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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. (Frisch 2011, 31)

Introduction: 1985

Wikipedia (read August 10, 2010) gives the following science-and-technology-related events which happened in 1985:

- February 19: The first artificial heart patient leaves a hospital;
- February 20: Minolta releases world's first autofocus single-lens reflex camera;
- March 4: The Food and Drug Administration approves a blood test for AIDS, used since then to screen all blood donations in the United States;
- July 24: Commodore launches the Amiga personal computer;
- November 20: Microsoft Corporation releases the first version of Windows; and
- Franco Modigliani receives the Nobel prize for “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 paper:

- August 19: 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 chairing this session.
- December 17-18: Two-day symposium to mark Joop Klant's retirement from the Chair of History and Philosophy of Economic, University of Amsterdam. The papers presented (de Marchi 1988) were '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 Weinraub, '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.
- December 29: At the Ninety-Eight Annual Meeting of the American Economic Association, a Joint Session with the History of Economic Society was held on 'First Forays Into the History of Economics'[1]. The papers presented at this session were: 'Cowles' problems revisited' by Roy J. Epstein, 'Statistics without probability and Haavelmo's revolution' by Morgan (1987) and 'The development of the British

[1] This is how it was announced (AEA 1985, 907). According to Morgan this is probably a misprint, it should be 'Econometrics' instead of 'Economics'.

school in econometrics' by Christopher Gilbert (1986)[2]. Leamer was discussant and David Levy was presiding this session.

- December 30: At the same AEA Meeting, another Joint Session with the HES was held on 'Popper and the LSE Economists'. The papers presented at this session were: 'Popper and the LSE' by de Marchi (1988a), 'Ad hocness in economics' by Hands (1988), 'Popper misapprehended' by Hausman and 'Testing and the information content of theory' by Dale Stahl. Caldwell, Alexander Rosenberg, Chris Archibald, Lawrence A. Boland were discussants and Weintraub was presiding this session.

Three other relevant events of 1985 were the publications of Weintraub's book *General Equilibrium Analysis* and Michael McAleer, Pagan and Paul A. Volker's article 'What will take the con out of econometrics?'[3], and the foundation of *Econometric Theory* by Peter C.B. Phillips.

Phillips was not only the founder of *Econometric Theory* but also its editor since its first issue published in 1985 till today. The first issue contains an extensive Editorial explaining the editorial policy objectives of this journal. 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

[2] This is also the title of his D.Phil. (Oxon) Thesis defended in 1988.

[3] This was a response to Leamer (1983).

> 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. […]
>
> 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 are many facts to show that in 1985 there was close mutual interaction of history of economics, philosophy of science and econometrics. This interaction continued till 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 though, discussion, and perhaps some controversy", a chapter (by Kevin Hoover) on methodology "generally absent from textbooks" and two on the history of econometrics (by Richard W. Farebrother and Gilbert and Qin).

As a matter of fact this volume on the history of econometrics before us also originated due to 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, organised 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 on its own on the history of econometrics.

To investigate its feasibility, Boumans started to survey the literature on contributions on the history of econometrics to see who the earlier contributors were and whether they were still active to find out whether a critical mass of historians would be interested "to turn up the heat and bring the history of econometrics back to the forefront of the field" (Hoover 2010, 19). Surveying the literature it soon became clear that the interest in the history of econometrics arose from within econometrics itself, and that its histories are mainly written by econometricians. Only a few papers on econometrics were written by historians of economics. According to Weintraub (1985) this was a "scandal":

> 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. (Weintraub 1985, 10)

But he only rectified this scandal partly; his own study was on the history of mathematical economics only.[4]

Twenty-five years after this scandal, the situation has not really improved. Although since Weintraub's call in 1985 occasionally a history of econometrics appears in the *Journal of the History of Economic Thought* and in *HOPE*, the number of articles remains very small in proportion to the general interest of historians in 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 historiographic biases. As will be discussed 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 aim, its methods, its scientific values, etc. The risk is that history is

[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).

