history has its characteristic victorious paradigms" (93).

For Weintraub (2002, 262) this was—a few years later—the main reason no longer to tell "the story of twentieth century economics as the rise and fall, or the progress or degeneration, of various scientific research programs in economics." History should be done from "a perspective based not on asking of science how it should be done, but rather how it was and is done" (267). This is the so-called naturalistic turn: to understand science, whether historically, philosophically, sociologically, or economically, one should look at its practice as it is actually done. Weintraub was not the only one among historians and philosophers of economics who came to this conclusion. A marking event was a conference, organized by De Marchi and Blaug, in 1989 on the island of Capri. The conference intended to investigate whether Lakatos's methodology of scientific research programs "had shown itself to be an appropriate framework for analysing what economics is and is not like" (De Marchi 1991, 1). This neutrally phrased question turned out to be the geyser of a heated debate at the conference, reflected by the organization of the contents of the conference volume (De Marchi and Blaug 1991). The conference organizers and editors of the proceedings decided not to write a joint introduction; instead, De Marchi wrote the introduction and Blaug an afterword. Where De Marchi (1991, 18) observed that the "tone" of many Capri papers contrasted "sharply" with the mood of an earlier Lakatos conference held in 1974—"not a few participants displayed more awareness of difficulties . . . or impatience"—Blaug (1991, 500) sensed hostility:

> No one could possibly have predicted how the mixture of people collected at Capri would react to our instructions but I was personally taken aback by what can only be described as a generally dismissive, if not hostile, reaction to Lakatos's MSRP. Of the 37 participants, I estimate that only 12 were willing to give Lakatos a further run for his money and of the 17 papers delivered at the conference not more than five were unambiguously positive about the value of MSRP.

One of the principal consequences of this conference was an increased interest among historians and philosophers of economics in the subject of scientific practices in economics as an alternative to a rule-based prescriptive approach—the naturalistic turn.

But, as Weintraub (2002, 269) rightly admits, there can be "no escape from 'frameworks.' There is no view from nowhere, no platforms on which I, the historian, can stand apart and aloof from the materials on which I work." For his history of economics as a mathematical science, Weintraub (2002) employs a framework developed by the historian of mathematics Leo Corry. Corry (1989, 411) makes a distinction between what he calls the "body of knowledge" and the "image of knowledge":

> We may distinguish, broadly speaking, two sorts of questions concerning every scientific discipline. The first sort are questions about the subject matter of the discipline. The second sort are questions about the discipline *qua* discipline, or second-order questions. It is the aim of a discipline to answer the questions of the first sort, but usually not to answer questions of the second sort. These second-order questions concern the methodology, philosophy, history, or sociology of the discipline and are usually addressed by ancillary disciplines.

The body of knowledge includes all those contents related to the subject matter of the given discipline; these are its theories, facts, methods, and open problems. The images of knowledge include all claims about knowledge itself: they serve as guiding principles and pose and resolve questions that arise from the body of knowledge, but are not part of and cannot be settled within the body of knowledge itself. These second-order questions, for example, include the following: Which of the open problems of the discipline most urgently demand attention? How should we decide between competing theories? What is considered a relevant experiment? What procedures, individuals, or institutions have authority to adjudicate disagreements within the discipline? What is to be taken as a legitimate methodology?

This division between the body and image of knowledge is not sharp and is historically determined. The answers to the second-order questions depend on the contents of the body of knowledge at a given stage of the discipline's development. Moreover, changes in the body of knowledge may alter these answers. But these answers are not exclusively determined by the body of knowledge; they may be influenced by other, external factors as well. In turn, the image of knowledge plays a decisive role in directing research and further determining the development of the body of knowledge. They constantly interact dynamically.

Corry's motivation for this distinction is that the study of the interaction between these two "layers" "might provide a coherent explanation of the effect of sociohistorical factors on the realm of pure ideas, while avoiding dubious 'strong' explanations that overemphasize the effects of these factors. Such explanations, which attribute the content of theories to factors *absolutely* external to them, lead unavoidably (and sometimes intentionally) to relativism" (412). Of relevance to understanding the history of econometrics is Corry's discussion of how mathematics is different from the other exact sciences: "Mathematics is the only exact science in which statements *about* the discipline may still be inside the discipline" (413), which he called the "reflexive character of mathematics." This distinguishing character of mathematics led to a more precise differentiation between reflexive knowledge and images of knowledge in mathematics. Reflexive knowledge is "all thinking *about* mathematics that is carried out strictly *inside* mathematics" (414), for example, proof theory. Images of knowledge are "all claims about mathematics that at a given historical point are not an integral part of the mathematical body of knowledge" (414); it includes "comprehensive research programs such as Klein's Erlangen program of Hilbert's list of problems of 1900" (414); it also includes the philosophy and history of mathematics.

Using Corry's categories to characterize the role of history in econometrics, it seems that history of econometrics is used as reflexive econometric knowledge, but in contrast to mathematics, it is thinking about econometrics that is not an integral part of the econometric body of knowledge but takes place where the body of knowledge and the images of knowledge interact dynamically. Corry states that second-order questions, which concern methodology and history, are "usually addressed by ancillary disciplines" (411), except mathematics because of its reflexive character, but what we saw is that econometricians also address these questions and that it has been hardly taken over by "ancillary" disciplines.