used to legitimize set boundaries, while actually these boundaries should be historicized. Another problem with history as a means to delineate is that most histories are written from the perspective of econometrics as a separate science, and so they are constructed separate from the histories of connected disciplines like mathematics, economics, statistics, computer science, physics, engineering, biometrics, psychometrics, genetics, accountancy, actuarial sciences, etc. Moreover, delineating history has also a blind spot for applied research where disciplinary boundaries are usually blurred, but also 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 historiographic biases by inviting contributions to 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 on econometrics, to get at a history of the histories of econometrics so to say. Questions like where this interest of econometricians in the history of econometrics came from, why historians of economics might be interested in econometrics or not, were questions we wished to have addressed at the conference. A way to deal with these questions was to take the advantage that the *HOPE* conferences would be held at Duke University where several of the 1985 characters had and have their (temporary) home office. The original idea was to have a 'witness seminar' with the 1980s witnesses. A witness seminar is a seminar "where several people associated with a particular set of circumstances or events are invited to meet together to discuss, debate, and even disagree about their reminiscences" (Tansey 1997). Because a witness seminar's dynamics depends on its focus on a "particular set of circumstances or events", we started to doubt whether we actually could have such a seminar. The main question was to investigate what happened in the 1980s with respect to this "boiling" interest in the history of econometrics. A decade is not an event, so it would be hard to know what to focus on. Nevertheless we decided to have a collective-memory stimulating session with the Duke people (currently called the HOPE Group) and the conference participants, and not to call it a 'witness seminar' but a 'chart session' instead:[5] Morgan and Boumans had prepared several charts to stimulate the participant's memories.[6] These 'charts' showed diagrams and graphs of 'relationship among disciplines', 'types of histories', 'number of publications', 'kinships', 'PhD's and trainings', 'institutions/journals', and 'geography'.

The surprising result of this session[7] was that after a warming-up stage participants started to call several events that appeared to be important catalysers for several works on the history of econometrics. This is the reason why we started above with listing the 1985 events. It seems that for this kind of oral history, whether you would want to call it a witness seminar or not, events are essential.

Two events of the 1980s episode turned out to be crucial: The very early (8:00am) Sunday morning session on 'First forays into the history of econom[etr]ics' was very

[5] The whole session was video-recorded and is available **[Paul: ?]**.

[6] Because of the ash clouds spit out by the Iceland volcano Eyjafjallajökull, Morgan and Dupont-Kieffer were not able to attend physically, they participated via the internet.

[7] For writing this introduction we benefitted a lot of the 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.

well attended and brought de Marchi and Gilbert to the idea of having the papers of this session published in the *Oxford Economic Papers* (1989), of which Gilbert had been general editor from 1979 to 1988. As elected vice president of the History of Economics Society, de Marchi had the responsibility to concoct three sessions for the AEA meetings. Coats (1987, 62) 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 (see below) where several of the characters involved in this history publicly took distance from a Lakatosian historiography. The consequences of this step will be discussed below.

Is Econometrics Science?

The reason for the convergence of econometrics, history of economics and philosophy of science in the 1980s is the accusation in the 1970s of the econometrics' inability to meet the high expectations of the 1950s as producer of reliable predictions and policy advises. Particularly, Hendry, in his LSE inaugural lecture 'Econometrics – Alchemy or Science?', used the opportunity to revisit the famous Keynes-Tinbergen debate as a backdrop to re-iterate the scientific possibilities of econometrics. This lecture was published in 1980, the same year in which Sims' 'Macroeconomics and Reality' was published in *Econometrica*, the article in which his VAR approach was introduced.

What started as a critique of John Maynard Keynes (1939) on 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 which 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 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. (Keynes 1940, 156)

Hendry labelled 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, mis-specifying the dynamic reactions and lag lengths, incorrectly pre-filtering the data, invalidly inferring causes from correlations, predicting inaccurately (non-constant 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 mis-specification, 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 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" (Hendry 1980, 402). The ease with which spurious correlations can be produced by a mechanical application of the econometric method 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 that are of particular interest here. Firstly, Hendry in the need of saying something about what science is, referred to and used mainly Popperian literature: Chalmers 1976, Popper 1968, 1969 and Lakatos 1974, but also mentions Kuhn (1962). Secondly, research for this paper was financed in part by a grant (HR6727) from the Social Science Research Council (SSRC) to the Study in the History of Econometric Thought at the LSE. This grant was also used to finance Morgan's PhD research on the history of econometrics which she, in 1979, had just started at the LSE, supervised by Hendry.[8] In an *ET* 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 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 afterwards, 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,

[8] She finished her PhD in 1984. Her 1990 book originated from this thesis.
[9] In 1983, the Social Science Research Council had changed into Economic and Social Research Council.

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 usage of the term alchemy to discuss the scientific nature of econometrics. Another way of denoting this discussion is to see how much econometrics differs from "economic tricks" (or "econo-mystics" 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

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

[11] "Con" must have the meaning of "a swindle in which you cheat at gambling or persuade a person to buy worthless property", like con in "con man": "a man who cheats or tricks someone by means of a confidence trick". So to take the con out of something is taking the tricks away. We thank Morgan for her to understand this phrase. McAleer, Pagan and Volker (1985) has a similar interpretation.

problem of nonexperimental settings (usual the case in economics) compared to 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 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 programme, which means pseudo-science like alchemy.

But also "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 (1964) *Personal Knowledge* as a philosophical backing up for this notion of fact as "truth by consensus".

The problem of using opinions, however, is their "whimsical nature". An inference is not "believable" if it is fragile, if it can be reversed by 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. Beside 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, Eugine 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" (172), Poirier (Hendry, Leamer and Poirier 1990) organized a "dialogue" in *ET* between Hendry, Leamer and himself.

This *Methodenstreit* fits nicely in a longer tradition of debates about the most appropriate empirical scientific method: the already earlier mention Keynes-Tinbergen debate and the measurement-without-theory debate between Tjalling Koopmans (Cowles Commission approach) and RutledgeVining (NBER 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 that can help to prevent economics to become a pseudo-science, 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 its subsequent development of econometrics are closely related to attempts of finding the most appropriate scientific empirical methodology for economics. According to Ragnar Frisch, who first 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)

In his first Editorial of the newly established journal *Econometrica*, Frisch (1933) gave an explanation of the term econometrics:

[12] See Bjerkholt and Qin (2011a) for a more detailed history of the founding of the Econometric Society with the aim of "scientization" of economics.

> 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 promote 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." (Frisch 1933, 1)

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 "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 scepticism 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 economic, would be 'scientific' enough to warrant a prize of this kind on an equal footing with prizes in ‘hard sciences’ like physic 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 a list of potential charter members for the Econometric Society in formation. The list consisted of 28 names from 11 countries (Divisia 1953, Bjerkholt and Qin 2011a).[13]

Another way of delineating a discipline is to appoint forerunners. With respect to this kind of delineation, it is remarkable that from its first issue in 1933 onwards (till the 1960s however) *Econometrica* regularly published articles on denominated forerunners:[14] 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*.

[13] Luigi Amoroso, L. v. Bortkiewicz, A.L. Bowley, T.N. Carver, Gustav Cassel, J.B. Clark, J.M. Clark, C. Colson, F. Divisia, G.C. Evans, Mordecai Ezekiel, Corrado Gini, J.M. Keynes, Hans Mayer, H.L. Moore, Jacques Moret, Bertil Ohlin, Warren M. Persons, A.C. Pigou, Umberto Ricci, Jacques Rueff, H. Schultz, J. Schumpeter, E. Slutsky, T. de Pietri Tonelli, Gustavo del Vecchio, Harald Westergaard and W. Zadadski.

[14] The names with an asterisk have also their photograph printed in *Econometrica*. In issue 2.4 (1934) there is also a “specimen” printed of Walras’ correspondence.