History of Econometrics as Reflexive Knowledge

As a result of "a renewed interest in econometric methodology" and "the articulation of many distinctive viewpoints about empirical modelling and the credibility of econometric evidence" in the 1980s (Hendry 1986, xi), econometricians like Hendry and Spanos felt the need to bridge the gap between the textbook econometric theory techniques "in all their formal glory" and the ad hoc procedures empirical researchers had to resort to: "Econometrics textbooks encouraged the 'myth' that the main ingredients for constructing good empirical econometric models were a 'good' theoretical model and a menu of estimators (OLS, GLS, 2SLS, LIML, IV, 3SLS, FIML)" (Spanos 1986, xv).

To set up a new methodology, the first section of the first chapter of Spanos's textbook *Statistical Foundations of Econometric Modelling* (1986, with a foreword by Hendry) starts with a "brief historical overview" to delineate its intended scope. Its starting point is a "working definition" of econometrics: "Econometrics is concerned with the systematic study of economic phenomena using observed data" (3).

As a matter of course, a new definition of econometrics leads to a significant revision of the list of forerunners. According to Spanos's history, they are as follows: Thomas Bayes, Daniel Bernoulli, Carl Charlier, Pafnuty Chebyshev, J. M. Clark, Charles Davenant, Abraham De Moivre, Edgeworth, Ronald Aylmer Fisher, Francis Galton, Johann Carl Friedrich Gauss, John Graunt, William Sealey Gosset, Edmond Halley, Reginald Hawthorn Hooker, King, Andrei Nikolaevich Kolmogorov, Joseph-Louis Lagrange, Pierre-Simon Laplace, Adrien-Marie Legendre, Robert A. Lehfeldt, Alexandre Mikhaïlovitch Liapounov, Malthus, Andrei Andreevich Markov, Wesley Clair Mitchell, Moore, Karl Pearson, Petty, Adolphe Quetelet, Schultz, Slutsky, Alfred Russel Wallace, Herman O. A. Wold, Elmer J. Working, Philip G. Wright, and George Udny Yule. Only four persons survived the earlier *Econometrica* lists: Edgeworth, Moore, Petty, and Slutsky. Looking at this list, one can only conclude that "econometrics began as an offshoot of the classical discipline of Mathematical Statistics because the data found in economics had unusual properties" (Granger 2006, xi).

Spanos used for this brief history, besides the earlier works by historians of statistics and probability theories (Cramer 1972, Maistrov 1974, Seal 1967, and Stigler 1954 and 1962) and by the philosopher of science Hacking (1975), the then more recent histories of econometrics: Hendry and

Morgan 1995,[18] and Morgan 1982 and 1984. A new methodology needs new histories.

While Spanos and Tintner still rooted econometrics partly in Petty's political arithmetic, Hendry and Morgan (1995, 4) cut that root off: "Econometrics emerged, not from the long-lost seventeenth-century tradition of Political Arithmetik, but from the nineteenth-century explosion of statistical social science and more particularly from the biometrics tradition which flowered at the end of that century." The readings they provided included articles of only six persons appearing on the *Econometrica* lists: I. Fisher, Frisch, Jevons, Moore, Schultz, and Wald.

Twenty years after the publication of his textbook, Spanos (2006, 16) chose Moore as "the quintessential pioneer" of early-twentieth-century econometrics:

> His empirical studies were instrumental in generating discussions on how the newly developed statistical tools of Galton, Karl Pearson and Yule could be utilized to render economics an empirical science. This early period is important, because some of the crucial weaknesses of the current textbook approach can be traced back to Moore (1911, 1914).

While Moore put econometrics on the wrong track, it was R. A. Fisher who contributed the most important ideas to the development of econometrics. Note that R. A. Fisher did not appear on any of the lists of forerunners, except in Spanos's own 1986 history.

Conclusions

Corry's distinction between the body and image of a science can thus provide an understanding of the history of econometrics and its own continuing struggle with its boundaries. The changes and developments of the econometricians' image of science, including their perception of the views of the philosophers of science at a relevant period, can help clarify the development of econometrics. In other words, understanding the development of econometrics as a modern science also requires understanding the development of the image of science, which includes the history of philosophy of science and the history of economic methodology. In addition, philosophers of science need to be historically informed, and

18. This "Readings in Econometric Thought," as it was then titled, was expected to be published in 1986.

historians of science need to be philosophically informed, that is, they need to be informed about the developments of the philosophical ideas about science. To paraphrase Lakatos: "Philosophy of science without the history of science is empty; history of science without the *history* of philosophy is blind."

Corry's distinction between the body and image of knowledge provides a framework not only to write histories of econometrics but also to write a history of these histories. From its beginnings, econometricians have considered historical knowledge as reflexive knowledge useful to delineate their discipline. As such, the histories written in each period reflect the image of their discipline in that period. We saw that in the 1930s, when mathematics was still considered an essential component of econometrics, many histories referred to the "mathematico-economic" roots, by surveying this literature but also by denominating mathematical economists as forerunners. Cournot, Pareto, and Walras do not appear in any current history of econometrics. Each period has its own list of forerunners and founding fathers.

In the postwar period till the 1980s, the history of econometrics was the history of Cowles Commission econometrics. This changed only in the 1970s when the limits of the Cowles program became visible and "young turks" like Hendry, Leamer, and Sims started to develop alternative programs. These new programs initiated histories of areas "before" and "beside" the Cowles program. Econometrics today is much more considered as statistics applied to economic data, which is reflected by the increased attention for histories of statistics in relation to the history of econometrics and with a more prominent role of R. A. Fisher. We leave it to future historians to delineate the kind of econometrics that is reflected by this volume; we are simply too close to it to see it.

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Part 1
Disciplinary Boundaries