Gerhard Tintner (1953) used this strategy of appointing forerunners as one of the means to "define" econometrics. These were Gregory King, William Petty's Political Arithmetik, Edgeworth, Pareto, and Henry L. Moore's Synthetic Economics. He concluded the essay that the preferred definition of econometrics (still) is "a combination of economics, mathematics and statistics" (31).

From 1939 onwards, 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, I. 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 died more or less out in the 1960s. Note, for example, that *Econometrica* didn't 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 saw earlier, it was *Econometric Theory*, established in 1985, that continued this tradition.

This delineation of econometrics by listing forerunners and founding fathers is a continuation of a longer tradition of producing bibliographies of "mathematico-economic writings". Jevons (1878) 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: firstly, "with the purpose of discovering such forgotten works and memoirs" and secondly, "in the hope that suggestions may be thereby elicited for its extension and correction" (398).[15] The list contained 71 publications. Walras (1878) extended Jevons' list to 98 writings. He published this list with a translation of Jevons' call for extensions and corrections in the *Journal des Économistes*. The lists were sent around for further "completion" (Jevons 1957, xx). This resulted in a "list of mathematico-economic books, memoires, and other published writings" in the second edition of his *Theory of Political Economy* [1879]. This list contained 146 publications. In the preface to this 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 1957, xxi)

According to Jevons, the list could be decomposed into four distinct classes. A first class of publications are 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

[15] Jevons 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" (Jevons 1879, xliii).

quantitative ideas" (xxv). The most important class, however, is the fourth class of 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" (xxxii) was Hermann Heinrich Gossen.[16]

This tradition was subsequently continued by I. Fisher. In his doctor's 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 had selected out of Jevons' 1888 list "those 50 which are either undoubtedly mathematical or are closely associated logically or historically with the mathematical method" (Fisher 1892, 120). To this list a second list was added with 66 publications up to 1892. This bibliography was "superseded" by a 'bibliography of 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 of 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

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

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 C. 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 to promote new theories, tools or 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 and 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 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 the delineation of econometrics and therefore cannot be detached from the written histories in each of these periods.

[17] Phillips was the supervisor of Epstein's (1984) Ph.D. Thesis "Econometric Methodology in Historical Perspective". "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 we have to take 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 done 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) history of general equilibrium analysis sympathized with the developments in econometrics to look for "historical evidence pertaining to the development of the work they evaluate" (140). For that reason he quoted Lakatos's paraphrase of Kant: "Philosophy of science without the history of science is empty; history of science without philosophy is blind" (Lakatos 1971, 91). Influenced by the philosophical views of that time, that is, of Kuhn and Lakatos, methodology should be historically informed, as also de Marchi and Gilbert (1989, 9) 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 imperceptibility 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) 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" (i), but nevertheless he took for granted that Lakatos's model of research programmes was appropriate for the study of mathematical economy.

However, the methodology used for reconstructing a development had, according to Lakatos, to be normative. He was quite explicit about this. Taking its cue from the above paraphrase of Kant's dictum, Lakatos's (1971) 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'. (Lakatos 1971, 91)

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

For Weintraub 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" (Weintraub 2002, 262). History should be done from "a perspective based not on asking of how science 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 programmes "had shown itself to be an appropriate framework for analysing what economics is and is not like" (de Marchi 1991, 1). This neutral 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 but instead de Marchi wrote the introduction and Blaug an 'Afterword'. Where de Marchi (1991) 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” (18), Blaug (1991) 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. (Blaug 1991, 500)

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 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” (Weintraub 2002, 269). For his history of economics as a mathematical science, Weintraub (2002) employs a framework developed by the historian of mathematics Leo Corry. Corry (1989) 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. (Corry 1989, 411).

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; they 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, are: Which of the open problems of the discipline most urgently demands attention? How should we decide between competing theories? What is to be 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 development of the discipline. 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. (Corry 1989, 412)

Of relevance to understand 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), e.g. 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, its 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 due to 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" (Hendry 1986, xi) in the 1980s, 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 (1986) textbook 'Statistical Foundations of Econometric Modelling' (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" (Spanos 1986, 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: Thomas Bayes, Daniel Bernoulli, Carl Charlier, Pafnuty Chebyshev, 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, George Udny Yule. Only four persons survived the earlier *Econometrica* lists: Edgeworth, Moore, Petty and Slutsky. Looking at this list, one can only but 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, beside the earlier works by historians of statistics and probability theories: Cramer (1972), Maistrov (1974), Seal (1967), Stigler (1954), and Stigler (1962), and by the philosopher of science Hacking (1975), also the then more recent histories of econometrics: Hendry and Morgan (1995)[18], Morgan (1982), Morgan (1984). A new methodology needs new histories.

While Spanos and Tintner still rooted econometrics partly in Political Arithmetik of Petty, Hendry and Morgan (1995, 4) cut that root of: "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 only articles of 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) choose 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). (Spanos 2006, 16)

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

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

Conclusions

Corry's distinction between the body and image of a science can so provide an understanding of the history of econometrics and its own continuing struggle with its boundaries. The changes and developments of the econometrician's image of science, that is, inclusive their perception of the views of the philosophers of science at a relevant period, can help to clarify the development of econometrics. In other words, understanding the development of econometrics as a modern science also asks for understanding of the development of the image of science, which includes the history of philosophy of science and the history of economic methodology. Beside that philosophers of science need to be historically informed, historians of science need to be philosophical informed, which mean more precisely here that 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 does not only provide a framework 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 to be 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 post-war period till the 1980s, history of econometrics was the history of Cowles Commission econometrics. This only changed when in the 1970s 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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References

AEA. 1985. Preliminary Announcement of the Program: Ninety-eighth Meeting of the American Economic Association. *The American Economic Review* 75.4:899-917.

Blaug, M. 1991. Afterword. In (de Marchi and Blaug 1991).

Bjerkholt, O. and D. Qin. 2011a. Teaching Economics as a Science: The Yale Lectures of Ragnar Frisch. In (Bjerkholt and Qin 2011b).

—, eds. 2011b. *A Dynamic Approach to Economic Theory*. London and New York: Routledge.

Chalmers, A.F. 1976. *What is this Thing Called Science?* Queensland: University of Queensland Press.

Christ, C.F. 1952. A History of the Cowles Commission 1932-1952. In *Economic Theory and Measurement*. Cowles Commission for Research in Economics.

—. 1967. Econometrics in Economics: Some Achievements and Challenges. *Australian Economic Papers* 6:155-170.

—. 1977. Karl Brunner at the Cowles Commission: A Reminiscence. *Journal of Money, Credit and Banking* 9.1 Part 2:245-246.

—. 1983. The Founding of the Econometric Society and Econometrica. *Econometrica* 51.1:3-6.

—. 1985. Early Progress in Estimating Quantitative Economic Relationships in America. *The American Economic Review* 75.6:39-52.

—. (1994). The Cowles Commission's contributions to econometrics at Chicago, 1939-1955. *Journal of Economic Literature* 32.1:30-59.

Coats, A.W. 1987. Review Essay: History of Political Economy. The AEA and the History of Economics. *History of Economics Society Bulletin* 9:61-66.

Corry, L. 1989. Linearity and Reflexivity in the Growth of Mathematical Knowledge. *Science in Context* 3.2:409-440.

Cournot, A. [1897] 1927. *Researches into the Mathematical Principles of the Theory of Wealth*. New York: Macmillan.

Cramer, H. 1972. On the History of Certain Expansions Used in Mathematical Statistics. *Biometrica* 59:205-7.

de Marchi, N., ed. 1988. *The Popperian Legacy in Economics*. Cambridge: Cambridge University Press.

—. 1988a. Popper and the LSE Economists. In (de Marchi 1988).

—. 1991. Introduction: Rethinking Lakatos. In (de Marchi and Blaug 1991).

de Marchi, N. and Christopher Gilbert, eds. 1989. *History and Methodology of Econometrics*. Oxford: Oxford University Press. Also published as a special issue of *Oxford Economic Papers* 41.1.

de Marchi, N. and M. Blaug, eds. 1991. *Appraising Economic Theories: Studies in the Methodology of Research Programs*. Aldershot: Edward Elgar.

Divisia, F. 1953. La Société d'Économétrie a Attient sa Majorité. *Econometrica* 21.1:1-30.

Epstein, R.J. 1987. *A History of Econometrics*. Amsterdam: North-Holland.

Ericsson, N.R. 2004. The ET Interview: Professor David F. Hendry. *Econometric Theory* 20.4:743-804.

Farebrother, R.W. 2006. Early Explorations in Econometrics. In (Mills and Patterson 2006).

Fisher, I. 1892. Mathematical Investigations in the Theory of Value and Prices. *Transactions of the Connecticut Academy of Arts and Sciences* 9:1-124.

—. [1897] 1927. Bibliography of Mathematical Economics. In (Cournot [1897] 1927).

—. 1898. Cournot and Mathematical Economics. *The Quarterly Journal of Economics* 12.2:119-138.

Frisch, R. 1926. Sur un Problème d'Économique Pure. *Norsk Matematisk Forenings Skrifter* 1.16:1-40.

—. 1933. Editorial. *Econometrica* 1.1:1-4.

—. 1971. On a Problem in Pure Economics. In *Preferences, Utility and Demand: A Minnesota Symposium*, edited by J.S. Chipman, L. Hurwicz, M.K. Richter, and H.F. Sonnenschein. Harcourt Brace Jovanovic. Translated by J.S. Chipman from Frisch (1926).

—. 2011. The Yale Lectures. In (Bjerkholt and Qin 2011b).

Gilbert, C.L. 1986. The Development of British Econometrics 1945-85. *Applied Economics Discussion Paper* 8. Institute of Economics and Statistics, Oxford.

Gilbert, C.L. and D. Qin. 2006. The First Fifty Years of Modern Econometrics. In (Mills and Patterson 2006).

Granger, C.W.J. 2006. Foreword. In (Mills and Patterson 2006).

Haavelmo, T. 1944. The Probability Approach in Econometrics. *Econometrica* 12 (supplement): i-viii, 1-118.

Hacking, I. 1975. *The Emergence of Probability*. Cambridge: Cambridge University Press.

Hands, D.W. 1988. Ad Hocness in Economics and the Popperian Tradition. In (de Marchi 1988).

Hendry, D.F. 1980. Econometrics – Alchemy or Science? *Economica* 47:387-406.

—. 1986. Foreword. In (Spanos 1986).

Hendry, D.F., E.E. Leamer, and D.J. Poirier. 1990. The ET Dialogue: A Conversation on Econometric Methodology. *Econometric Theory* 6:171-261.

Hendry, D.F., and M.S. Morgan, eds. 1995. *The Foundations of Econometric Analysis*. Cambridge: Cambridge University Press.

Hildreth, C. 1986. *The Cowles Commission in Chicago, 1939-1955*. Berlin: Springer-Verlag.

Hoover, K.D. 2006. The Methodology of Econometrics. In (Mills and Patterson 2006).

—. 2010. Minisymposium on the History of Econometrics: Introduction. *HOPE* 42.1:19-20.

Jevons, W.S. 1878. Bibliography of Works on the Mathematical Theory of Political Economy. *Journal of the Statistical Society of London* 41.2:398-401.

—. [1879] 1957. *The Theory of Political Economy*, 5th edition, (1879 2nd edition, 1888 3rd edition). New York: Kelley

Keynes, J.M. 1939. Professor Tinbergen's Method. *The Economic Journal* 49:558-568.

—. 1940. Comment. *The Economic Journal* 50:154-156.

Kuhn, T. 1962. *The Structure of Scientific Revolutions*. Chicago: University Press.

Lakatos, I. 1971. History of Science and its Rational Reconstructions. In *Boston Studies in the Philosophy of Science* 8, edited by R.C. Buck and R.S. Cohen. Dordrecht: Reidel.

—. 1974. Falsification and the Methodology of Scientific Research Programmes. In *Criticism and the Growth of Knowledge*, edited by I. Lakatos and A.E. Musgrave. Cambridge: Cambridge University Press.

Leamer, E.E. 1983. Let's Take the Con out of Econometrics. *American Economic Review* 79:31-43.

Lindbeck, A. 1985. The Prize in Economic Science in Memory of Alfred Nobel. *Journal of Economic Literature* 23.1:37-56.

Lundberg, E. 1992. The Prize for Economic Science in Memory of Albert Nobel. In *Nobel Lectures, Economic Sciences 1969-1980*, edited by A. Lindbeck. Singapore: World Scientific.

Maistrov, L.E. 1974. *Probability Theory: A Historical Sketch*. Academic Press.

McAleer, M., A.R. Pagan, and P.A. Volker. 1985. What Will Take the Con out of Econometrics? *The American Economic Review* 75.3:293-307.

Mills, T.C. and K. Patterson, eds. 2006. *Palgrave Handbook of Econometrics, Volume 1 Econometric Theory*. Basingstoke: Palgrave.

Morgan, M.S. 1982. Errors in Variables or in Equations? A Study in the History of Econometric Thought. Unpublished paper. London School of Economics.

—. 1984. The Probability Revolution: Haavelmo's Contribution to Econometrics. Unpublished paper. London School of Economics.

—. 1987. Statistics without Probability and Haavelmo's Revolution in Econometrics. In *The Probabilistic Revolution. Vol. II: Ideas in the Sciences*, edited by L. Krüger, G. Gigerenzer, and M.S. Morgan. MIT Press.

—. 1990. *The History of Econometric Ideas*. Cambridge: Cambridge University Press.

Pagan, A. 1987. Three Econometric Methodologies: A Critical Appraisal. *Journal of Economic Surveys* 1.1:3-24.

Phillips, P.C.B. 1985. Editorial. *Econometric Theory* 1.1:1-5.

Polanyi, M. 1964. *Personal Knowledge Towards a Post Critical Philosophy*. New York: Harper and Row.

Popper, K.R. 1968. *The Logic of Scientific Discovery*. London: Hutchinson.

—. 1969. *Conjectures and Refutations*. London: Routledge & Kegan Paul.

Qin, D. 1993. *Formation of Econometrics: A Historical Perspective*. Oxford University Press.

Seal, H.L. 1967. The Historical Development of the Gauss Linear Model. *Biometrica* 54:1-24.

Sims, C.A. 1980. Macroeconomics and Reality. *Econometrica* 48.1:1-48.

Spanos, A. 1986. *Statistical Foundations of Econometric Modelling*. Cambridge: Cambridge University Press.

—. 2006. Econometrics in Retrospect and Prospect. In (Mills and Patterson 2006).

Stigler, G. J. 1954. The Early History of Empirical Studies of Consumer Behaviour. *Journal of Political Economy* 62.2:95-113.

—. 1962. Henry L. Moore and Statistical Economics. *Econometrica* 30:1-21.

Tansey, E.M. 1997. What is a Witness Seminar? In *Wellcome Witnesses to Twentieth Century Medicine*, vol. 1, edited by E.M. Tansey, P.P. Catterall, D.A. Christie, S.V. Willhoft, and L.A. Reynolds. London: The Wellcome Trust. Also online publication http://www.ucl.ac.uk/histmed/publications/wellcome_witnesses.

Tinbergen, J. 1939. *Statistical Testing of Business-Cycle Theories. Vol. I A Method and its Application to Investment Activity; Vol. II Business Cycles in the United States of America*. Geneva: League of Nations.

Tintner, G. 1953. The Definition of Econometrics. *Econometrica* 21.1:31-40.

Walras, L. 1878. Bibliographie des Ouvrages Relatifs a l'Application des Mathématiques a l'Économie Politique. *Journal des Économistes* December:470-477.

Weintraub, E.R. 1985. *General Equilibrium Analysis: Studies in appraisal*. Cambridge: Cambridge University Press.

—. 2002. *How Economics Became a Mathematical Science*. Durham and London: Duke University Press.